MANIPULATION AND SELECTION IN UNEMPLOYMENT INSURANCE*

Luca Citino† Kilian Russ‡ Vincenzo Scrutinio§

October 2024

Abstract

In this paper we study the selection patterns of individuals who manage to have their layoff delayed around an age-at-layoff threshold entitling them to four additional months of unemployment insurance (UI), i.e. manipulators. Using administrative data from Italy and bunching techniques, we document substantial manipulation around the cutoff and show that manipulators are selected on their long-term nonemployment risk but not on their moral hazard cost. Finally, we develop a sufficient statistics framework to assess how these findings affect optimal UI duration in presence of manipulation.

Keywords: Unemployment Insurance, Adverse Selection, Bunching
JEL classification: E24, J64, J65, J68.

*This paper is a revised version of the paper "Happy Birthday? Manipulation and Selection in Unemployment Insurance". We are extremely thankful to our advisors Stephen Machin, Alan Manning, Jörn-Steffen Pischke and Johannes Spinnewijn for continuous guidance and support. This project also benefited from discussions with Andrea Alati, Andres Barrios Fernandez, Miguel Bandeira, Fabio Bertolotti, Rebecca Diamond, François Gerard, Simon Jäger, Felix König, Camille Landais, Attila Lindner, Clara Martinez-Toledano, Matteo Paradisi, Michele Pellizzari, Petra Persson, Frank Pisch, Michel Serafinelli, Enrico Sette, Martina Zanella, Josef Zweimüller, and seminar participants at briq, EUI, INPS, EIEF and the LSE. This project was carried out while Kilian Russ was visiting the London School of Economics as part of the European Doctoral Programme in Quantitative Economics. Financial support from the London School of Economics and the Bonn Graduate School of Economics is gratefully acknowledged. The realization of this project was possible thanks to the VisitInps initiative. We are very grateful to Massimo Antichi, Elio Bellucci, Mariella Cozzolino, Edoardo Di Porto, Paolo Naticchioni and all the staff of Direzione Centrale Studi e Ricerche for their invaluable support with the data. The opinions expressed in this paper are those of the authors alone and do not necessarily reflect the views of the Bank of Italy or INPS. All remaining errors are ours alone.

†Bank of Italy; Via Nazionale 91, Rome, Italy. Corresponding author; Email: luca.citino@bancaditalia.it
‡unaffiliated
§University of Bologna, CEP, IZA; Email: vincenzo.scrutinio@unibo.it

1 Introduction

The targeting of public policies on the basis of observable individual characteristics is widespread in OECD countries. Governments tax individuals based on their marital status, provide welfare payments which depend on the number of children in the household, and tie disability insurance to particular medical conditions. Tying public benefits to observable information holds the potential to increase cost-effectiveness and provide assistance and support to those most in need. While the theoretical desirability for targeting based on immutable tags has long been recognized on the grounds of efficiency considerations (Akerlof, 1978), in practice policy makers often have to condition benefits on *endogenous* tags, which leave room for strategic manipulation and self-selection of unintended recipients into benefit schemes.

How should we view such manipulation? Typically, the initial inclination is to regard it 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* behind it. 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. Understanding the underlying rationale and subsequent behavioral changes is essential for assessing the welfare consequences of manipulation and for the optimal design of these benefit schemes. Intuitively, how much policy differentiation is appropriate depends on who the manipulators are and how responsive they are to benefits. Given the prevalence of *endogenous* tags, this question seems highly policy-relevant but has not been addressed before.

In this paper, we aim to gain a deeper understanding of these issues in the context of unemployment insurance (UI), where differentiated policies and manipulation are widespread (see Spinnewijn (2020) for a survey, and Van Doornik et al. (2023) and Khoury (2023) for recent evidence of manipulation). To do this, we rely on confidential administrative data from the Italian Social Security Institute (INPS) on the universe of UI claims between 2009 and 2012, which we link to matched employer-employee records. In this period potential benefit duration (PBD) in Italy featured a sharp discontinuity based on the worker's age at layoff. Specifically, individuals separating before age fifty were entitled to eight months of UI, while those separating afterwards were entitled to twelve months of UI.[1] This notch in the PBD schedule created substantial incentives for some workers to strategically delay their layoffs, in order to be eligible for longer UI.

