Happy Birthday? Manipulation and Selection in Unemployment Insurance*

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Abstract

This paper documents strategic delays in the timing of layoffs around an age-at-layoff threshold entitling workers to a four months increase in potential unemployment insurance (UI) benefit duration in Italy. Manipulation is quantitatively important with over 15% of layoffs in the six weeks before workers' fiftieth birthday being delayed. Using bunching techniques, we estimate that the average manipulator collects an additional 2,339 Euros or 38,5% more in UI benefits. This substantial increase in UI benefit receipt is to 81% mechanically due to higher coverage and only 19% the result of moral hazard. Manipulators' implied responsiveness to additional UI coverage is modest and, in particular, not higher than for the average fifty-year-old, mitigating concerns about anticipated moral hazard as the main motive for manipulation. Contrary, we provide evidence that manipulators are highly selected on long-term nonemployment risk. Manipulation is most prevalent among female, white-collar, part-time workers at small firms suggesting adjustment costs and proximity to superiors may play a role in workers' ability to delay their layoff. Together, these findings illustrate how a more comprehensive understanding of the underlying motives for manipulation might influence how it is perceived.

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1 Introduction

The targeting of public policies on the basis of observable individual characteristics is ubiquitous in OECD countries. Governments tax individuals based on their marital status, provide welfare payments which depend on the number of children in the household, or tie disability insurance to particular medical conditions. The theoretical desirability for targeting based on immutable *tags* has long been recognized (Akerlof (1978)). In practice however, policy makers often rely on imperfect tags, which leave room for strategic manipulation and selection into benefit schemes.

How should we view such manipulation? Typically the initial inclination is to regard manipulation solely as opportunistic behavior. Undeserving individuals cheat their way to higher benefits and thrive at the expense of others. While manipulation undeniably increases public spending, this judgment lacks a more comprehensive understanding about the underlying *motivation* for manipulation. Perhaps, individuals who decide to manipulate value the additional benefits tremendously or they manipulate out of desperation. Manipulators might also be relatively less responsive to benefits once they qualify for them. The underlying rationale and subsequent changes in behavior are important to better understand manipulation and might ultimately shape the way the phenomenon is perceived by policy makers and society at large.

While quantifying additional expenditures is relatively straightforward, providing a comprehensive analysis of the motivation for manipulation is considerably more challenging. Our paper makes progress on this important question by studying a context in which differentiated policies and manipulation are widespread, namely unemployment insurance (UI) (see Spinnewijn (2019) for a survey, and Doornik et al. (2018) and Khoury (2018) for recent evidence of manipulation). We study the Italian UI scheme which until 2015 featured a discontinuous jump in potential benefit duration (PBD) depending upon whether the worker was laid off before or after her fiftieth birthday. Individuals separating before age fifty were entitled to eight months of UI, while those separating afterwards were entitled to twelve months of UI.[1]

We start by providing clear graphical evidence of manipulation in the form of systematic delays in the exact timing of layoffs around the age-at-layoff threshold. Using bunching techniques we estimate that over 15% of all layoffs within six weeks before workers' fiftieth birthday are strategically delayed. Over the subsequent nonemployment spell affected workers collect on average 2,239 Euros each, which corresponds to a 38,5% increase over their baseline UI benefit receipt.

While the above numbers are large, it is important to keep in mind that manipulation provides individuals with additional UI *coverage*. Even without a change in subsequent job search effort, manipulators would still collect additional UI benefits due to the extended coverage from month eight to twelve. To see this point, consider two extreme cases . First, suppose manipulators are individuals who would have found a job exactly after eight months, but are now staying nonemployed for four additional months before taking up their next job. In this case manipulation is motivated by an anticipated moral hazard response. The four additional months of benefits are paid only *because* individuals change their job search effort. Contrary, suppose manipulators are unemployed for at least twelve months with or without additional UI coverage. In this case, they would also collect four additional months of UI benefits. However, in the latter case, it is individuals' long-term nonemployment risk that drives selection into manipulation. The additional benefits are paid mechanically due to higher coverage. In reality manipulation is likely motivated by a combination of these forces, but it becomes clear that distinguishing between the two leads to very different positive views about manipulation.

Our survival analysis reveals that approximately 81% of the increase in UI benefit receipt is mechanically due to higher coverage, while the remaining 19% are the result of decreases in job search effort. Put differently, for one euro of mechanical UI transfer the government pays an additional 24 cents due to behavioral responses. Interestingly, we find virtually the same result when studying non-manipulators, i.e. individuals who were laid off just before their fiftieth birthday. This implies that manipulators are not adversely

[1]Similar policies are or have been in place in several OECD countries, such as Germany, Austria among others.

selected on their efficiency cost, which may mitigate concerns about anticipated moral hazard being the prime motive for selection into manipulation. Contrary, we document that manipulators are highly selected on long-term nonemployment risk. Even absent manipulation, manipulators would have exhausted eight months of UI benefits with 16.8 p.p. higher probability than non-manipulators.

To shed light on the underlying collusion behavior by which firms and workers agree to postpone the exact date of layoff, we provide evidence by comparing manipulators and non-manipulators based on observable characteristics. Some degree of manipulation is pervasive among all permanent contract workers in private sector firms, with the exception of large firms with more than fifty employees.[2] Manipulation is relatively more prevalent among female, part-time, white-collar workers at small firms. This suggests that lower adjustment costs, and closer proximity between workers and their supervisors may facilitate manipulation.

Together our results document widespread local manipulation in unemployment insurance and identify long-term nonemployment risk as an important motive to engage in manipulation. These findings highlight the importance of studying the underlying motives for manipulation and might influence how manipulation is perceived. Our analysis also implies that the type of manipulation we consider has only modest effects on economic efficiency, a conclusion that would not hold if anticipated moral hazard were a prime motive for manipulation. This in turn has implications for the design of optimal differentiated UI policies.[3]

Our work relates to several strands of the literature. A large body of work studies the disincentives effect and the effect on post-reemployment outcomes, such as wages, of UI, exploiting similar policy variation, see e.g. Card et al. (2007), Rosolia and Sestito (2012), Schmieder et al. (2012), Landais (2015), Nekoei and Weber (2017), Johnston and Mas (2018) among others. Contrary to our setting, these papers rely on the *absence* of

[2]We find no evidence of manipulation in public sector firms or among temporary contracts.

[3]Strictly speaking, manipulation itself already entails a behavioral response. Under the view that any UI after the eighth month of unemployment to individuals who should have been laid off before their fiftieth birthday, have *zero* social value, one could trivially conclude that all additional benefit payments constitute a loss of social welfare. We do not provide any evidence for or against this view in our work. However, such extreme welfare criteria are unlikely to be relevant in practice.

manipulation to identify the treatment effects of interest, whereas we study the effect of manipulation in a setting where it does occur. Furthermore, while most previous studies of UI focus on the distortion of job search efforts of the unemployed, we examine strategic behavior at the point of layoff. Our work closely relates to two recent contributions by Doornik et al. (2018) and Khoury (2018) who exploit manipulation in UI systems around an eligibility and seniority threshold in Brazil and France, respectively. Doornik et al. (2018) provide evidence of strategic collusion between workers and firms who time layoffs to coincide with workers' eligibility for UI benefits in Brazil. Khoury (2018) exploits a discontinuity in benefit levels for workers laid off for economic reasons and estimates an elasticity of employment spell duration with respect to UI benefits of 0.014. Due to the nature of their policy variation neither of these papers studies the selection patterns we analyze in our work. From a methodological perspective our work is most closely related to the work by Diamond and Persson (2017), who study manipulation in Swedish high-stakes exams. The construction of the manipulation region and of the counterfactual density relies on standard bunching techniques, such as Saez (2010), Chetty et al. (2011) and Kleven and Waseem (2013).

Although the contribution of the paper is empirical, we do relate to the literature on the theoretical desirability of *tagging* (Akerlof (1978)) and ordeals (Nichols and Zeckhauser (1982)). We show that the bargaining over the exact timing of layoffs between workers and firms serves as a screening mechanism for long-term nonemployment risk. In recent work Michelacci and Ruffo (2015) argue for higher UI benefits for young workers by analyzing the canonical Baily (1978)-Chetty (2006) trade-off from a life-cycle perspective. Age as an useful tag for redistribution has also been studied in the context of taxation by e.g. Weinzierl (2011) and Best and Kleven (2013).

The fact that we find substantial manipulation and positive selection on long-term nonemployment risk also speaks to a recent literature studying the role of private information and adverse selection in unemployment insurance, see e.g. Hendren (2017) and Landais et al. (2017). This literature studies the role of private information about job loss risk in shaping the market for UI. Our results indicate that individuals hold information about their expected duration of unemployment at the point of layoff. Understanding to what degree this information is held privately is beyond the scope of this paper.

The remainder of this paper is organized as follows: Section 2 introduces the institutional setting and describes the data; Section 3 describes our quantities of interest and presents our identification strategy; Section 4 explains how we implement the latter in practice; Sections 5 and 6 report the results of our empirical analysis and robustness checks; Section 7 concludes.

2 Institutional Setting and Data

2.1 Institutional setting

This paper studies manipulation in Italy's *Ordinary Unemployment Benefits* (OUB) scheme.[4] The OUB was in effect from the late 1930s until its abolishment and replacement in January 2013.[5] OUB covered all private non-farm and public sector employees who lost their job either due to the termination of their temporary contract or due to an involuntary termination or quit for just cause, such as unpaid wages or harassment. Other types of voluntary quits, the self-employed and the dependent self-employed were not eligible for OUB.[6] To qualify for OUB workers additionally needed to have some labor market attachment. Concretely, workers needed to have started their first job spell at least two years before the date of layoff, and to have worked for at least 52 weeks in the previous two years.

Benefit levels were based on the average monthly wage over the three months preceding the layoff, but the replacement rate was declining over the unemployment spell: 60% of the average wage for the first six months; 50% for the following two months and 40% for any remaining period. OUB did not involve any form of experience rating.

Potential benefit duration (PBD) under OUB was a sole function of age at layoff and amounted to eight months if the layoff preceded the worker's fiftieth birthday and twelve months if it was thereafter. This discontinuous change (notch) in coverage created a

[4] *Indennità di Disoccupazione Ordinaria a Requisiti Normali* in Italian.

[5] OUB was introduced through *Regio Decreto 14th* in April 1939.

[6] For convenience, in the rest of the paper we will use the term "layoff" to indicate all job terminations that are eligible for claiming UI.

strong incentive for workers to delay their date of layoff to fall after their fiftieth birthday.

Two other UI benefit schemes were in place in Italy at the same time of our analysis: Reduced Unemployment Benefits (RUB) and Mobility Indemnity (MI). However, neither one is likely to interfere with our analysis due to different eligibility conditions and less generous benefit coverage. For completeness, we present the two other UI schemes in Appendix A.

2.2 Data

We use confidential administrative data from the Italian Social Security Institute (INPS) on the universe of UI claims in Italy between 2009 and 2012 and combine them with matched employer-employee records covering the universe of working careers in the private sector. Information on UI claims comes from the SIP database, which collects data on all income support measures administered by INPS as a consequence of job separation.[7] For every claim we observe the UI benefit scheme type, its starting date, duration and amount paid. We further observe information related to the job and the firm. This includes details about the type of the contract and a broad occupation category.

The SIP database does not contain the date of re-employment after receiving UI benefits. We therefore retrieve this information from the matched employer-employee database (UNIEMENS) and construct nonemployment durations as the time difference between the layoff date in the SIP and the first re-employment in UNIEMENS.[8] The UNIEMENS provides additional information on workers' careers in the private sector including detailed information on wages and the type of contract. We observe individuals in the UNIEMENS until 2016, which gives us at least four years of observations for all workers and we therefore censor all nonemployment durations at four years.

For our main sample we restrict attention to individuals who lost their job between February 2009 and December 2012, were between 46 and 54 years of age at the time of layoff, and claimed OUB. Unfortunately, our data does not cover the years prior to

[7] *Sistema Informativo Percettori* in Italian.

[8] We restrict the latter to be later than the former, which excludes a few short-term jobs that are compatible with the continuation of UI benefit receipt.

February 2009 and the introduction of a new UI scheme in January 2013 prevents us from including later years. We further restrict attention to individuals who separate from an employer in the private sector after a permanent contract. The motivation for this is twofold. First, we show in Section 5.5 that manipulation is confined to permanent contract private sector work arrangements. Second, the UNIEMENS database does not contain job information for public sector jobs, which means we have no information about the previous work arrangement, nor would we observe re-employment. At this point, one might be worried that we are missing some re-employment events, namely, those into public sector jobs. This in unlikely to affect our results because transitions from private into public sector jobs are rare for workers at such late stage in their careers. We replicated the analysis for a subsample of individuals for whom we have information on the full contribution history and results are qualitatively similar. After the exclusion of a few observations with missing key information we are left with 249,581 separation episodes that lead to UI claims.

Table 1 reports summary statistics for our main sample. The average worker receives UI benefits for about 30 weeks (6.9 months) corresponding to roughly one third of the 90 weeks (21 months) average nonemployment duration. An average of 50% and 39% of workers are still nonemployed after eight and twelve months, respectively, implying substantial exhaustion risk. Our sample of workers is predominately male, on full time contracts, and employed in blue collar jobs. Workers have spent about 27.5 years in the labor market since their first job and almost 6 years in their last firm. In terms of geographic distribution, 46% of workers are laid off in the south or the islands.[9] Workers earned about 70 Euro per day which is equivalent to $70 \times 26 = 1820$ Euro per month if working full time.[10] The separating firm is relatively old (14 years) and large (28.16 employees), but this is driven by a few very large firms. Indeed, more than 60% of workers come from firms with less than 15 employees while only 18% come from firms with more than 50 employees. Because our main sample contains workers in their late forties and early fifties, one might be concerned that transitions into retirement play an important

[9]This area encompasses the following regions: Abruzzo, Basilicata, Calabria, Molise, Puglia, Sardinia and Sicilia.

[10]This information is consistent with the monthly wage reported in our second data source, the SIP database, which reports an average monthly wage of 1,735 euros in the three months preceding the layoff.

role. However, this is not the case with only about 1,500 or 0.6% of workers in our sample claiming retirement benefits before the end of our observation window.[11] We now turn to a description of our objects of interest, which precedes our identification strategy.