While it is relatively straightforward to quantify the impact of such manipulation on

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

public spending, providing a comprehensive analysis of the motivations behind it is a more complex task. In this setting we distinguish two theoretical motives for selecting into manipulation: moral hazard and long-term nonemployment risk. To fix ideas, consider two extreme cases. First, suppose manipulators are individuals who would have found a job exactly eight months after layoff, but *because* they are now self-selecting into a more generous scheme, they are staying in nonemployment for four additional months before taking up their next job. The four additional months of benefits are paid as a consequence of the fact that individuals change their job search effort in response to longer UI. In this case manipulation is motivated by an anticipated moral hazard response to longer UI.[2] On the contrary, suppose manipulators would stay in nonemployment for at least twelve months irrespectively of the length of UI coverage. In this case, manipulation also allows them to collect four additional months of UI benefits. However, such additional benefits are paid mechanically due to longer statutory coverage, without any change in job search effort and total nonemployment duration. In this latter case, it is individuals' long-term nonemployment risk that drives selection into manipulation, rather than an anticipated moral hazard response. In reality manipulation is likely motivated by a combination of these forces, but it is clear that determining which one plays a stronger role may lead to very different positive and normative views related to manipulation. In this paper we estimate the relative importance of these two drivers for manipulators in explaining the total increase in UI benefit receipt for this group. We also show to what extent manipulators are selected on risk and moral hazard, when compared to non-manipulators, i.e. individuals who were laid off just before their fiftieth birthday.

We start our analysis by providing clear graphical evidence of manipulation, in the form of a sharp drop in the number of layoffs right before age fifty and a spike of equal size right after. Using bunching techniques (Kleven and Waseem, 2013) we estimate that over 15% of all layoffs of permanent workers in the private sector within six weeks before workers' fiftieth birthday are strategically delayed. To the contrary, we do not detect any manipulation for workers in the public sector or temporary workers in the private sector. In addition, we find no evidence that the notch leads to an overall increase in the number of job separations, what we label an *extensive margin* response.

Over the subsequent nonemployment spell, on average manipulators spend 14 more weeks on benefits and collect 2,239 additional Euro each, which corresponds to a 38.5% increase over their baseline UI benefit receipt. While these numbers seem large, this increase in spending could reflect both a decrease in job search effort in response to longer UI or a mechanical transfer to people who are at high risk of long-term nonemployment regardless of the length of UI coverage. Thus, after

[2] Estimating selection on moral hazard has proven notoriously difficult in practice, resulting in relatively little empirical work on the topic, with Einav et al. (2013) and Landais et al. (2021) representing two notable exceptions in the context of health and unemployment insurance, respectively.

quantifying the extent of manipulation, we study the relative importance of selection on nonemployment risk and selection on moral hazard in explaining the increased UI benefit receipt for manipulators. Disentangling these two types of selection requires constructing manipulators' counterfactual nonemployment duration in a world where no manipulation is allowed. This is hard both because we only observe "manipulated" data and because we cannot discern manipulators' identity. We overcome these challenges by combining ideas from the bunching estimator of Diamond and Persson (2016) and the untreated outcome test of Kowalski (2021). The intuition behind these methods is that abnormal changes in the outcome variables (e.g. nonemployment probabilities at different points in time during the nonemployment spell) close to the threshold, both on the side from where the manipulators move away and on the other side where the manipulators move to, combined with the share of manipulators driving these outcome changes, allow inferring the average outcomes for manipulators and non-manipulators under either policy regime. This approach relies on standard smoothness assumptions about the relationship between the outcome and the "running variable" (i.e. age-at-layoff), absent manipulation. Under these assumptions and thanks to the granularity of the admin data, our strategy allows us to study both how manipulators' monthly survival rates in nonemployment respond to longer coverage, but also how their *baseline* (or "untreated") monthly non-employment risk differs from that of non-manipulators. We are the first to combine insights from both Diamond and Persson (2016) and Kowalski (2021) to study at the same time selection on untreated outcomes and selection on treatment effects in a regression discontinuity (RD) setting with manipulation. Our empirical methods rely on classic bunching assumptions, such as the smoothness of the counterfactual density and the absence of extensive margin responses. This approach does not require knowledge of the manipulators' identities or policy variations induced by reforms over time, making it portable to other contexts. In addition, our estimators do not depend on structural assumptions such as the absence of choice frictions or rational beliefs.

Our survival analyses reveal that approximately 81% of the increase in UI benefit receipt for manipulators is mechanically due to longer coverage, while the remaining 19% is the result of a decline in job search effort caused by longer PBD. In order to measure manipulators' moral hazard responses, we compute BC/MC ratios (Schmieder et al., 2012; Schmieder and von Wachter, 2017; Gerard and Gonzaga, 2021), which relate the changes in UI expenditure and tax revenues arising from changes in the search behaviour of workers (behavioral cost, BC) to the cost arising from longer coverage without changes in the search behaviour of workers (mechanical cost, MC). By normalizing behavioural responses by the effective transfer (or economic incentive) the government is providing, BC/MC ratios allow inter-group comparisons (between, for example, manipulators and non-manipulators) of the cost of extra insurance for the government. We show that for one euro of mechanical UI transfer towards manipulators the government pays an additional 24 cents due to behavioral responses during nonemployment. Interestingly, we find virtually the same result when studying non-manipulators. This implies that manipulators are *not* adversely selected on their efficiency cost, which may mitigate concerns about anticipated moral hazard responses being the prime motive for selection into manipulation. Rather, 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 a 16.8 percentage points higher probability than non-manipulators.