3 Conceptual framework

3.1 The moral hazard cost of extended UI coverage

Manipulation provides individuals with additional UI coverage. As in any insurance context the increase in coverage might cause individuals to change their behavior by reducing the incentive to avoid adverse states of the world. This change in behavior, in our context a reduction in job search intensity, constitutes a classical moral hazard response. From an efficiency perspective it is crucial to understand how much of the increase in total insurance payments is driven by changes in behavior and how much is mechanically due to higher coverage. We consider distinguishing between these two effects as one of this paper's main contributions. Quantifying the relative importance of these effects also leads to potentially different positive views about manipulation and the motivation behind it, which in turn might shape how the phenomenon is perceived both by policy makers and society at large.

In the following we formalize the above line of reasoning and introduce the relevant quantities of interest. It is constructive to decompose the increase in insurance payments, i.e. UI benefit receipt, under the twelve and eight months scheme as follows:

$$\begin{aligned}\Delta B = B^{12} - B^{8} &= \int_0^{12} b_t \cdot S_t^{12}\, dt - \int_0^{8} b_t \cdot S_t^{8}\, dt \\ &= \underbrace{\int_0^{12} b_t \cdot (S_t^{12} - S_t^{8})\, dt}_{\text{behavioral response } (\Delta B^{MH})} + \underbrace{\int_8^{12} b_t \cdot S_t^{8}\, dt}_{\text{mechanical effect } (\Delta B^{ME})} \quad , \qquad (1)\end{aligned}$$

where B and S denote the average benefit receipt and the survival probability each

[11] For these workers we define the nonemployment spell as the period between the end of the previous employment and the date at which they claim their pension.

under the twelve and eight months PBD scheme, respectively, and b_t is the benefit amount in period t. The behavioral moral hazard response, ΔB^{MH}, captures the part of the benefit receipt increase that is paid due to the outward shift of the survival curve. The mechanical effect, ΔB^{ME}, corresponds to the remaining increase in benefit receipt that occurs even absent any behavioral response and is uniquely due to the additional UI coverage in months eight to twelve. Figure 1 illustrates decomposition (1) graphically by plotting hypothetical nonemployment survival rates under the eight and twelve months PBD scheme, under the simplifying assumption of a constant benefit level. The total increase in benefit receipt corresponds to the sum of the behavioral/moral hazard effect (dark gray area) and mechanical effect (light gray area).

While the above quantities capture how manipulators respond to extended UI coverage, they are difficult to compare across groups of individuals, such as manipulators and non-manipulators, or to relate to empirical evidence from other studies. In order to facilitate such cross-group comparisons and summarize the extent of moral hazard in one statistic we follow Schmieder and von Wachter (2017) who suggest normalizing the behavioral response by the mechanical effect. Concretely, we take the ratio of the behavioral and mechanical cost to the government:

$$\frac{BC}{MC} = \frac{\Delta B^{MH}}{\Delta B^{ME}}. \tag{2}$$

The BC/MC ratio measures by how many additional euros benefit receipt increases for each euro of mechanical increase. Put differently, if the government transfers one additional euro of mechanical UI transfers it ends up paying a total of $1 + \frac{BC}{MC}$ in additional UI benefits. Two things are worth noticing: first, given that the replacement rate is decreasing over the spell, behavioral changes earlier in the non employment spell generate larger fiscal externalities than comparable behavioral changes later in the spell. Secondly, as long as b_t is a time-varying fraction of (pre-determined) previous earnings, BC/MC ratios are independent of such earnings. The statutory replacement rate is therefore the only piece of information needed.

The analysis thus far focused on additional benefit payments and abstracted from the second source of cost to the government: the loss in tax revenues due to longer nonemployment durations. Contrary to the analysis of UI benefit receipt, longer nonemployment durations do not entail a mechanical effect and are solely the result of a behavioral response. Formally, we have:

$$\Delta N = N^{12} - N^{8} = \int_0^\infty S_t^{12} dt - \int_0^\infty S_t^{8} dt = \underbrace{\int_0^\infty (S_t^{12} - S_t^{8}) dt}_{\text{behavioral response } (\Delta N^{MH})} \tag{3}$$

where, as above, N and S denote the average nonemployment duration and the survival rate each under the twelve and eight months PBD scheme, respectively. Since all of the increase in nonemployment duration constitutes a moral hazard response, we add the resulting cost to the behavioral cost and adjust formula 2 as follows:

$$\frac{BC}{MC}^{\tau} = \frac{\Delta B^{MH} + \tau \cdot \Delta N^{MH}}{\Delta B^{ME}}, \tag{4}$$

where τ is the statutory tax rate that balances the budget of the UI system. We do not take a stance on what the appropriate tax rate in this context is, but follow Schmieder and Von Wachter (2016) and use a 3% UI tax.[12]

3.2 Identification strategy

This section provides a self-contained sketch of our estimation strategy and explains the sources of variation in the data that are used to pin down parameters of interest. The main idea is to exploit the local nature of manipulation by extrapolating outcomes from regions that are unaffected by manipulation to learn about what would have happened in the manipulation region in the absence of it. We first assess the range of the manipulation region with standard bunching techniques. We then fit polynomials to the unmanipulated part of the data and interpolate to construct a counterfactual layoff frequency and recover the number and share of manipulators. Similarly, we construct counterfactuals of outcomes that are not directly manipulated, such as subsequent benefit receipt or nonemployment survival probabilities, to learn whether these outcomes respond to manipulation. Intuitively,

[12] Results are very similar when considering other tax rates.

any unusual change in these outcomes near the cutoff together with an estimate of how many manipulators are causing it, let us recover manipulators' responses. Under minimal additional assumptions, estimates of the response for the average individual combined with the share of individuals who are manipulators let us recover the responses of non-manipulators, whom we use to benchmark manipulators' responses. We also illustrate how we can use part of the procedure just described to study selection into manipulation. Our approach is closely related to that of Diamond and Persson (2017). In the remainder of this section we lay out our approach in more detail.

Quantifying manipulation: Consider a hypothetical manipulated layoff density as in Figure 2a. Absent any manipulation we would expect the frequency of layoffs to be smooth in the neighborhood of the cutoff. Manipulation instead causes a sharp drop in the number or layoffs right before and a spike right after age fifty. As in standard bunching techniques, we recover the counterfactual frequency of layoffs by fitting a polynomial to the unmanipulated parts of the data (on the left and right of the cutoff) and interpolate inwards. We determine the lower bound of the missing region by visual inspection, and then iteratively try different upper bounds of the excess region until we are able to balance the missing and excess mass. The difference between the observed frequency and the fitted counterfactual lets us recover missing and excess shares, as well as the number of manipulators in each bin of the missing and excess regions. This estimation strategy assumes that manipulation takes the form of a pure re-timing of layoffs that would have occurred anyways. One concern is that the increase in PBD at the age threshold leads to extensive margin effects (Jäger et al., 2018). We provide evidence that this is not the case in our setting in Section 6.2.

Effects of manipulation: Equipped with a measure of how many manipulators there are, we then study outcomes which are not directly manipulated but potentially affected by it. Figure 2b illustrates the idea for one of our outcomes of interest: nonemployment survival rates. Manipulation provides workers with additional UI coverage from month eight to twelve. Thus, it is likely that nonemployment survival rates respond to the increase in coverage. Consider a hypothetical statistical relationship between nonemployment survival and age at layoff, as in Figure 2b. In order to estimate how manipulators' survival rate responds, we take the difference between two quantities: manipulators' actual survival probability and manipulators' counterfactual survival probability had they not been able to manipulate. As illustrated in Figure 2b, we obtain these quantities by separately studying the missing and excess region. First, we fit a flexible counterfactual on the right side of the threshold and estimate the difference between the observed and predicted survival rates to assess manipulators' actual survival probability. Intuitively, survival rates in the excess region are higher than predicted by the un-manipulated region to the right only due to manipulation. The extent to which observed and predicted nonemployment survival rates differ, together with an estimate of how many manipulators are causing this difference, let us recover manipulators' actual nonemployment survival probability. We use analogous arguments to back out manipulators' counterfactual nonemployment survival probability on the left side of the threshold. The exact estimation and calculation steps are presented in Section 4.[13]

Effects of UI on the average individual and on non-manipulators: Counterfactual outcomes allow us to recover the statistical relationship between such outcomes and age-at-layoff, absent manipulation. Under some assumptions the jump in counterfactual outcomes at the threshold gives us an estimate of the treatment effect of additional UI coverage for the average individual in the population, akin to a Donut-RD design (Barreca et al., 2011). In Figure 2b this would correspond to the difference between the grey dots on the right and on the left of the threshold. Responses obtained in this way are nothing but a weighted average of responses for manipulators' and non-manipulators, absent manipulation. Assuming that manipulators' response after receiving four extra months of UI does not depend on whether they have *chosen* to manipulate or whether they have been randomly assigned to such treatment, then the Donut-RD coefficient, together with previously estimated manipulators' response and shares allow us to recover the implied response for non-manipulators. We use the latter to benchmark the results for manipulators.

Selection into manipulation: The procedure illustrated in Figure 2b also lets us

[13] All confidence intervals in the paper are obtained by a simple non-parametric bootstrapping: we operationalize this by resampling separation episodes and re-estimating the entire procedure – inclusive of the share of manipulators – 5000 times.

study selection into manipulation by comparing manipulators' counterfactual outcomes to non-manipulators realized outcomes. Figure 2b highlights this comparison and would suggest that even absent manipulation, manipulators would have had a higher nonemployment survival rate than non-manipulators due to the drop in the outcome variable to the left of the cutoff. This is indeed what we show in Section 5. In light of the selection patterns we document, it is worth bearing in mind that we are estimating the effect of manipulation on individuals who endogenously decide to engage in manipulation, akin to a local average treatment effect.

4 Regression Framework

In this section we present the details of how we operationalize our identification strategy in a regression framework.

4.1 Estimating the number of manipulators

In order to quantify the amount of manipulation we follow standard bunching techniques (Saez (2010), Chetty et al. (2011), Kleven and Waseem (2013)). At every age, we estimate a counterfactual layoff frequency by fitting a second order polynomial to the observed frequency, but excluding data from the manipulation region. Concretely, we group all layoffs into two week bins based on the workers' age at layoff and estimate the following specification:

$$c_j = \alpha + \sum_{p=0}^{P} \beta_p \cdot a_j^p + \sum_{k=z_L}^{z_U} \gamma_k \cdot \mathbb{I}[a_j = k] + \nu_j, \quad (5)$$

where c_j denotes the absolute frequency of layoffs in headcounts in bin j, a_j is the mid-point age in bin j, P denotes the order of the polynomial. Coefficients γ_s control flexibly (bin-by-bin) for differences between the observed data and the counterfactual frequency in the manipulation region $[z_L, z_U]$.[14] The whole counterfactual layoff frequency can be recovered from the fitted values of equation (5) omitting the contributions of the

[14]The inclusion of these dummies is equivalent to estimating the polynomial after excluding observations in the corresponding bins.

missing and excess region dummies, i.e. the counterfactual number of individuals in bin j is given by $\hat{c}_j = \sum_{p=0}^{P} \hat{\beta}_p \cdot a_j^p$. Notice that $\hat{\gamma_k} < 0$ if k belongs to the portion of the manipulation region before age fifty, while $\hat{\gamma_k} > 0$ in the portion of the manipulation region after age fifty. This sign difference will be important below when we compute the shares of manipulators.

Crucial to our estimation procedure is a definition of the manipulation region $[z_L, z_U]$. Here we follow the procedure employed in Kleven and Waseem (2013). We first rely on visual inspection to determine z_L. We set this to be six weeks away from the age fifty cutoff (three bins). Subsequently, we try different specifications that increase z_U by little margins (one bin at the time), until the difference between the missing mass and the excess mass is sufficiently small. If the counterfactual density could be recovered without error by a polynomial, we would stop when $\sum_{k=z_U}^{z_L} \hat{\gamma}_k \cdot \mathbb{I}[a_j = k] = 0$. In practice, we stop when this quantity falls below a critical threshold. This procedure leaves us with a manipulation region of six weeks to the left and four weeks to the right of the threshold. The distinction between the portion of the manipulation region to the left and to the right of the threshold will be overly important in the following analysis. For practical purposes we will refer to them as the "missing region" and the "excess region", respectively.

The observed layoff frequency and the estimated counterfactual let us compute the headcount for several groups of individuals in the manipulation region, separately to the left and to the right of the threshold. First, we define the total number of manipulators in the missing region and non-manipulators in the missing region respectively as:

$$N_{\text{mani}}^{\text{missing}} = \sum_{k \in \text{missing}} |\hat{\gamma}_k| \tag{6}$$

$$N_{\text{non-mani}}^{\text{missing}} = \sum_{k \in \text{missing}} c_k. \tag{7}$$

Second, we distinguish between manipulators in the excess region and all other individuals in the excess region who are not manipulators. Formally, we define the total number of individuals in each of these two groups as:

$$N^{\text{excess}}_{\text{mani}} = \sum_{k \in \text{excess}} \hat{\gamma}_k \tag{8}$$

$$N^{\text{excess}}_{\text{w/o mani}} = \sum_{k \in \text{excess}} c_k - \hat{\gamma}_k, \tag{9}$$

respectively. Note that we deliberately reserve the term "non-manipulator" for individuals, who were laid off before their fiftieth birthday and therefore – at least in principle – could have engaged in manipulation but did not. Given the total headcounts, it is straightforward to compute the share of manipulators in the missing and excess region, respectively, as follows:

$$s^{\text{missing}} = \frac{N^{\text{missing}}_{\text{mani}}}{N^{\text{missing}}_{\text{mani}} + N^{\text{missing}}_{\text{non-mani}}} \tag{10}$$

$$s^{\text{excess}} = \frac{N^{\text{excess}}_{\text{mani}}}{N^{\text{excess}}_{\text{mani}} + N^{\text{excess}}_{\text{w/o mani}}}. \tag{11}$$

Similarly, we define the bin-by-bin shares as:

$$s^{\text{missing,}}_k = \frac{|\hat{\gamma}_k|}{|\hat{\gamma}_k| + c_k} \quad \text{for } k \in \text{missing} \tag{12}$$

$$s^{\text{excess,}}_k = \frac{\hat{\gamma}_k}{c_k} \quad \text{for } k \in \text{excess}. \tag{13}$$

Having estimated a measure of the size of manipulation we now turn to studying affected outcomes.

4.2 Estimating the effects of manipulation

In the previous section we constructed the number of manipulators and the share they represent in the missing and excess region. We now move to the estimation of the effect of manipulation on outcomes, such as benefit receipt or nonemployment survival, that are not directly manipulated but might respond to manipulation. As outlined in

Section 3.1, we relate differences in observed and predicted outcomes in the missing and excess region to the missing and excess share of manipulators to recover our outcomes of interest.