Overall, the underlying firm-worker collusion decision to delay the date of layoff seems to act as an effective screening mechanism for long-term nonemployment risk, while preventing selection on moral hazard responses. We investigate this mechanism further by documenting observable worker and firm characteristics that are associated with manipulation. Manipulation is relatively more prevalent among female, part-time, white-collar workers and in smaller firms. Manipulators' careers are similar to non-manipulators', both in terms of months worked, past and current earnings. However manipulators are more likely to have been absent from work for various reasons (e.g. sickness, accidents, a child's illness) in the years before the separation, although these differences are not always statistically significant. On the one hand these results suggest that lower adjustment costs and proximity between workers and supervisors may facilitate manipulation in our context. On the other hand, they also suggest that firms may favor disadvantaged workers with poor labor market prospects, by using visible indicators to determine who might benefit from longer PBD, whether due to illness or caregiving responsibilities.

Our results carry broader implications for the design of targeted UI programs in the presence of manipulation, an understudied area of research of direct policy relevance (Spinnewijn, 2020). In the last part of the paper we provide a sufficient statistics characterization that highlights which empirical moments are required to evaluate targeted UI when manipulation occurs with respect to a *given* tag. We show that the share of manipulators, the degree of selection on BC/MC ratios and selection on baseline nonemployment risk, which we estimate in the paper, play a crucial role, because they contribute to enlarging or shrinking manipulators' impact on the government budget. Since manipulation is localized near the age-fifty threshold, the short prolonging of unproductive work relations is unlikely to generate first-order welfare consequences and so we ignore them in the paper. To the contrary, our focus on manipulation is motivated by the first-order welfare gains it produces for manipulators, together with the fiscal externalities it imposes on the government budget.

Our theory shows that manipulation can improve welfare the more manipulators are positively selected on their consumption smoothing value and negatively selected their effective moral hazard cost. The model thus reveals whether or not further differentiation with respect to a *given* tag – after taking manipulation into account – has any potential benefit or if the optimal policy is in fact undifferentiated. Our theory guides the design of differentiated UI schedules with respect to a *given* endogenous tag, but it does not provide guidance for choosing among several potential tags or for assessing their appropriateness more generally. Finding out which dimensions allow welfare-improving targeting in different policies is a fruitful avenue for future research, although policy makers might ultimately refrain from exploiting some of them, because of, for example, transparency, administrative costs, horizontal equity and fairness concerns.

Ideally, policymakers might eliminate manipulation by directly addressing information asymmetries. However, finding credibly *exogenous* tags in the realm of social insurance has proven hard (Spinnewijn, 2020), and the challenge becomes that of designing an efficient *second-best* social insurance system (Nichols and Zeckhauser, 1982). For example, recent studies (Wunsch and Zabrodina, 2023; Mueller and Spinnewijn, 2023) show that job histories predict long-term nonemployment risk and moral hazard responses particularly well. While these may be information-rich tags, they are also highly endogenous and prone to unintended behavioral responses. Concretely, if more discontinuous job histories made workers eligible for longer UI, individuals at the margin would respond by staying unemployed for a longer period of time. The selection into these behavioral responses and associated fiscal externalities could invalidate or reinforce any beneficial effects from differentiation along these informative tags, which is precisely the mechanism we highlight in the paper. Our paper contributes to this discussion by identifying and measuring the fiscal externalities associated with behavioral responses to endogenous tags in UI, offering insights on how optimal second-best policies should account for these effects. Unfortunately, providing a full welfare assessment of manipulation would require estimating other moments for which nor the data nor the appropriate variation is available, notably manipulators' selection on the value they assign to longer UI.

Our results show that manipulation is highly concentrated around age-fifty threshold. For this reason, one might worry that manipulation is almost inconsequential for welfare or optimal UI duration in our setting. Although the number of manipulators is indeed small compared to the overall population of UI claimants, we think that our results are still informative, for three reasons. First, the selection patterns that we observe could also be present in other settings, where policy rules are more ambitiously differentiated and thus the welfare consequences of ignoring manipulation could be larger. Our methodology could be readily and fruitfully applied to these cases, allowing to characterize manipulators, together with the costs and benefits associated to their actions. Secondly, we show that individuals possess relevant information concerning their long-term nonemployment risk. Finding ways to extract this information could substantially benefit governments' ability to target the most vulnerable workers. Lastly, our setting suggests that such information is – at least partially – in the hands of firms, which might also contribute to improve the targeting process by screening the "right" workers. Although estimates are often noisy, in our setting firms appear to favour individuals who showed signs of vulnerabilities in the past, such as more absences due to sickness, care duties and accidents. This suggests that firms can observe characteristics which help them to identify workers at a higher risk of long-term unemployment. Interestingly, these measures are also observed by the Social Security Institute, which pays the workers in case of accidents and absences, and could then potentially be used to further screen workers. Finding ways to exploit this informational advantage without providing firms with additional bargaining power over workers appears challenging but has the potential to improve targeting.