As a first step we run the following regression on individual-level data:

$$\begin{aligned} y_i = \ & \alpha + \sum_{p=1}^{P} \beta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i \leq 50] + \sum_{p=0}^{P} \beta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i > 50] \\ & + \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \xi_i, \end{aligned} \tag{14}$$

where y_i the outcome of interest, e.g. weeks of UI benefit receipt or probability of still being nonemployed eight months after the layoff, $\beta_p^{\leq 50}$ and $\beta_p^{>50}$ are coefficients of two P^{th} degree polynomials in age, that are constructed based on information from the left-hand side and right-hand side, respectively. Due to the inclusion of $\mathbb{I}[a_i = k]$ indicator variables, the counterfactual polynomial is estimated as if we were excluding observations from the manipulation region $[z_L, z_U]$. The coefficients δ_k capture the difference in average outcomes between the observed data and the estimated counterfactual in the manipulation region.

Specification (14) allows for a treatment effect of longer PBD on average outcomes, i.e. $\beta_0^{>50}$. We refer to $\beta_0^{>50}$ as the "Donut" RD coefficient. This coefficient captures the average treatment effect of four additional months of PBD for the average individual in the population, as shown in Barreca et al. (2011). We will use it to benchmark our results for the response of manipulators (more on this below). Intuitively, $\beta_0^{>50}$ recovers the difference between the two grey dots in Figure 2b.

The central idea of our estimation strategy is the re-scaling of these estimated differences (δ_k) by the respective share of manipulators responsible for them. Formally let Y denote our outcome of interest, e.g. UI benefit receipt, and $\bar{Y}_l^j$ its average over group l in region j. For each bin k in the missing region, we calculate

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} - \bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\delta_k}{s_k^{\text{missing}}} \tag{15}$$

which gives us the difference in average (counterfactual) outcomes between manipulators and non-manipulators, in bin k in the missing region. Note that the average outcome of non-manipulators in bin k is observable and given by

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k}, \tag{16}$$

which allows us to recover manipulators' counterfactual outcome in bin k as

$$\bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} - \frac{\delta_k}{s_k^{\text{missing}}} \tag{17}$$

and manipulators average counterfactual outcome over the entire missing region as

$$\bar{Y}^{\text{missing}}_{\text{mani}} = \frac{1}{N^{\text{missing}}_{\text{mani}}} \sum_k |\hat{\gamma}_k| \cdot \bar{Y}^{\text{missing}}_{\text{mani},k}, \tag{18}$$

where the $\hat{\gamma}_k$ are estimated in Section 4.1.[15] The logic behind this re-scaling is straightforward: if we found that the absence of 10% of individuals in the missing region resulted in a 100 unit drop starting from a predicted counterfactual of 1000 units, we could infer that the now missing individuals must have had an outcome of $\frac{1000-0.9\times(1000-100)}{0.1} = 1900$ units on average.

Following an analogous argument on the right-hand side of the age cutoff, we first re-scale the regression coefficient for bin k to obtain

$$\bar{Y}^{\text{excess}}_{\text{mani},k} - \bar{Y}^{\text{excess}}_{\text{w/o mani},k} = \frac{\delta_k}{s_k^{\text{excess}}}. \tag{19}$$

Notice that the observable average outcome in bin k in the excess region has to satisfy

[15] Equation 18 is nothing but an application of the law of iterated expectations, as average outcomes in the bins are aggregated using the share of manipulators in each bin.

$$\bar{Y}^{\text{excess}}_{\text{observed},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} = \frac{\gamma_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k} + (c_k - \gamma_k) \cdot \bar{Y}^{\text{excess}}_{\text{w/o mani},k}}{c_k}. \tag{20}$$

Combining the two expressions above and rearranging terms gives us an estimate of manipulators' actual outcome in the form of

$$\bar{Y}^{\text{excess}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} + (1 - s^{\text{excess}}_k) \cdot \frac{\delta_k}{s^{\text{excess}}_k}, \tag{21}$$

for bin k in the excess region. We again calculate manipulators' average actual outcome over the entire excess region by

$$\bar{Y}^{\text{excess}}_{\text{mani}} = \frac{1}{N^{\text{excess}}_{\text{mani}}} \cdot \sum_k \gamma_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k}, \tag{22}$$

which, together with equation (18) lets us define manipulators' response (or treatment effect) as

$$Y^{TE}_{\text{mani}} \equiv \bar{Y}^{\text{excess}}_{\text{mani}} - \bar{Y}^{\text{missing}}_{\text{mani}}. \tag{23}$$

4.3 Recovering the implied response of non-manipulators

Having obtained an estimate of manipulators' response we benchmark these results against the implied response of non-manipulators. As noted above, $\beta_0^{>50}$ provides an estimate of the effect of four additional months of PBD for an average individual who is moved over the threshold exogenously (i.e. without manipulation). Assuming that manipulators would have shown the same response to additional PBD coverage had they been moved over the threshold exogenously we can decompose the response for the average individual as follows:

$$s^{\text{missing}} \cdot Y^{TE}_{\text{mani}} + (1 - s^{\text{missing}}) \cdot Y^{TE}_{\text{non-mani}} = \beta_0^{>50}. \tag{24}$$

A fraction of s^{missing} of the estimated jump in the polynomionmal $\beta_0^{>50}$ is due to the response of manipulators, the remaining $(1 - s^{\text{missing}})$ has to be due to the response of non-manipulators.[16] Rearranging thus gives us an estimate for non-manipulators' response:

$$Y^{TE}_{\text{non-mani}} = \frac{\beta_0^{>50} - s^{\text{missing}} \cdot Y^{TE}_{\text{mani}}}{1 - s^{\text{missing}}}. \tag{25}$$

5 Results

In this section we examine the main findings. We start by presenting graphical evidence of manipulation in the form of strategic delays in the timing of layoffs around the fiftieth birthday threshold. After quantifying the magnitude of manipulation, we estimate the additional increase in UI receipt and actual UI duration that arises from manipulators' strategic behavior. Building on the insight that part of this increase may simply capture the fact that manipulators have higher invariant risk of being long-term non employed, we proceed to decompose such an increase into a mechanical and behavioral component. We do so by combining information on the statutory replacement rates with a survival analysis at the monthly frequency. Despite the fact that financing manipulators' extra coverage is expensive, we highlight that most of the increase in cost is mechanical and would have arisen even absent any subsequent decrease in job search effort. When exploring this result in more detail, we do indeed find that manipulators have substantially higher risk of exhausting the eight month UI scheme, compared to non manipulators. This is consistent with the idea that manipulators may be motivated by their long-term non employment risk, rather than anticipated moral hazard responses. In the final subsection we also

[16] The assumption that manipulators' response would have been the same had they been moved over the threshold exogenously seems plausible in our setting and would, for instance, hold in a fixed cost model of manipulation.

use a similar method to characterize manipulators and non-manipulators on the basis of observable characteristics. Among manipulators we find a higher fraction of female, workers employed in part-time jobs and in small firms. Furthermore manipulation is confined to open-ended contracts in the private sector. We now move to a more thorough description of our results.

5.1 Evidence of manipulation

To provide graphical evidence of manipulation, Figure 3 plots the relative frequency of layoffs against workers' age at layoff. Figure 3b covers the entire age range from 26 to 64 years of age, while Figure 3a zooms into a narrower, four year window around the age-fifty threshold.[17] Both figures show a clear drop in the frequency of layoffs just before, and a pronounced spike after, the age-fifty threshold.

Following our estimation strategy outlined in Section 4.1, we find the manipulation region to consist of all bins from six weeks before (missing region), up to four weeks after the threshold (excess region). Table 2 reports our estimates for the respective headcounts for the four groups of interest: manipulators in the missing region, non-manipulators in the missing region, manipulators in the excess region and all individuals in the excess region who are not manipulators, as well as share estimates for the missing and excess region (see equations (6) - (11) above). We estimate that a total of 571 layoffs are strategically delayed corresponding to 15.8% of layoffs in the missing region. The counterfactual relationship appears almost perfectly linear and is robust to the choice of the order of the polynomial. The estimated number of manipulators in the excess region, 609, deviates slightly from that in the missing region due to measurement error and corresponds to approximately 20.3% of layoffs in the excess region.

We consider the evidence presented until here as this papers' first contribution. It documents that incentives generated by the UI system can influence the timing dimension of layoffs and thereby the length of an employment spell. Complementing previous work on

[17] By plotting the layoff frequency over the entire age range in Figure 3b, we already rule out that manipulation is caused by other mechanisms like (round-) birthday effects. All our estimates for the counterfactual density and counterfactual outcomes are based on the narrower (46-54) window. Section 6 presents additional robustness checks.

the extensive margin response of job separations, we focus on the timing dimension of the layoff decision.[18] Having established sizable manipulation, we now turn to the estimation of its effect on manipulators' benefit receipt.

5.2 Effects of manipulation: UI benefit receipt and duration

Successful manipulation provides workers with four more months of potential UI coverage, after the eighth month of nonemployment. In this section we study the effects of such longer coverage on manipulators' actual benefit receipt and benefit duration. We begin by plotting these outcomes against workers' age at layoff in Figure 4a and 4b, respectively. The observed pattern in the raw data fits with the model of manipulation we laid out in Section 3 and constitutes clear non-parametric evidence that UI receipt and actual duration respond to manipulation. As explained in Section 4.2 our procedure combines abnormal changes in outcomes near the threshold with the share of manipulators causing them. This allows us to retrieve manipulators' as well as non-manipulators' responses.

We report all relevant estimates with associated 95% confidence intervals in Tables 3 and 4. Our estimates indicate that manipulators would have collected 5814.2 Euros, and spent 27.8 weeks on benefits, had they not manipulated (column (1)). When manipulation lengthens individual UI coverage, these figures jump up to 8053.6 Euros and 41.8 weeks, generating an increase in fiscal outlays of 2239 Euros per manipulator. In order to benchmark this number, we compute the same increase for non-manipulators, which we report in column (6). We find that this corresponds to 1637 Euros only. From an accounting perspective our results indicate that overall it would be cheaper to finance longer coverage for non-manipulators rather than for manipulators. However, the size of the efficiency cost of financing manipulators' crucially depends on subsequent behavioral changes that purposefully reduce the probability of finding a new job. We therefore ask a more interesting question: what fraction of these additional UI expenditures is actually due to behavioral responses and how much is instead mechanically due to longer coverage? In the next subsection we make use of a survival analysis to shed light on this question.

[18] Jäger et al. (2018) and Doornik et al. (2018) both study the extensive margin response of job separations to UI benefits.

5.3 Distinguishing behavioral responses from mechanical effects

In this section we make use of the methodology presented in Section 3 to decompose UI receipt and actual duration response into a mechanical and a behavioral component, so as to shed light on the effective moral hazard cost of manipulation. Nonemployment survival probabilities, together with information on statutory replacement rates, are the crucial pieces of information needed to measure the relative size of these two sources of cost. Intuitively, it is important to understand *when* manipulators respond, in order to distinguish between relatively expensive moral hazard responses during months of benefit receipt from those that happen after benefit exhaustion.

Similarly to what we did for UI receipt and duration, in Figure 5 we report the observed relationship between survival in nonemployment and age at layoff for a selected set of months after separation. Qualitatively, we observe bigger jumps around the thresholds precisely during the months with extra coverage. Within the manipulation region we also see outcome changes that are abnormal, compared to what could be predicted by the data outside of it. Similarly to before, we combine these changes with the share of manipulators causing them to trace monthly survival curves for both manipulators and non-manipulators.

Figure 6a shows the estimated nonemployment survival curve of manipulators under the eight and twelve months PBD scheme. Figure 6b reports the difference between the two curves at any point, with associated bootstrapped 95% confidence bands. The difference between the two curves reveals the effect of longer PBD along manipulators' survival curve. It shows virtually no difference in survival probabilities in the first six to seven months, after which the two curves start diverging. The shift in manipulators' survival curve is substantial with their nonemployment probability after twelve months increasing by 16.7 p.p. due to the more generous scheme. Perhaps unsurprisingly, the behavioral response is concentrated in the months eight to twelve and coincides with the time of extended UI coverage. However, as pointed out, there is very little evidence of moral hazard in the first eight months of nonemployment. The difference between the two curves then decreases again after month twelve, consistent with the idea that these individuals increase their job search efforts again once the benefits expire. We replicate the same type of analysis for non-manipulators and report it in Figure 7a and 7b. The qualitative picture is similar. Also in this case we see very limited anticipatory responses of longer coverage during the months zero to eight, and a pronounced divergence after month eight, indicative of a moral hazard response.[19]

Absolute shifts alone are not appropriate to represent efficiency costs because they ignore the fact that not all individuals have the same probability of still being nonemployed during the periods of longer coverage. To solve this issue we follow Schmieder et al. (2012) and compute BC/MC ratios, as detailed in Section 3.1. We compute these ratios by numerically integrating the survival curves over the relevant ranges, and appropriately weighting by statutory survival rates. We perform integration by using the midpoint rule and impose that the behavioral cost has to be weakly positive at any given point.[20]

We report BC/MC ratios in Table 5. In column (1) we report the simple BC/MC ratio, as in equation 2. Manipulators' estimate of 0.24 implies that for one additional Euro used to provide longer UI coverage in the months eight to twelve the government would have to spend an additional 24 cents due to behavioral responses that occurs in months zero to twelve. The corresponding estimate for non-manipulators' is remarkably similar, implying that manipulators are not adversely selected on the basis of their *effective* moral hazard cost. In column (2) we enrich our analysis by following equation 4 by also considering the cost of lost tax revenue during the whole nonemployment spell. These numbers are higher because the government is marginally losing money out of the UI system due to long nonemployment durations. In selecting the tax rate we follow Schmieder and Von Wachter (2016) and use a 3% tax rate. Also in this case, numbers across the two groups are virtually identical. Together these results reinforce the idea that manipulators' responses in terms of decreased job search effort do not generate efficiency costs that are higher than those

[19]Due to the fact that non-manipulators' actual survival curve under the eight-month scheme is *observed* and not estimated, confidence bands are much narrower.

[20]In the first few months the point estimates indicate that the survival probability in nonemployment slightly *decreases* as a consequence of higher PBD. As these negative contributions to the overall integral would lead us to underestimate BC/MC ratios for manipulators, we want to stay as conservative as possible by making sure that our results do not depend on these unusual patterns in the data at the beginning of the spell. Results are qualitatively unaltered whenever we do not impose this non-negativity constraint.

of the average individuals in the same age range. This may also mitigate concerns that selection on anticipated moral hazard is the prime motive behind manipulation. As a final note, it is worth pointing out that our BC/MC ratios for manipulators, as well as for the non-manipulators, are in the lower range of estimates in the previous literature (see Schmieder and Von Wachter (2016) for an overview).

5.4 Selection on long-term nonemployment risk

While manipulators are not adversely selected on their effective moral hazard cost, it is still true that financing their UI coverage is more expensive from a budgeting perspective. As a matter of fact in Table 3 we previously saw that providing four additional months of UI coverage increased the average UI benefit receipt by 2239 Euros for manipulators and only by 1637 Euros for non-manipulators. This seems to suggest that manipulators are instead adversely selected on their long-term nonemployment risk. In this subsection we corroborate this hypothesis by showing that manipulators' have higher UI exhaustion rates even when they have the same PBD as non-manipulators. Figure 8a illustrates this point by plotting survival rates for manipulators and non-manipulators under the eight month scheme. Comparing manipulators and non-manipulators when they face the same incentives isolates permanent differences in risk. The figure illustrates that even with shorter PBD, the probability of exhausting such benefits without finding a new job is almost 20 p.p. higher for manipulators. The large exhaustion risk is what makes most of the increases in benefit receipt and duration mechanical and thus lowers the BC/MC ratio, *ceteris paribus.*

5.5 Characterizing manipulators

Until now we have quantified manipulation and studied its consequences, but we have abstracted from understanding how it occurs. In this section we present a characterization of the manipulators along observable characteristics, in order to provide some suggestive evidence on the economic mechanisms that generate it. In Figure 9 we start by visually inspecting the age distribution of layoffs for different types of contracts (permanent and temporary) and sectors (private and public). Workers in the public sector, either with permanent or temporary contracts, show little ability or interest to delay their layoff and the density of layoff does not exhibit any discontinuous pattern for either of these groups. The density for workers laid off from permanent contracts in the public sector also shows substantial variance, due to a smaller number of individuals. Once we move to the private sector, we can observe that workers on permanent contracts are able to manipulate their date of layoff, while the same is not true for workers on temporary contracts. This is consistent with temporary workers having little ability choose a start date for their contracts that positions them on the right-hand-side of the threshold, once laid off. It is also consistent with lower bargaining power with the employer, due to e.g. shorter tenure.[21]

In what follows we focus on the subset of workers who claimed UI after losing a permanent job in the private sector, which was also our sample of interest in the main analysis. To provide a more precise assessment, we make use of a procedure developed in (Diamond and Persson, 2017, Section 6.2). The idea is similar in spirit to the rest of our analysis. Let us say that we want to investigate whether manipulators are more likely to have a given characteristic, e.g. being female. If there are disproportionately more (less) women in the excess (missing) region compared to what a fitted counterfactual would predict, then manipulators are more likely to be female. Results are in Table 7. Columns (1) and (2) report the estimated mean characteristic for manipulators and non-manipulators, respectively. The difference of the two is reported in column (3), together with bootstrapped 95% confidence intervals. In column (4) we report the estimated mean for yet another group, i.e. all individuals whose unmanipulated age-at-layoff falls in the missing region. We find that manipulators are 18 p.p. more likely than non-manipulators to be female, 17 p.p. more likely to be employed in white collar jobs and 7 p.p. less likely to have full-time contracts. We observe that their wages are 6% lower, although estimates are relatively imprecise. No significant difference emerges in terms of tenure and geographic location. We notice that firm size is an important element: manipulators come from firms that are about 40% smaller with respect to firm of non-manipulators. We only see minor and statistically insignificant differences in terms of age of the firm. We

[21] Although the McCrary test identifies the presence a discontinuity also in this case, this is substantially smaller than the one observed for workers coming from permanent contracts.

can only speculate as to the reasons behind the firm-size differential in manipulation: the effect may work through personal relationships, workers' (credible) threat to sue the firm for unjust dismissal, or direct bribes paid with part of the extra UI. Our data do not allow us to disentangle these possibilities and leave this question to future research. Overall, these findings suggest that adjustment costs, bargaining power and proximity to managers play a role in workers' ability to engage in manipulation.

6 Robustness

6.1 Placebo tests

One key identifying assumption of our empirical strategy is that the bunching patterns we observe in the data just reflect the strong incentives given by higher PBD and are not linked to other institutional features of the labor market discretely changing at age fifty. In this subsection we test this assumption by looking at two other UI schemes that were introduced *after* 2012 and that did not feature sharp changes in generosity at age fifty. Intuitively we would expect to see no missing and excess mass to the left and to the right of the threshold, respectively. In Figure 10 we report the corresponding layoff densities. In order to be consistent with our original sample definition, we focus on workers who were employed on permanent contracts in the private sector. In both cases we fail to detect any graphical evidence of manipulation and see that the density evolves smoothly around the threshold. This suggests that the discontinuous shape of the density in our main sample is directly related to the PBD extension that characterized the OUB scheme.

6.2 Extensive margin responses

Manipulation induces a re-timing of existing layoffs from the weeks immediately preceding workers' fiftieth birthday to right after, generating a missing and an excess mass compared to the counterfactual frequency. One of the identifying assumptions of the methods used in this paper is that manipulation is the only reason why we observe these changes in the vicinity of the threshold. However, if longer PBD increases workers' outside option out of employment, it is possible that the number of layoffs discontinuously increases after age fifty, even absent any manipulation. We call this increase an "extensive margin response". This is worrisome for two reasons: first we would be mismeasuring the upper bound of the manipulation region (z_U), and second, if the extra layoffs are *selected*, we would be altering the composition of jobs in the manipulation region for reasons other than manipulation, introducing a bias.

The nature of the selection is not straightforward. As discussed in Jäger et al. (2018), in a standard Coesean bargaining framework, positive changes in workers' outside options induce separations for those (marginal) jobs that have relatively low joint (firm + worker) surplus. These could be e.g. the least productive jobs employing the least skilled workers. In other (non-Coasean) settings, changes in outside options induce a higher number of separations among jobs with low workers' surplus. These could be the workers who value leisure relatively more or are employed in physically strenuous occupations, and not necessarily the least productive ones. In both cases this *extensive margin* response on the number of separations would alter the composition of jobs in a way that is potentially correlated with outcomes of interest. These concerns are not purely theoretical: Feldstein (1976), Feldstein (1978) and Topel (1983) provided a theoretical framework and some preliminary evidence on how more generous benefits may generate additional layoffs. Jäger et al. (2018) also finds an effect of extended PBD on job separation rates in Austria. They find that the job matches of the workers who do not separate are not more resilient in subsequent years, casting doubts on the Coasean framework. Recent work by Albanese et al. (2019) documented an increase in the probability of separation for Italian workers who become eligible to the OUB scheme for the first time. In what follows, we show these concerns find little empirical support in our setting.

In testing for the importance of extensive margin responses, we consider two different scenarios. In the first scenario, all jobs can be hit by random shocks that decrease their value, and whose distribution does not feature any point of discontinuity. Since *all* jobs to the right of the threshold are less resilient due to lower worker surplus, we would expect to see an upward shift in the whole density of layoffs. In the second scenario, there are no shocks, but a limited set of jobs with small and positive surplus will mature into negative surplus as workers' age cross the age-fifty threshold, due to increased outside option of the worker. In this case additional layoffs might be concentrated right after workers' fiftieth birthday, with the following age bins being unaffected. We analyse the former case by checking whether either the layoff density or workers' observable characteristics exhibit a jump at the threshold, even after accounting for the presence of manipulation. We then consider the latter case by a direct comparison of the excess and missing masses under different definitions. Finally, we discuss sample-related and institutional reasons which cast doubt on the presence of extensive margin effects in our setting.

6.3 Testing for shifts in the density

Let us now turn to the first check: we look at whether the layoff density exhibits an upward shift at age fifty even after flexibly controlling for the presence of manipulation. We do so by running a classic RD model on the layoff density, but excluding observations belonging to the manipulation region.The estimating equation reads as follows:

$$d_j = \alpha + \lambda \cdot a_j + \gamma \cdot \mathbb{I}[a_j \geq 50] + \delta \cdot \mathbb{I}[a_j \geq 50] \cdot a_j + \nu_j, \tag{26}$$

where d_j is the density of layoffs in bin j, a_j is the mid-point age in the bin and ν_j is an error term. Our coefficient of interest is γ, which represents the possible discontinuity in the density at the age fifty threshold. Ideally the coefficient should be close to zero, indicating no extensive margin responses.[22] We report the results in Table 8. In column (1) we run equation 26 on the whole sample, that is also including the manipulation region. As expected we detect a significant jump at the threshold, which is consistent with excess layoffs after age fifty. In column (2) we run the same model but exclude observations in the manipulation region. We find that the discontinuity becomes much less relevant quantitatively, and statistically not different from zero. In column (3) we repeat the same exercise but with an alternative and extended definition of the manipulation region. Contrary to the traditional Kleven and Waseem (2013) method here we use as

[22] Note that in this case we have used a linear specification instead of a quadratic, as higher order polynomial would provide too much weight on extreme observations and might lead to a poorer overall fit. The Akaike Information Criterion and Bayesian Information Criterion both suggest that the linear and quadratic specification are roughly equivalent, although the linear one is slightly preferred. Other measures of goodness of fit such as the R^2 also show substantial equivalence of the two models.

missing (excess) region the one characterized by the longest sequence of negative (positive) coefficients starting from the threshold. The resulting missing region is substantially larger and it goes up to 4 months before the cutoff (9 bins) while the excess region is remarkably similar and it adds only a couple of bins to the one used in our baseline estimates.[23] This involves a simple assumption of continuity and increasing cost of manipulation in the distance from the threshold, and delivers convex missing and excess regions. Also in this case the estimate for γ is quantitatively negligible.

6.4 Testing for discontinuities in observable characteristics

As a second check, we assess whether workers separating on either side of the cutoff differ systematically, above and beyond what can be explained by manipulation. We therefore run two regression models, a naive one that does not control for manipulation (and serves as a benchmark) and one that explicitly controls for it. The naive model, ran on the full sample reads:

$$x_i = \alpha + \sum_{p=1}^{P} \lambda_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i < 50] + \sum_{p=0}^{P} \lambda_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i \geq 50] + \xi_i \tag{27}$$

which is a standard RD model where $\lambda_0^{>50}$ is the jump at the threshold. The other model adds bin-by-bin indicator variables for the manipulation region and is as follows:

$$\begin{aligned} x_i = \kappa &+ \sum_{p=1}^{P} \theta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i < 50] + \sum_{p=0}^{P} \theta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i \geq 50] \\ &+ \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \nu_i, \end{aligned} \tag{28}$$

If manipulators are selected on observables, we would expect $\lambda_0^{>50}$ to be different from zero, a point also raised in section 5.5. If however manipulation is the only reason

[23] In order to reduce the influence of very small coefficients, we ignore the sign of a coefficient if its absolute value is smaller or equal to 1/1000 of the average density across all bins. This is roughly equal to a deviation of three workers from the predicted counterfactual.

why selection arises, we would expect $\theta_0^{>50}$ to be equal to zero. We reports tests on these two coefficients in Table 6 for a large set of observable characteristics. Columns (1) to (3) report estimates from model 27. Observable characteristics are indeed different on the two sides of the threshold, because of manipulation, but potentially also because of extensive margin responses. Columns (3)-(5) rule this last channel out. The fact that, after accounting for manipulation, the distribution of observable characteristics is continuous at the threshold makes the presence of additional layoffs related to changes workers' outside options. This is very reassuring for the validity of our design, as it seems that changes in PBD do not induce extensive margin changes in the number of layoffs.

6.5 Testing for the presence of extra excess mass

So far, these analyses suggest negligible effects of unemployment benefits on layoffs. We now move to testing the second type of extensive margin response, that is the one that emerges only near the threshold. The basic idea behind the test we propose now is to see if we can detect additional excess mass to the right of the cutoff, above and beyond what would be predicted by the missing mass. In absence of extensive margin responses, excess and missing mass should be equal, so any difference in favour of the excess mass would make us think PBD is inducing extra layoffs right after the threshold. In order to implement our test, we estimate the following regression model on the layoff density:

$$c_j = \alpha + \beta a_j + \sum_{k=A}^{50^-} \tilde{\gamma}_k \cdot \mathbb{I}[a_j = k] + \sum_{k=50^+}^{B} \tilde{\delta}_k \cdot \mathbb{I}[a_j = k] + \zeta_j \tag{29}$$

Where the set of $\tilde{\gamma}_k$ and $\tilde{\delta}_k$ coefficients are enough to measure the size of the manipulation region. Same as in 6.3 we consider an extended manipulation region that includes bins from 18 weeks before workers' fiftieth birthday up to 8 weeks afterwards. The lower and upper bounds are denoted by $A < z_L$ and $B > z_U$, respectively. After having estimated the previous equation we rescale the difference between excess and missing mass by the excess mass itself. This yields the share of the excess mass that can be explained by extensive margin responses. Such share amounts to only 1.3%, which is very reassuring about the validity of our identification strategy.

6.6 Why are extensive margin responses so small?

In this subsection we discuss why it might be plausible that our extensive margin responses are smaller compared to those found in the studies of Jäger et al. (2018) and Albanese et al. (2019). Broadly speaking, the reasons have to do with the fact that benefit changes at the threshold are smaller compared to those in these studies, and also that some institutional features in our setting limit the scope for big behavioral responses at the extensive margin.

More specifically, Jäger et al. (2018) study an Austrian policy change that in 1988 increased PBD from 30 to 209 weeks, a seven-fold increase. This is much larger than in our case, where PBD increased just by 50%. Differences in our estimation strategies and setting make it difficult to map our results and theirs directly. Here we just perform a back of the envelope calculation that assumes linearity in the effects of longer PBD. Jäger et al. (2018) find an increase of separations by 11 percentage points over a baseline of 36%, implying a β of $\frac{11}{209-30} = 0.061$. Since in Italy the absolute change in the number of weeks of PBD has been $4 \times 4.33 = 17.32$, the implied increase in separation in Italy would have been $17.32 \times 0.061 = 1.06$ percentage points.[24] This would represent a very small change in our overall density and it unlikely to generate substantial bias.

Secondly, it is worth stressing that two features of our institutional setting make it difficult to extend results from Jäger et al. (2018) to our framework. A relevant aspect that should be taken into account is that the higher separation rate in Austria is partly driven by quits rather than layoff. Indeed, in the Austrian system workers who quit their job are eligible to receive unemployment benefits while this is not possible in Italian legislation, unless under particular circumstances. In addition, the longer unemployment benefits under the Austrian REBP scheme could be used by workers to bridge towards retirement after turning 55. This made unemployment more attractive to workers. The Italian pension system was, in the period considered, much less generous. Even with seniority pensions, workers needed to be close to sixty year old to retire. Both these differences

[24] Alternatively one could assume that proportional (and not absolute) changes are constant and do similar calculations by deriving an elasticity. Using the numbers above we find such elasticity to be $\frac{11/36}{179/30} = 0.051$. This would imply that the predicted percentage change in the Italian setting is $0.051 \times 50\% = 2.55\%$.

make it less likely that the extension of potential benefit duration leads to excess layoffs.