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

In this sense, we also contribute to the small but growing literature on manipulation in social insurance. Two recent contributions by Van Doornik et al. (2023) and Khoury (2023) also study manipulation in UI systems around an eligibility and seniority threshold in Brazil and France, respectively. Van Doornik et al. (2023) provide evidence of strategic collusion between workers and firms who time layoffs to coincide with workers' eligibility for UI in Brazil. Khoury (2023) 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. While both of these papers document the presence of manipulation in social insurance contexts, neither of these papers studies the selection patterns we analyze in our work nor they analyze the consequences of manipulation for optimal UI through the lenses of a model. In the last part of the paper we also provide a review of the literature to rationalize why manipulation may be present in some settings, but not others.

Although the main contribution of our paper is empirical, we do relate to the literature on the theoretical desirability of *tagging* (Akerlof, 1978). 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). Second, our setting gives rise to both selection on risk and moral hazard, which have traditionally been analysed separately.[3] Recent efforts to integrate the two are presented in Landais et al. (2021), Hendren et al. (2021) and Marone and Sabety (2022). All three contributions study the welfare implications of offering some form of choice in (regulated) insurance markets.[4] Importantly, and conceptually different from our setup, these papers focus on policies that do not discriminate between different individuals but rely on self-selection through market prices. In our paper we provide an innovative approach to estimate both selection on baseline risk and selection on moral hazard in contexts where a market for supplemental insurance does *not* exist. [5]

The fact that we find positive selection on long-term nonemployment risk also speaks to a literature studying the role of private information and (ex-post) adverse selection in explaining the unravelling of insurance markets, see e.g. Hendren (2017) for unemployment insurance and Cabral (2016) for dental insurance. Our results indicate that individuals hold information about their expected duration of unemployment around the time of layoff. This could be partially known by firms if workers find it worthwhile to disclose or whenever good proxies are available. In this respect, we show that manipulators are selected on several health-related indicators which may signal to firms their greater vulnerability in nonemployment, although estimates are often imprecise.

From a methodological perspective, our empirical strategy is most closely related to recent work by Diamond and Persson (2016), who study manipulation of test scores in Swedish high-stakes exams.[6] They propose a bunching estimator to measure the effect of teacher discretion in grading around important exam thresholds on students future labour market outcomes. They also show how these techniques can be used to study selection on observables. We extend their methodology to study selection on unobservables, that is potential "untreated" outcomes in absence of manipulation, similarly to the "untreated outcome test" of Kowalski (2021) in the instrumental variable setting, which we apply to regression discontinuity. We borrow several ideas from standard bunching techniques surveyed by Kleven (2016). Conceptually, our empirical insights also relate to the literature on "essential heterogeneity" in instrumental variable

[3]While moral hazard is the key concept in most of the work on unemployment insurance design, adverse selection has received a lot of attention in the context of health insurance, in particular, in the US context.

[4]In their work Landais et al. (2021) provide the first assessment of the desirability of a UI mandate in the Swedish context. Adverse selection under a universal mandate has also been studied in the context of health insurance, see Hackmann et al. (2015).

[5]Barnichon and Zylberberg (2022) show that it might be theoretically desirable to offer a menu of contracts to the unemployed screening individuals by how they trade lump-sum severance payments with UI benefits.

[6]For an example of test score manipulation in the US context, see Dee et al. (2019) who study the impact of manipulation of test scores in New York Regents Examinations on students subsequent educational outcomes.

settings, in which individuals select into treatment in part based on their anticipated treatment effect, see e.g. Heckman et al. (2006).

The remainder of the paper is organized as follows: Section 2 presents the institutional setting and data, Section 3 outlines the empirical strategy, Section 4 reports our results and robustness checks. We presents the main insights from our theory in Section 5 and in Section 6 we conclude.

2 The Italian Unemployment Insurance Scheme

2.1 Institutional Setting

2.1.1 Unemployment insurance in Italy

We study manipulation in the Italian *Ordinary Unemployment Benefits* scheme (OUB).[7] The OUB had been in place from the late 1930s until its abolishment and replacement in January 2013.[8] The structure of the benefit scheme changed over the years and we limit our attention to the 2009-2012 period, over which the characteristics of the OUB were stable. 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 (a layoff), or a quit for just cause, such as unpaid wages or harassment. Workers who left their job by using other types of voluntary quits and the self-employed were not eligible for OUB.[9]

To qualify for OUB, workers were also required to have some labor market attachment prior to the layoff. 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.[10]

Benefit levels were based on the average monthly wage, calculated over the three months preceding the layoff. 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.