We now turn to comparing our work to Albanese et al. (2019), who find a sizable increase in the separation rate for workers who become eligible to the OUB scheme in Italy for the first time. We present two reasons why we think these responses are unlikely to be present in our sample, although we are studying the same UI scheme. First of all it is worth stressing that the workers in our sample have already experienced a jump their PBD in the past, precisely when they met their eligibility criteria. It follows that the observed matches, which end in a separation in our dataset, have already survived a large increase in their outside option, so they should be less sensitive to further increases in it. Secondly, Albanese et al. (2019) exploit variation in UI eligibility rules, which allow workers with no UI to have access to some. We instead study variation at the *intensive* margin, since our workers obtain four extra months of PBD. Whether these two responses should be the same has not been explored so far but it can be argued that the former should be larger than the latter. To our knowledge there is no explicit analysis of this aspect in existing studies and we leave it to future research. All in all, all these considerations might explain the discrepancy between our results and the higher probability of separation identified by Albanese et al. (2019).

7 Concluding Remarks

This paper studies manipulation in the context of unemployment insurance. We document substantial manipulation in forms of strategic delays in the timing of layoffs around an age-at- layoff threshold entitling workers to a four months increase in potential UI benefit duration in Italy. Using bunching techniques we study the selection pattern and moral hazard response of manipulators. We argue that changes in subsequent job search intensities are informative about the underlying motives for manipulation and we identify long-term nonemployment risk as an important factor for selecting into manipulation. Manipulators are only modestly responsive to the increase in UI coverage mitigating concerns about anticipated moral hazard.

All in all, we illustrate how a more comprehensive understanding of the underlying motivation for manipulation might shape how the phenomenon is perceived. Furthermore, our results highlight the importance to take layoff responses into account when designing differentiated UI schemes and point to potential limits of governments' ability to target UI benefits.

Although a full welfare assessment is beyond the scope of this paper we deem it a fruitful avenue for future research. So is the more general question of the desirability of differentiated UI policies.

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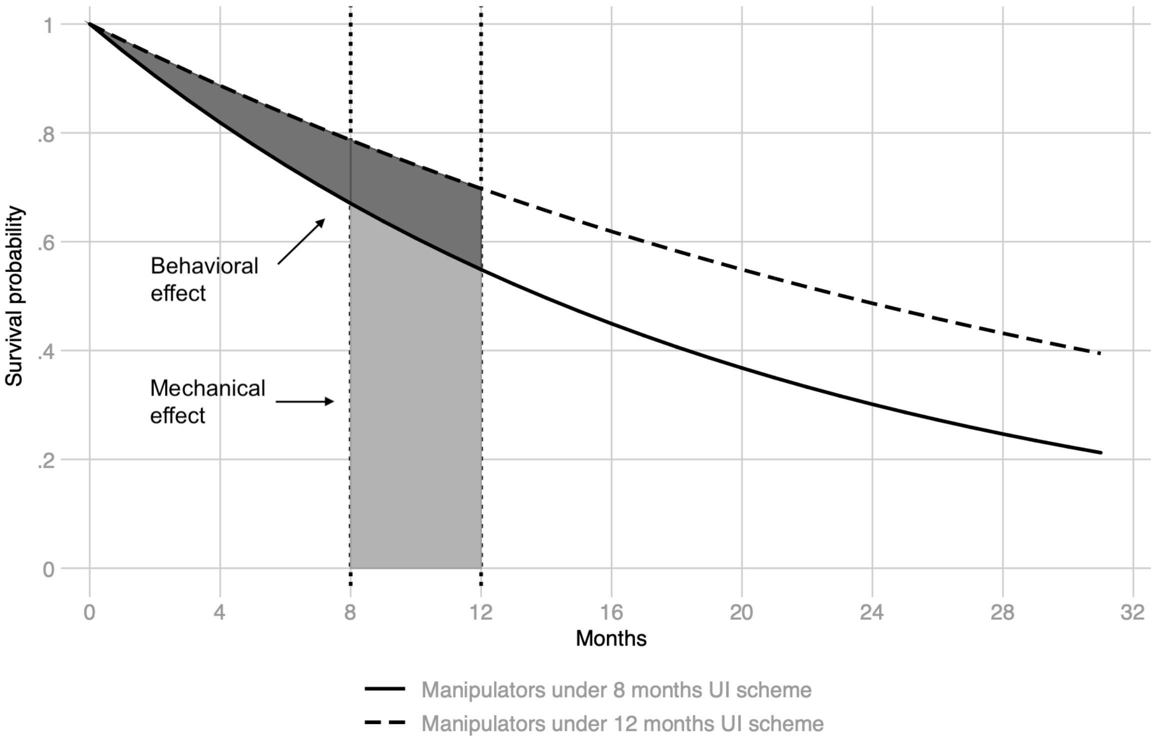
Figures

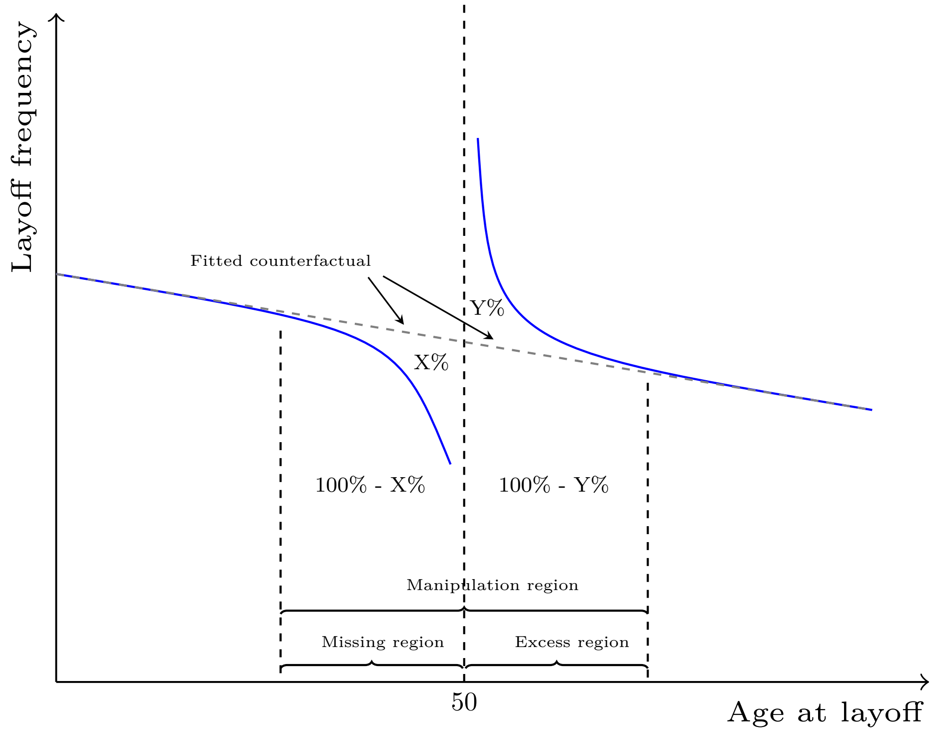
Figure 1: The moral hazard cost of extended UI coverage



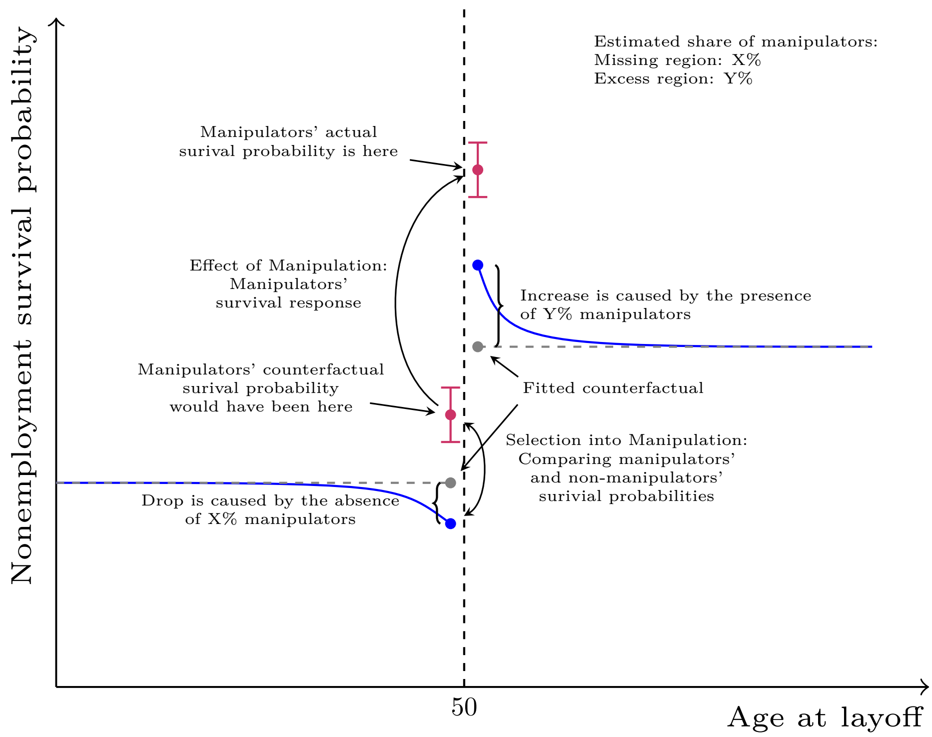
Note: The figure displays manipulators' survival curves (S_t) in nonemployment under two alternative scenarios: manipulators' potential benefit duration (PBD) is eight months (solid line), and manipulators' PBD is twelve months (dashed line). The dashed line is above the solid line under the assumption that higher PDB lowers the hazard rate of exit from nonemployment. The curves are simulated as negative exponentials with a constant hazard rate of 5% and 3%, respectively. The increase in the fiscal cost (shaded areas) is due to two components: (1) the mechanical cost (light-shaded area) due to extra UI outlays covering months eight to twelve, absent any behavioral change; (2) behavioral component (dark-shaded area) due to a shift in the survival curve in months zero to twelve, induced by the change in PBD. The effective moral hazard cost is given by the ratio of (2) and (1).

Figure 2: Illustration of identification strategy

(a) Quantifying manipulation



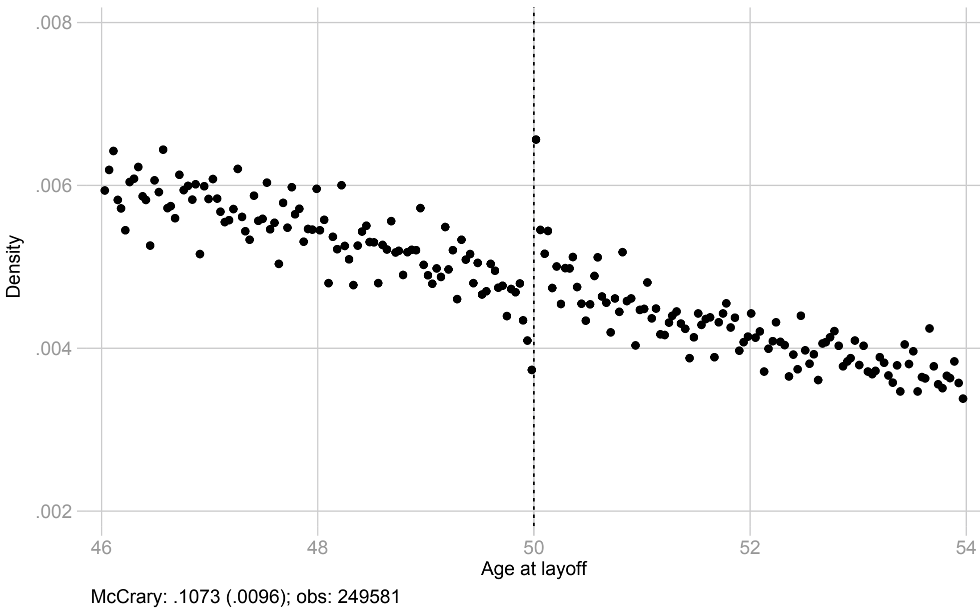
(b) Effect of and selection into manipulation



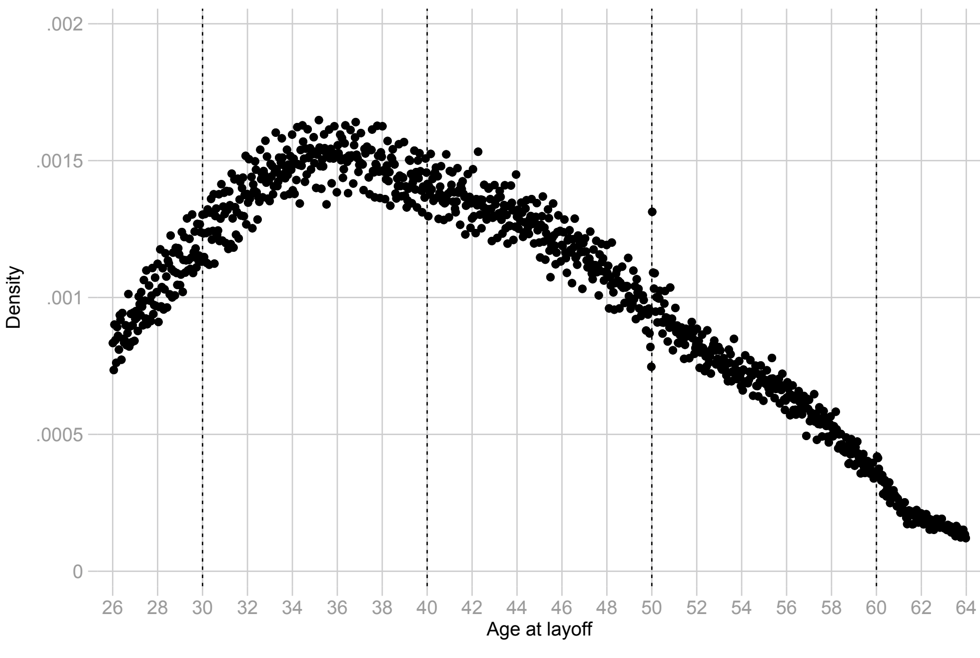
Note: The figure visualizes our identification strategy. Panel (a) illustrates how we estimate the number and respective share of manipulators in both the missing and excess region. Panel (b) constructs manipulators' survival response and illustrates the relevant comparison when studying selection into manipulation. Section 4 lays out how we estimate the fitted counterfactuals in practice.