PBD under OUB only depended on age at layoff: eight months if the layoff preceded the

[7] *Indennità di Disoccupazione Ordinaria a Requisiti Normali* in Italian. We are not the first to study the Italian OUB scheme, see, for example, Anastasia et al. (2009), Rosolia and Sestito (2012), Scrutinio (2018) and Albanese et al. (2020), of which we discuss the last in more detail in Appendix F.

[8] OUB was introduced through *Regio Decreto 14.* in April 1939 and replaced by the ASPI on the first of January, 2013.

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

[10] 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 E.

worker's fiftieth birthday and twelve months if it followed it. This discontinuous change (a notch) in coverage created a strong incentive for workers to delay their date of layoff so that it occurred after their fiftieth birthday.

2.1.2 Layoffs in Italy

During the period under study, the Italian employment protection legislation (EPL) allowed firms to lay off individual workers only under specified circumstances[11]: for economic reasons such as restructuring or downsizing (*giustificato motivo oggettivo*), for not performing job tasks (*giustificato motivo soggettivo*), or for outright misbehaviour (*giusta causa*). Contrary to the idea of *employment at will*, firms could not dismiss workers in order to substitute them with other workers.[12]

In all cases the employer must provide a layoff notice in written form, specifying the reasons for the layoff. Except in the case of misbehavior, the employer must provide an advance notice period. The length of the notice period is regulated by collective bargaining agreements and may vary with tenure and occupation of the worker.[13]

When laid off, workers can appeal to the court against dismissal. In this case, a judge would determine whether there are grounds to lay off the worker or whether the layoff was in fact illegitimate. During trial, the burden of proof on the reasons that led to the layoff is on the employer. If the layoff is deemed legitimate, no firing costs or compensation is due. In case of an illegitimate layoff, firms must pay firing costs which vary with the size of the firm. Firms up to 15 employees must pay a severance package (*tutela obbligatoria*) ranging from 2.5 to 6 months of salary. Firms with more than 15 employees must compensate workers with all wages foregone in the period between the dismissal and the court decision. Since trials can last multiple years, this could constitute a substantial cost for firms. Moreover, these larger firms were forced to reinstate the worker, unless she was willing to accept an additional severance payment of 15 monhts (Schivardi and Torrini, 2008).[14]

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

[11]Contrary to individual dismissals, collective dismissals make workers eligible for more generous benefit schemes not studied in this paper (Mobility Indemnity, MI). We provide more background details on these in Appendix E.

[12]For example, firms could not dismiss an older worker with high earnings to replace her with a younger worker paid at a lower wage (Schivardi and Torrini, 2008).

[13]For example, the wholesale and retail trade CBA requires 2 weeks of advance notice for low-ranked workers with less than 5 years of tenure, but 4 months for high-ranked with more than 10 years of tenure.

[14]Workers subject to collective dismissals are entitled to another type of UI called mobility indemnity (MI), which is outside the scope of our analysis. We describe this scheme in more detail in Appendix E

private sector. Information on UI claims comes from the SIP database,[15] which collects data on all income support measures administered by INPS as a consequence of a job separation. 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. Unfortunately, our data does not cover the years prior to February 2009 and the introduction of a new UI scheme in January 2013 prevents us from including later years.

The SIP database does not contain the date of re-employment after receiving UI. 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 date in UNIEMENS occurring after the individual stopped receiving UI.[16] The UNIEMENS database 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 database until 2016, which gives us at least four years of observations for all workers. We therefore censor all nonemployment durations at this horizon.

As it is common in many other administrative databases, our nonemployment measure combines true nonemployment and informal jobs, which we do not observe.[17] In Section 4.3 we discuss why the presence of informal jobs does not bias our empirical results nor it alters the conclusions of the paper. Intuitively, the efficiency cost of providing extra UI to a group of individuals only depends on the combined impact of true nonemployment and informality on the government budget, so that distinguishing between them is not relevant for welfare (Gerard and Gonzaga, 2021). For ease of exposition, we refer to the combination of true nonemployment and informality simply as 'nonemployment' throughout the rest of the paper.

Finally, we complement information on careers with INPS individual records on transfers received by workers due to several types of absences covered by benefits and social security contributions.[18] Notably, these transfers are observable by the firm. In particular we observe the total Euro amount received by the worker due to: sickness; work accidents; disability (own or of relatives); child leave related to a child birth or a child's illness. In the case of both work accidents and sickness we can only obtain information for events longer than 7 days, due to privacy reasons. For each of these types of events, we calculate the cumulative transfers received by the worker in the four years preceding the layoff in the separating firm and then normalize it by the average wage of the worker in

[15] *Sistema Informativo Percettori* in Italian.

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

[17] According to the Italian National Institute of Statistics (Istat), in the years under study (2009-2012) the share of informal workers oscillated between 12.2 and 12.6% of total employment.

[18] This information is available in the *Differenze accredito* database.

three months prior to the layoff, as reported by the SIP database.[19]

Initially we restrict our 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, had a job in the private sector, and claimed OUB. After the exclusion of a few observations with missing key information we are left with 462,607 separation episodes.

In Section 4.1 we show that manipulation is confined to *permanent* contracts in the private sector, which motivates us to focus on this subset of separations only. This restricted sample includes 249,581 separation episodes that led to OUB claims.[20] The choice of focusing on the private sector is also motivated by the fact that the UNIEMENS database does not contain information on public sector jobs. This means we would have no information about the previous work arrangement, nor would we observe re-employment for workers in the public sector. At this point, one might be worried that we are missing some re-employment events, namely, transitions from the private to the public sector after unemployment. This in unlikely to affect our results because transitions from private into public sector jobs should be rare for workers at such late stage in their careers.

Table 1 reports summary statistics for the full sample, i.e. temporary or permanent workers in the private sector, and the restricted sample of permanent workers in the private sector. Starting from workers in the full sample, we observe that the average worker receives UI for about 26 weeks (6 months), roughly corresponding to one third of the 70 weeks (21 months) of average nonemployment duration. About 50% and 39% of workers are still in nonemployment after eight and twelve months, respectively, implying substantial exhaustion risk. Workers in the sample are predominately male, on full time contracts, and employed in blue collar jobs. These workers have spent about 27.5 years in the labor market since their first job and almost 4.3 years in their last firm. In terms of geographic distribution, 43% of workers are laid off in the South or the Islands.[21] Workers earned about 70 Euro per day (gross) which is equivalent to $70 \times 26 = 1820$ Euro per month if the worker is on a full time contract.[22] The separating firm is relatively old (15 years) and large (85 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. About 65% of the workers in the

[19]The dataset does not provide the number of absence days; it only reports the total transfers received by the worker, broken down by month and firm.

[20]While this figure may appear low, it has to be considered that the age distribution of UI benefits recipients peaks between age 30 and 38 and then progressively declines. In addition, workers with temporary contracts are an important component of the flows into unemployment (about 55%). Their exclusion leads to a large decline in the sample size, but allows us to focus on the most informative sub-population for manipulation. Indeed, even for the age group under study (46-54 years of age), they still represent about 50% of total UI recipients.

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

[22]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 Euro in the three months preceding the layoff.

firms laying off the workers claiming UI are on permanent contracts and about 22% are white collars. Managers are relatively rare and only 0.2% of UI claims origin from firms that have at least one manager. We can also notice that absences due to sickness, work accidents, or related to a disability of the worker herself or a relative, as well as child leave in the four years before the layoff are infrequent.[23] We restrict our attention to absences at the separating firm, since this is what the firm would be able to observe and use to determine whether to allow manipulation. For each type of absence, we compute the total transfers received by the worker and then normalize it by the average monthly wage in the three months before the layoff. Generally, absences due to sickness are more frequent, with workers receiving an average of 0.27 months of wages over four years, while work accidents generate about 0.063 months of wage transfers. Other events are even less frequent; only a few workers receive transfers for their own disability (0.001 months of wage transfers), while transfers due to the disability of relatives are slightly more common (0.021 months of wage transfers). Finally, child leave transfers concern only a minority of cases, with only 0.011 months of transfers over the four years before the layoff. In terms of past careers, workers were employed for about 84 months on average in the ten years before the UI spell and earned about 125,000 euros.

A comparison of the full sample with the restricted sample reveals several interesting differences. First, workers who separate from a permanent contract have a substantially higher risk of long term nonemployment: they spend about 90 weeks on average in nonemployment and 52% of them are still in nonemployment after 12 months (while this share is about 39% for the full sample). A larger fraction of them is male (68.9%) and more likely to live in the South (46% vs 39% in the full sample). In terms of job characteristics the two samples show remarkable similarities, but workers coming from permanent contracts have higher tenure on average (almost 6 years) and come from smaller firms (average firm size is 28 workers, as opposed to 85 in the full sample). Firms laying off workers on permanent contracts have a much larger share of permanent contracts (93%), while they have a similar share of white-collar workers (23%) and managers (0.2%). Similar differences emerge when we consider transfers for absences from work. Workers who were laid off from permanent contracts received larger amounts for absences of all types, ranging from almost double for sickness leave to 50% more for other types of transfers. This can be explained both by the fact that workers on permanent contracts had spent more time in the firm laying them off, as evidenced by the higher tenure, and by the fact that their greater job security may lead them to take more time off for sickness and other events (Ichino and Riphahn, 2005). In addition, workers on permanent contracts also spend more time employed (about six more months over ten years compared to the average for the full sample) and earn 14,000 Euro more over the ten years before the layoff.