Figure 3: Layoff frequency for permanent contract private sector workers

(a) Age-at-layoff between 46 and 54 years



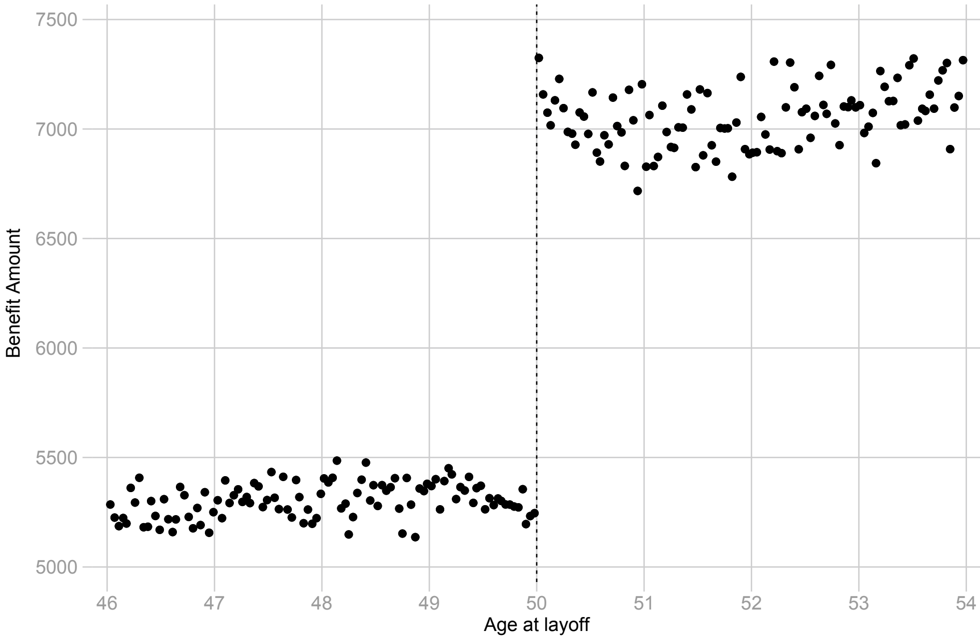
(b) Age-at-layoff between 26 and 64 years



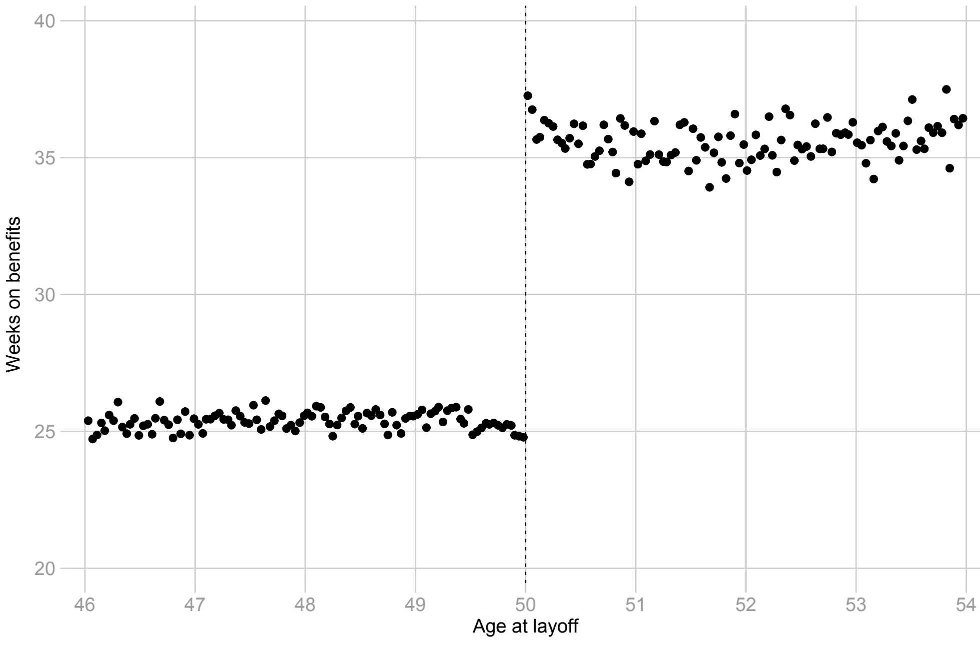
Note: The figure shows the density of layoffs in the private sector, for individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period February 2009 till December 2012. Panel (a) plots the density for the age range from 46 to 54 years, while Panel (b) does so for the entire age range from 26 to 64 years of age. In both panels each dot represents a two-week bin. The underlying data in Panel (a) consists of 249,581 layoffs.

Figure 4: Benefit receipt and duration

(a) average UI receipt in euros



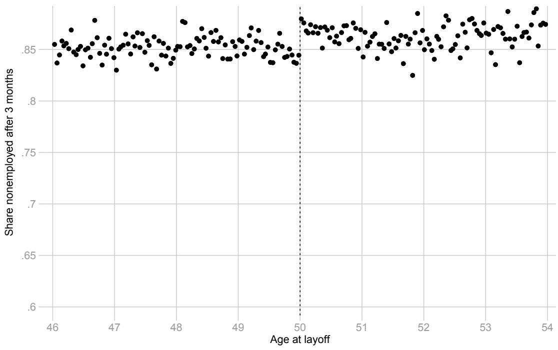
(b) average UI benefit duration in weeks



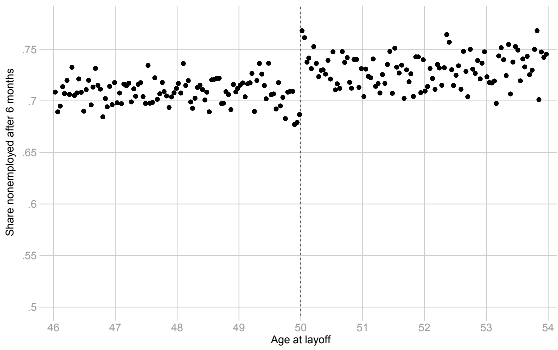
Note: The figure displays the average UI receipt in euros (panel (a)) and average UI benefit duration in weeks (panel (b)) by age-at-layoff. In both panels each dot represents a two week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period February 2009 till December 2012. The underlying data consists of 249,581 layoffs.

Figure 5: Nonemployment survival probabilities

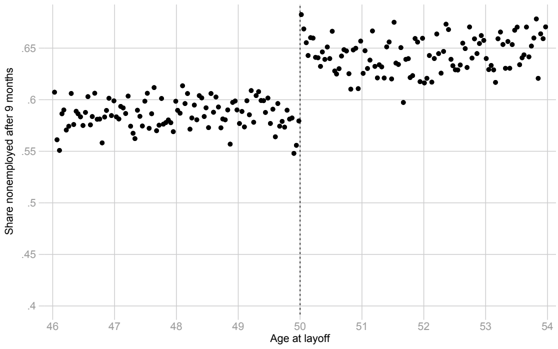
(a) Probability of still being unemployed after 3 months



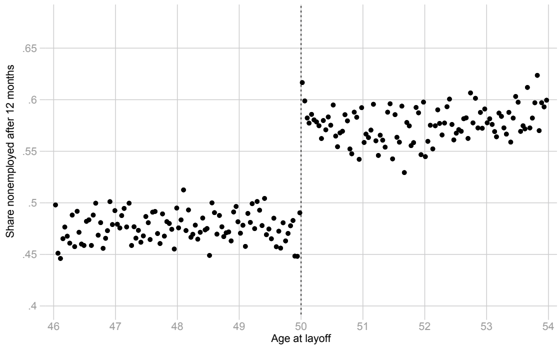
(b) Probability of still being unemployed after 6 months



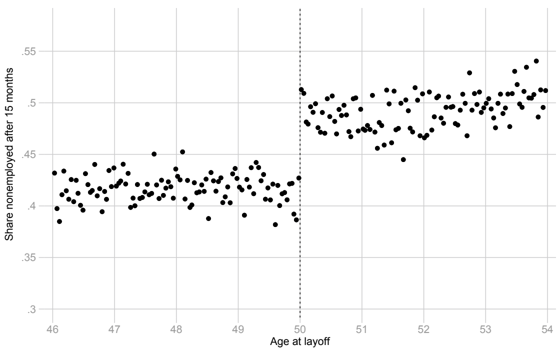
(c) Probability of still being unemployed after 9 months



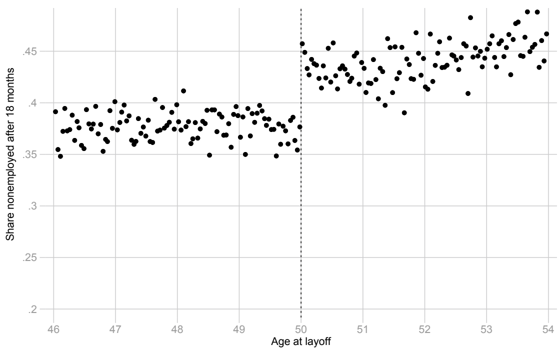
(d) Probability of still being unemployed after 12 months



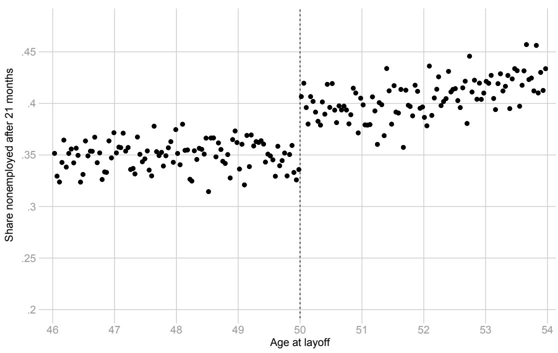
(e) Probability of still being unemployed after 15 months



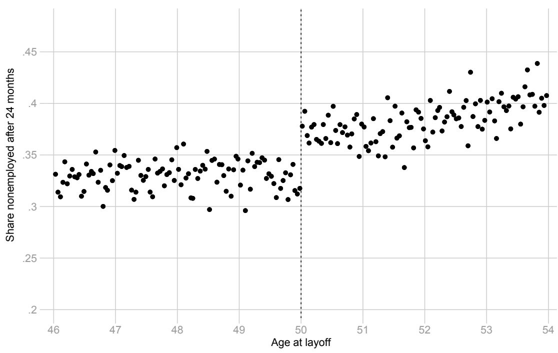
(f) Probability of still being unemployed after 18 months



(g) Probability of still being unemployed after 21 months



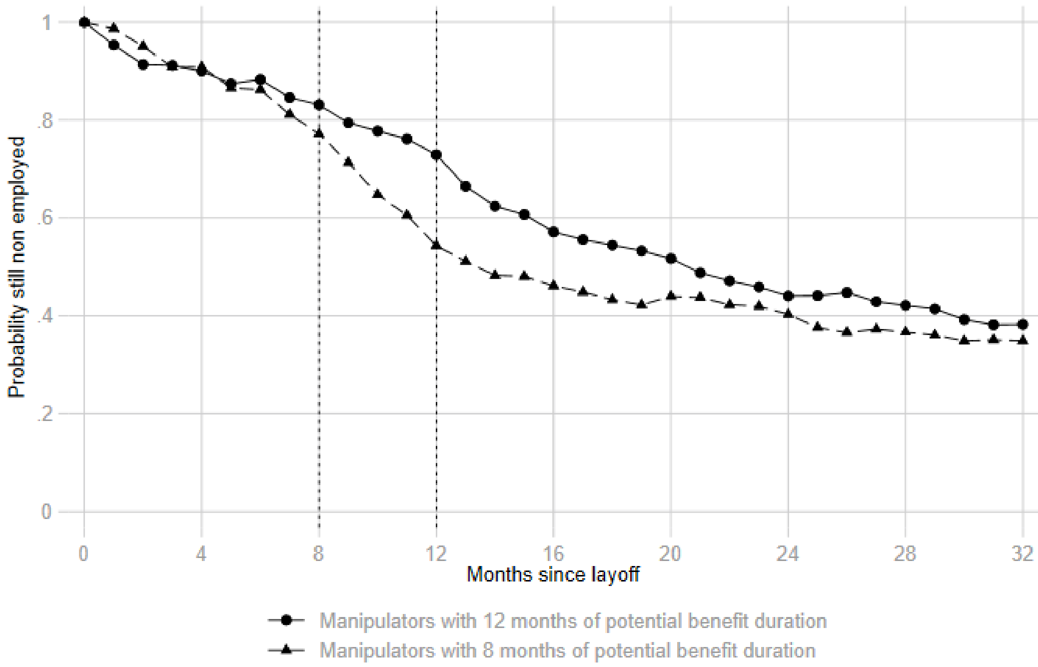
(h) Probability of still being unemployed after 24 months



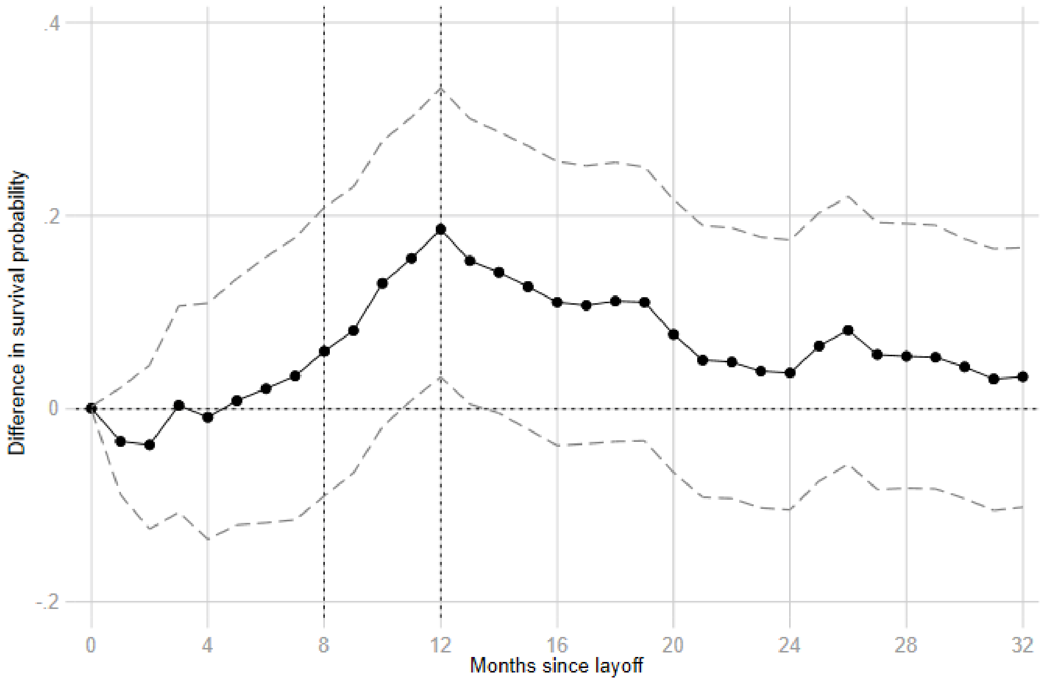
Note: The figures show the share of laid off workers, who are still unemployed after 3, 6, ..., 24 months. In all panels each dot represents a two week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period February 2009 till December 2012. The underlying data consists of 249,581 layoffs.