Because our sample contains workers in their late forties and early fifties, one might be

[23]Transfers are winsorized at the top 0.1% of the distribution to remove the influence of a few outliers.

concerned that transitions into retirement could play a non-negligible role. However, this is not the case with only about 1,500 or 0.6% of workers in our restricted sample claiming retirement benefits before the end of our observation window (4 years since layoff).[24] We now turn to a description of our objects of interest and identification strategy.

3 Empirical Strategy

This section sketches our empirical strategy and explains the sources of variation in the data that we use to pin down different parameters of interest. The main idea is to exploit the local nature of manipulation by extrapolating outcomes from regions that are unaffected by it, to learn about what would have happened in a counterfactual world without manipulation. 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 inwards 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 but are affected by manipulation, such as subsequent benefit receipt or nonemployment survival probabilities. Intuitively, 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 and "untreated" outcomes. Under plausible assumptions, we also recover the response of non-manipulators, i.e. individuals who were laid off just before their fiftieth birthday. The "untreated" outcomes for this group are instead observed. Our approach is closely related to that of Diamond and Persson (2016) and Kowalski (2021).

3.1 Quantifying manipulation

Consider a hypothetical manipulated layoff density as in Figure 1a. 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 of layoffs right before age fifty and a spike right after it. We refer to the first region as the "missing" and the latter the "excess" region, which together make up the "manipulation" region. 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. 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 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. We provide supporting evidence that this is indeed the case in Section 4.6.

[24]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.

We operationalize this identification strategy following standard bunching techniques, e.g. Saez (2010), Chetty et al. (2011), Kleven and Waseem (2013). First, we group all layoffs into two-week bins based on the workers' age at layoff. Second, we determine the lower bound of the missing region z_L by visual inspection, in our case three bins (six weeks). Last, we iteratively try different upper bounds for the excess region z_U until we balance the missing and excess "mass", that is, the estimated number of manipulators on either side of the threshold. We estimate the number of manipulators by fitting a second order polynomial to the observed layoff frequency, including a full set of dummies for bins in the manipulation region, and retrieving the relevant regression coefficients. In practice, we estimate the following specification:

$$c_j = \alpha + \sum_{p=0}^{P} \beta_p \cdot a_j^p + \sum_{k=z_L}^{z_U} \gamma_k \cdot \mathbb{I}[a_j = k] + \nu_j, \tag{1}$$

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. The parameters γ_k recover the differences between the observed data and the counterfactual frequency in the manipulation region $[z_L, z_U]$. Using hat-notation to denote OLS regression coefficients, our estimate for the number of manipulators in the missing and excess region, respectively, is given by:

$$N_{\text{mani}}^{\text{missing}} = \sum_{k \in \text{missing}} |\hat{\gamma}_k| \quad \text{and} \quad N_{\text{mani}}^{\text{excess}} = \sum_{k \in \text{missing}} \hat{\gamma}_k. \tag{2}$$

Note that $\hat{\gamma}_k < 0$ if k belongs to the missing region, while $\hat{\gamma}_k > 0$ if it belongs to the excess region. We repeat the above procedure for different values of z_U until $N_{\text{mani}}^{\text{missing}} \approx N_{\text{mani}}^{\text{excess}}$. In our application we estimate a manipulation region consisting of three bins (six weeks) for the missing and two bins (four weeks) for the excess region.

Because they will be useful in the next steps, let us define estimates for the number of non-manipulators, which is an observable quantity, and the number of individuals in the excess regions who are not manipulators, respectively, as:

$$N_{\text{non-mani}}^{\text{missing}} = \sum_{k \in \text{missing}} c_k \quad \text{and} \quad N_{\text{w/o mani}}^{\text{excess}} = \sum_{k \in \text{excess}} c_k - \hat{\gamma}_k. \tag{3}$$

Note that we deliberately reserve the term "non-manipulator" for individuals in the missing region who, 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:

$$s^{\text{missing}} = \frac{N_{\text{mani}}^{\text{missing}}}{N_{\text{mani}}^{\text{missing}} + N_{\text{non-mani}}^{\text{missing}}} \quad \text{and} \quad s^{\text{excess}} = \frac{N_{\text{mani}}^{\text{excess}}}{N_{\text{mani}}^{\text{excess}} + N_{\text{w/o mani}}^{\text{excess}}}. \tag{4}$$

Analogously, we define the share of manipulators in age bin k by:

$$s_k^{\text{missing}} = \frac{|\hat{\gamma}_k|}{|\hat{\gamma}_k| + c_k} \text{ for } k \in \text{missing} \quad \text{and} \quad s_k^{\text{excess}} = \frac{\hat{\gamma}_k}{c_k} \text{ for } k \in \text{excess}. \tag{5}$$

Equipped with a measure of the size of manipulation, we now turn to studying affected outcomes.