Figure 6: Manipulators with 8 and 12 months of potential benefit duration

(a) Nonemployment survival rates



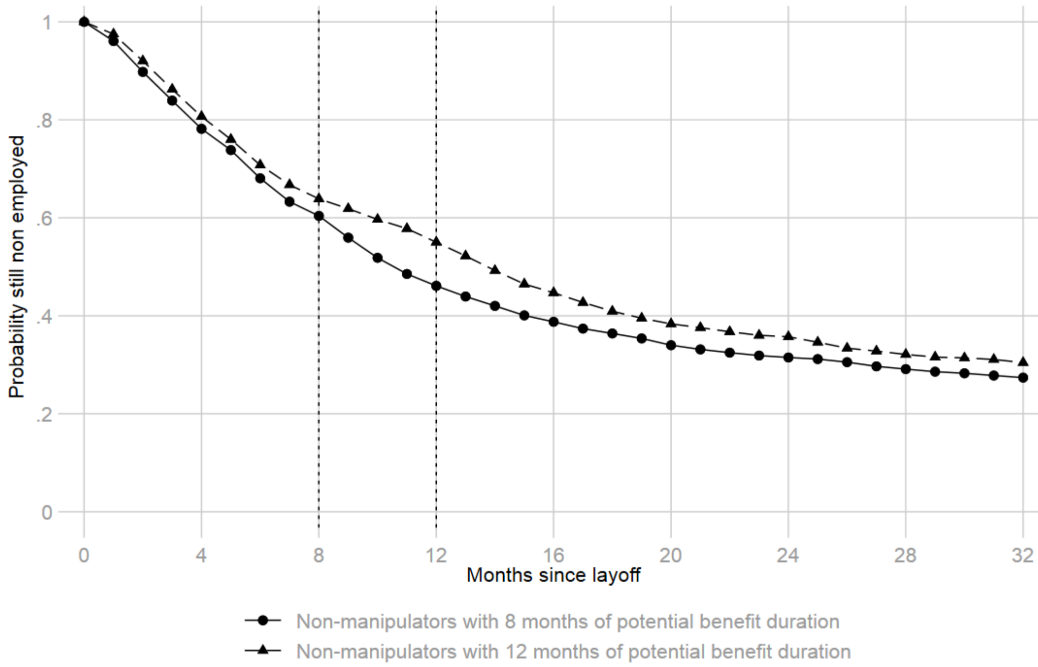
(b) Difference in survival rates



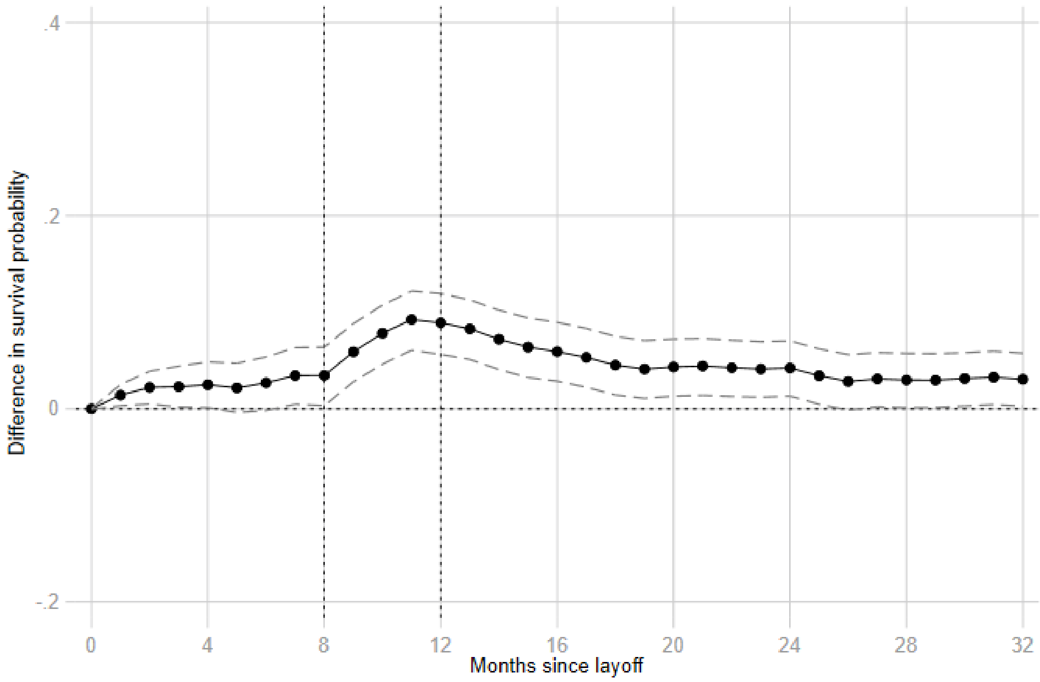
Note: Panel (a) plots point estimates of manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in section 4.2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 7: Manipulators with 8 and 12 months of potential benefit duration

(a) Nonemployment survival rates

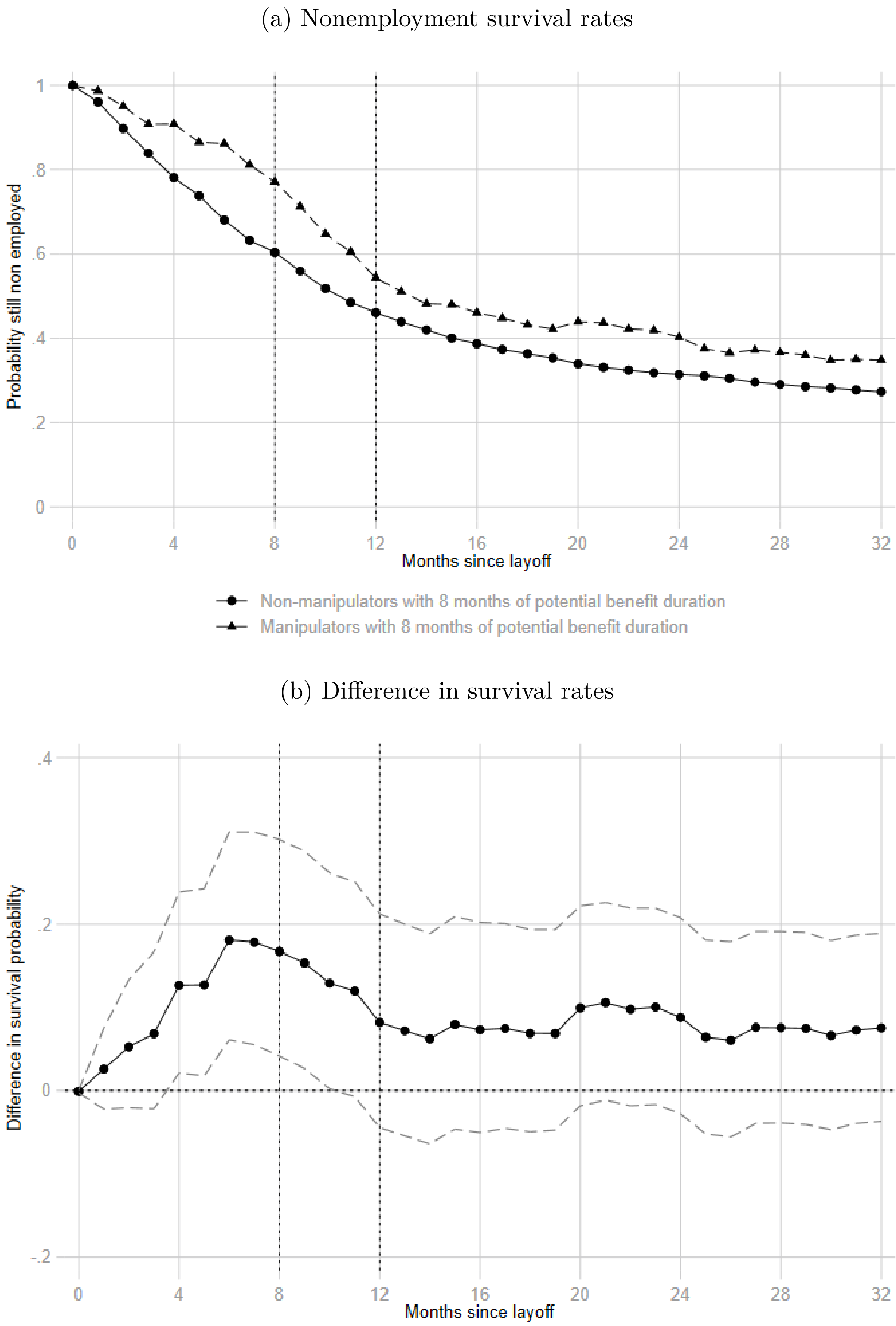


(b) Difference in survival rates



Note: Panel (a) plots point estimates of non-manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in section 4.2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

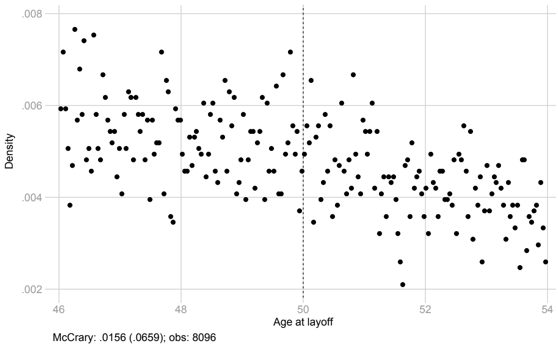
Figure 8: Manipulators and non-manipulators with 8 months of potential benefit duration



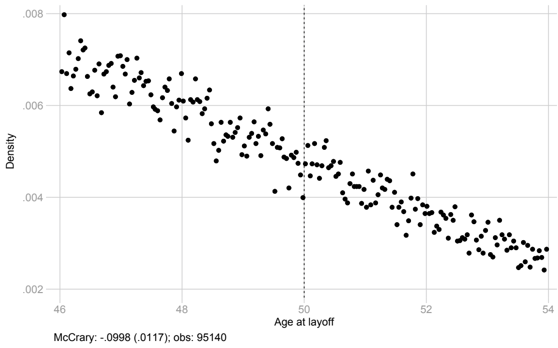
Note: Panel (a) plots point estimates of manipulators' and non-manipulators' nonemployment survival over the first 32 months after layoff under eight months of PBD. The estimation of the former is outlined in section 4.2. The latter represents the observed mean survival rate in the missing region. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 9: Density of Layoff by Private and Public sector and by Contract Type

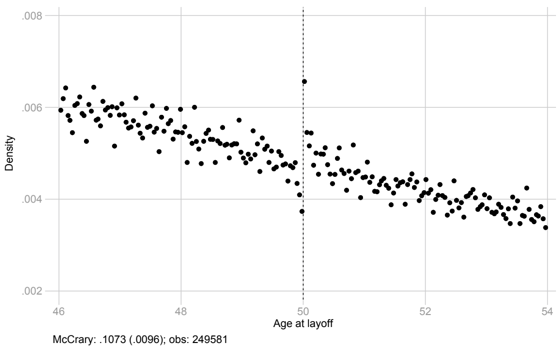
(a) Public Sector: Permanent



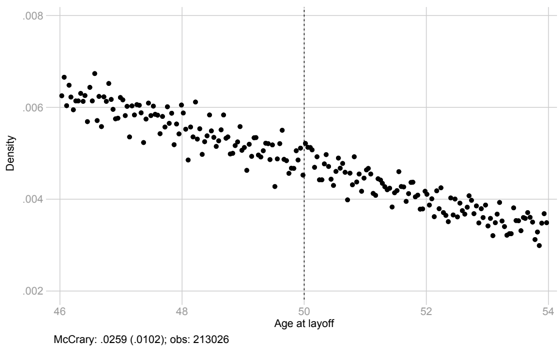
(b) Public Sector: Temporary



(c) Private Sector: Permanent



(d) Private Sector: Temporary

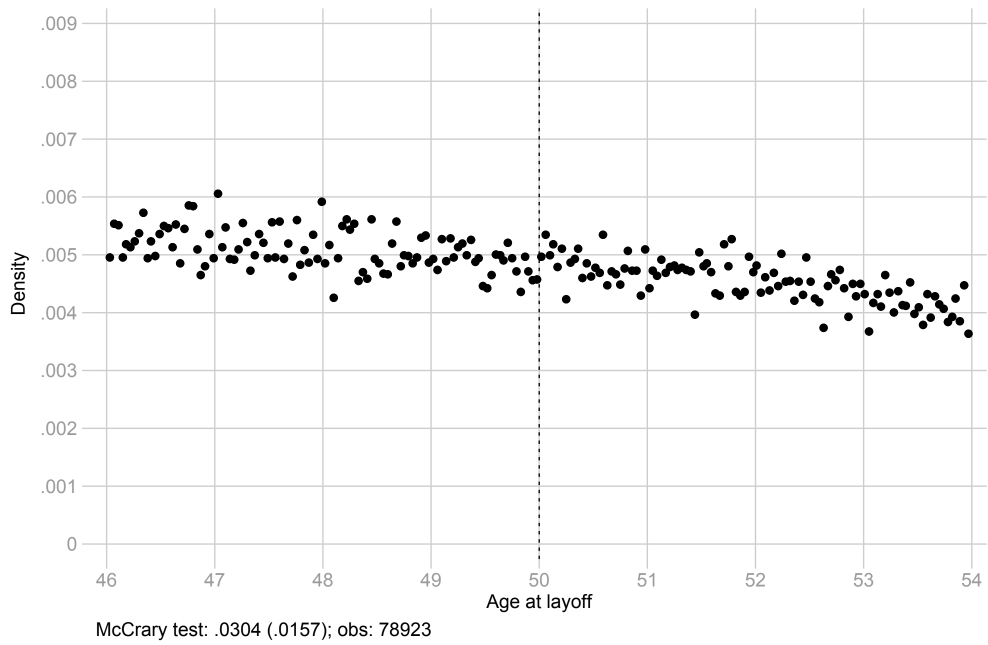


Note: The figure shows the density of layoffs by contract type. The data cover the period February 2009 till December 2012. In all panels each dot represents a two-week bin. Individuals are classified as "public sector" workers if they are present in the SIP database but a corresponding employment spell could not be observed in the data for universe of workers in the private sector (UNIEMENS).

Figure 10: Placebo checks: MiniASpI and NASpI and density of recipients at 50 years of age

(a) MiniASpI

(b) NASpI



Note: The figure shows the density of layoffs for workers laid off in the private sector and receiving MiniASpI (2013-April 2015) or NASpI (2016). In both panels each dot represents a two-week bin. The sample has been restricted to workers coming from permanent contracts in the private sector.

Tables

Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
Nonemployment outcomes				
UI Benefit receipt duration (in weeks)	29.853	15.923	0.14	52.00
Nonemployment duration (in weeks)	89.995	79.092	0.00	208.00
Nonemployment survival prob. 8 months	0.502	0.500	0.00	1.00
Nonemployment survival prob. 12 months	0.388	0.487	0.00	1.00
Individual characteristics				
Female (share)	0.311	0.463	0.00	1.00
Time since first employment (in years)	27.656	8.552	2.00	40.00
White collar (share)	0.208	0.406	0.00	1.00
North (share)	0.367	0.482	0.00	1.00
Center (share)	0.174	0.379	0.00	1.00
South and islands (share)	0.459	0.498	0.00	1.00
Previous job characteristics				
Full time (share)	0.807	0.395	0.00	1.00
Tenure (in years)	5.931	6.113	0.08	30.00
Daily income (in Euros)	69.900	70.300	0.04	13,981.01
Firm age (in years)	14.367	12.115	0.00	109.83
Firm size	28.158	259.010	1.00	14,103.00
Firm size below 15 (share)	0.606	0.489	0.00	1.00
Firm size between 15 and 49 (share)	0.213	0.409	0.00	1.00
Firm size above 49 (share)	0.181	0.385	0.00	1.00

Note: The table reports summary statistics of our main sample consisting of all OUB claims between February 2009 and December 2012 from individuals who are employed in permanent private sector work arrangements and are between 46-54 years of age at the time of layoff. The sample contains a total of 249,581 nonemployment spells from 210,041 individual workers. Nonemployment duration is censored at four years and defined as the time distance between the date of layoff and the date of the first re-employment event that leads to UI benefit termination. Tenure is defined as the total number of years (not necessarily uninterrupted) spent with the last employer. The geographical South and islands dummy encompasses employment in one of the following regions: Abruzzo, Basilicata, Calabria, Molise, Puglia, Sardinia and Sicilia.

Table 2: Headcount and share estimates

(1) Headcount manipulators missing region	(2) Headcount non-manipulators missing region	(3) Headcount manipulators excess region	(4) Headcount all other ind. excess region	(5) Share estimate missing	(6) Share estimate excess
571.2	3038.0	608.6	2390.4	0.158	0.203
(458.5,680.0)	(2931.0,3150.0)	(496.0,718.5)	(2379.4,2401.3)	(0.127,0.188)	(0.172,0.231)

Note: The table reports estimates of the total number of individuals in four groups: (1) manipulators in the missing region, (2) non-manipulators in the missing region, (3) manipulators in the excess region and (4) all other individuals in the excess region. Column (5) and (6) contain estimates for the share of manipulators in the missing and excess region, respectively. Bootstrapped 95% confidence intervals are in parentheses. We formally define all quantities in Section 4.1. All results are based on our main sample consisting of 249,581 observations.

Table 3: UI Benefit receipt estimates (Euros)

(1) Benefit receipt manipulators missing region	(2) Benefit receipt non-manipulators missing region	(3) Benefit receipt manipulators excess region	(4) Benefit receipt all other ind. excess region	(5) Benefit receipt response manipulators	(6) Benefit receipt response non-manipulators
5814.2	5223.5	8053.6	7044.2	2239.4	1636.9
(5178.5, 6459.2)	(5125.0, 5325.7)	(7326.9, 8836.5)	(6974.5, 7112.4)	(1276.7,3261.6)	(1410.9,1849.6)

Note: The table reports estimates of the mean UI benefit receipt (in Euro) of individuals in four groups: (1) manipulators in the missing region, (2) non-manipulators in the missing region, (3) manipulators in the excess region and (4) all other individuals in the excess region. Column (5) and (6) contain estimates of the UI benefit receipt response of manipulators and non-manipulators, respectively. Bootstrapped 95% confidence intervals are in parenthesis. We formally define all quantities in Section 4.2. All results are based on our main sample consisting of 249,581 observations.

Table 4: Benefit duration estimates (weeks)

(1) Benefit duration manipulators missing region	(2) Benefit duration non-manipulators missing region	(3) Benefit duration manipulators excess region	(4) Benefit duration all other ind. excess region	(5) Benefit duration response manipulators	(6) Benefit duration response non-manipulators
27.8	24.8	41.8	35.8	13.9	9.9
(25.2,30.6)	(24.4,25.2)	(38.3,45.6)	(35.5,36.2)	(9.4,18.7)	(8.9,10.9)

Note: The table reports estimates of the mean benefit duration (in weeks) of individuals in four groups: (1) manipulators in the missing region, (2) non-manipulators in the missing region, (3) manipulators in the excess region and (4) all other individuals in the excess region. Column (5) and (6) contain estimates of the benefit duration response of manipulators and non-manipulators, respectively. Bootstrapped 95% confidence intervals are in parenthesis. We formally define all quantities in Section 4.2. All results are based on our main sample consisting of 249,581 observations.

Table 5: BC/MC Ratios

	BC/MC ratios	
	(1) without taxes	(2) with taxes ($\tau = 3\%$)
(a) *Manipulators*	0.24 [0.02; 0.89]	0.32 [0.03; 1.13]
(b) *Non-manipulators*	0.26 [0.12; 0.41]	0.32 [0.15; 0.50]

Note: The table reports BC/MC ratios for manipulators (a) and non-manipulators (b). BC/MC without taxes are defined in equation 2 in Section 3.1. BC/MC with taxes are defined in equation 4 in the same section. Bootstrapped 95% confidence intervals in parentheses.

Table 6: Test for Discontinuity of observables at cutoff

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Simple RD model			**"Donut" model**			
Variable	$\lambda_0^{>50}$	s.e.	T-stat (1)/(2)	$\theta_0^{>50}$	s.e.	T-stat (4)/(5)	Baseline
Female	0.011	0.005	**2.43**	0.000	0.005	-0.03	0.31
Full Time	0.001	0.005	0.26	0.005	0.005	1.09	0.81
White Collar	0.017	0.005	**3.71**	0.005	0.005	0.86	0.20
Market Potential Experience	0.177	0.095	1.85	0.093	0.107	0.87	27.34
Tenure	-0.040	0.063	-0.63	-0.095	0.078	-1.22	5.85
(Log) Daily Wage	0.000	0.006	0.03	0.005	0.007	0.69	4.17
South	-0.003	0.006	-0.56	-0.005	0.007	-0.74	0.47
(Log) Size	-0.038	0.014	**-2.72**	-0.015	0.016	-0.94	2.02
Age Last Firm (Years)	-.116	.130	-0.89	-.122	.137	-0.89	14.269

Note: The table reports results for the robustness tests described in Section 6.4. The analysis based on 249,581 spells of individuals laid off from a permanent contract between 2009 and 2012. $\lambda_0^{>50}$ and $\theta_0^{>50}$ are OLS coefficients from specifications 27 and 28, respectively. Columns from (1) to (3) report RDD coefficient for dicontinuity of observables at cutoff for whole sample together with standard error and associated t-stat. Columns from (4) to (6) replicates same exercise for sample excluding manipulation region. In both cases, the specification includes a dummy equal to 1 if the worker is fired after turning 50 years of age, a squared polynomial in age in difference from the cutoff and flexible on the two sides. T-stats are bold if coefficients are significantly different from zero at the 5% level. Baseline reports average for the individuals fired between 49 and 50 years of age. Standard Errors clustered at local labour market level.

Table 7: Difference in observables between manipulators and other groups

Variable	(1) Manipulators	(2) Non Manipulators	(3) Difference (1)-(2)	(4) Baseline Group	(5) Difference (1)-(4)
Female	0.450	0.270	0.180	0.306	0.144
			[0.100; 0.281]		[0.078; 0.206]
White Collar	0.351	0.180	0.170	0.199	0.152
			[0.101;0.239]		[0.094; 0.208]
Full Time	0.754	0.822	-0.067	0.806	-0.052
			[-0.134; -0.000]		[-0.106; 0.004]
Tenure	6.577	5.718	0.859	5.933	0.644
			[-0.142; 1.853]		[-0.166; 1.449]
Log Daily Wage	4.115	4.176	-0.0610	4.168	-0.053
			[-0.142; 0.023]		[-0.120; 0.015]
South	0.483	0.471	0.012	0.469	0.014
			[-0.072; 0.098]		[-0.056; 0.083]
(Log) Size	1.862	2.258	-0.395	2.207	-0.345
			[-0.640; -0.155]		[-0.546; -0.148]
Age firm (years)	14.546	14.335	0.211	14.482	0.064
			[-1.945; 2.320]		[-1.647; 1.780]

Note: The table reports differences in observable characteristics between manipulators and non-manipulators. The analysis is based on 249,581 spells of individuals laid off from a permanent contract between 2009 and 2012. Column (1) reports estimated means of manipulators' characteristics; column (2) does the same for non-manipulators; Column (4) reports estimated means for baseline group, defined as the set of individuals we would have observed in the missing region, in absence of manipulation. Columns (3) and (5) report the difference between these groups. Bootstrapped confidence interval at 95% are reported in parentheses.

Table 8: Testing for discontinuities in the layoff density at the threshold

	(1) Whole sample	(2) Without manipulation region	(3) Without manipulation region (alternative definition)
Age	-0.0366***	-0.0335***	-0.0319***
	(0.0027)	(0.0023)	(0.0026)
$\mathbb{I}[\text{age} \geq 50] \times$ Age	-0.0000	0.00029	0.0002
	(0.0042)	(0.0032)	(0.0033)
$\mathbb{I}[\text{age} \geq 50]$	0.0270**	0.0100	0.0015
	(0.0105)	(0.0075)	(0.0079)
Observations	208	203	195
R-squared	0.866	0.898	0.9040
Mean	.48	.48	.48

Note: The table reports a parametric test for the discontinuity in the density of layoff at the cutoff of 50 years of age. Column (1) includes all bins. Column (2) excludes the manipulation region which encompasses the three bins before the cutoff and the two bins after the cutoff. Column (3) also excludes the manipulation region but uses an alternative definition of such region. Details about the alternative definition are provided in Section 6.3. Robust standard errors are reported in parentheses.

A Further details about Italian UI

A.1 Other UI benefit schemes active in Italy from 2009-2012

During the years from 2009 to 2012 two other main UI schemes were in place: the Reduced Unemployment Benefits (RUB) and the Mobility Indemnity (MI).[25]

On the one hand, the RUB was directed to workers who would have been eligible for OUB, except for contribution requirements. While still requiring the first contribution to social security to have happened two years before, the RUB scheme only required 13 weeks (78 days) worked in the last year, instead of 52. Potential benefit duration was proportional to the days worked in the previous year (up to 180 days), while the replacement rate granted 35% of the average wage earned in the previous year for the first 120 days and 40% for the following 60 days. This measure was substantially less generous than OUB. As a consequence, had workers met OUB requirements, they would have chosen the former. [26]

On the other hand, MI was active until 2017 and was targeted to workers fired during mass layoffs or business reorganizations. This measure combined a long and generous income support with active labor market policies, with an eye at improving workers' occupational perspectives. During the period under study the potential duration of this scheme depended on the worker's age at layoff and the geographical area where she worked, with a maximum PBD of 48 months in southern regions and of 36 months in northern regions. The benefit amounted to 80% of the salary for the first 12 months (with a cap annually set by law) and 64% during the following months.

This measure represents a particularly attractive alternative for individuals involved in mass layoffs and could lead to underrepresentation of these types of workers in our sample. What is more relevant for our purposes is that selection for this benefit is mostly beyond the control of the worker: indeed, the firm needed to be undergoing significant economic restructuring and have a minimum size, while workers needed to meet some tenure requirements.

[25] *Indennità di Disoccupazione Ordinaria a Requisiti Ridotti* and *Indennità di Mobilità* in Italian, respectively.

[26] For additional information, please refer to Anastasia et al. (2009).

A.2 Other UI benefit schemes active in Italy after 2012

The Italian welfare system underwent several reforms after 2012, which aimed reducing the fragmentation of benefits. In January 2013, both the OUB and the RUB were replaced respectively by the ASpI and MiniASpI.[27] On the one hand, the ASpI mimicked many aspects of the OUB both in terms of requirements and in terms of structure of the benefit. In order to be eligible for the benefit, the worker had to have contributed for the first time to social security at least two years before the start of the unemployment spell and needed to have cumulated at least one year of work in the previous two years. Similarly to the OUB, the worker was eligible to eight months of benefit if she was fired before turning fifty while she was eligible to twelve months if the worker was fired after turning fifty years of age. The duration of the benefit was later modified on several occasions in 2014 and 2015, which makes it more difficult to use it for our analysis. The amount of the benefit was proportional to wages in the last two years and the worker received 75% of the average reference wage for the first six months and the amount was reduced by 15 percentage points every six months (up to 45% after one year). On the other hand, the MiniASpI was aimed at workers who did not meet the requirement for the ASpI, but had cumulated at least thirteen weeks of work in the last year. Potential benefit duration was equal to half of the weeks worked in the last year. Benefit receipt was proportional to past wages: workers received 75% of the average wage received during the two previous years.

Following April 2015, both measures were replaced by a unique UI scheme which provided a homogeneous coverage to workers in case of layoff. The new benefit, the NASpI, was mostly based on the structure of the MiniASpI. Workers were eligible to the benefit if they had worked for at least 78 days in the year before the layoff. Potential benefit duration was equal to half of the weeks worked in the past 4 years. The benefit amount was proportional to past average wages with a decreasing schedule. More specifically, the worker was eligible to receive 75% of the average wage in the past four years and the amount was reduced by 3 percentage points for every month after the first four. This new scheme created greater harmonization within the UI system and provided uniform coverage to workers previously eligible to different programs. In addition, it removed discontinuities

[27] *Assicurazione Sociale per l'Impiego* in Italian.

in potential benefit duration, thus removing incentives for workers to delay their layoff.