3.2 Effects of manipulation

This section outlines our empirical strategy for studying outcome variables that are not directly manipulated but could potentially be affected by manipulation. Figure 1b 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 1b. 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 1b, we obtain these quantities by separately studying the missing and excess region. First, we fit a flexible counterfactual on the right-hand 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-hand side of the threshold.

In practice, we start by running the following regression on individual-level data:

$$y_i = \alpha + \sum_{p=1}^{P} \beta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i \leq 50] + \sum_{p=0}^{P} \beta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i > 50] + \\ + \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \xi_i, \quad (6)$$

where y_i is 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 estimated 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 (6) allows for a treatment effect of longer PBD on outcomes, i.e. $\beta_0^{>50}$. We refer to $\beta_0^{>50}$ as the "donut" regression discontinuity (RD) coefficient. This coefficient captures the treatment effect of four additional months of PBD for the average individual in the population, as in Barreca et al. (2011) and Scrutinio (2018).[25] We use it to benchmark our results for the response of manipulators (more on this below). Graphically, $\beta_0^{>50}$ recovers the difference between the two grey dots in Figure 1b.

The central idea of our estimation strategy is the re-scaling of the estimated differences ($\hat{\delta}_k$) by the respective share of manipulators. Formally, let Y denote our outcome of interest and $\bar{Y}_l^j$ its average over individuals l in region j. For each bin k in the missing region, we may calculate the difference in average outcomes between manipulators and non-manipulators as:[26]

$$\bar{Y}_{\text{non-mani},k}^{\text{missing}} - \bar{Y}_{\text{mani},k}^{\text{missing}} = \frac{\hat{\delta}_k}{s_k^{\text{missing}}}. \quad (7)$$

Note that the average outcome of non-manipulators in bin k is observable and given by

[25] Alternatively one could derive bounds on the average treatment effect following the method of Gerard et al. (2020). Because manipulation is clearly visible and locally confined in our setting we use a "donut" regression discontinuity design.

[26] Indeed, we can write the coefficient $\hat{\delta}_k$ as:

$$\hat{\delta}_k = \bar{Y}_{\text{non-mani},k}^{\text{missing}} - \left(s_k \bar{Y}_{\text{mani},k}^{\text{missing}} + (1 - s_k) \bar{Y}_{\text{non-mani},k}^{\text{missing}} \right)$$

which after some rearrangement leads to our equation 7.

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k}, \tag{8}$$

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

$$\bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} - \frac{\hat{\delta}_k}{s^{\text{missing}}_k} \tag{9}$$

and manipulators average counterfactual outcome over the entire missing region as

$$\bar{Y}^{\text{missing}}_{\text{mani}} = \frac{1}{N^{\text{missing}}_{\text{mani}}} \sum_k |\hat{\gamma}_k| \cdot \bar{Y}^{\text{missing}}_{\text{mani},k}. \tag{10}$$

The logic behind this re-scaling is straightforward: if we found that the absence of 10% of individuals in the missing region, namely the manipulators, 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

$$\bar{Y}^{\text{excess}}_{\text{mani},k} - \bar{Y}^{\text{excess}}_{\text{w/o mani},k} = \frac{\hat{\delta}_k}{s^{\text{excess}}_k}. \tag{11}$$

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

$$\bar{Y}^{\text{excess}}_{\text{observed},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} = \frac{\hat{\gamma}_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k} + (c_k - \hat{\gamma}_k) \cdot \bar{Y}^{\text{excess}}_{\text{w/o mani},k}}{c_k}. \tag{12}$$

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

$$\bar{Y}^{\text{excess}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} + (1 - s^{\text{excess}}_k) \cdot \frac{\hat{\delta}_k}{s^{\text{excess}}_k}, \tag{13}$$

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

$$\bar{Y}^{\text{excess}}_{\text{mani}} = \frac{1}{N^{\text{excess}}_{\text{mani}}} \cdot \sum_k \hat{\gamma}_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k}, \tag{14}$$

which, together with equation (10) lets us define manipulators' moral hazard response (or treatment effect) as

$$Y_{\text{mani}}^{TE} \equiv \bar{Y}_{\text{mani}}^{\text{excess}} - \bar{Y}_{\text{mani}}^{\text{missing}}. \tag{15}$$

Consistently with the theoretical and empirical literature on optimal social insurance, our definition of moral hazard response only encompasses *changes* in outcomes that are caused by longer UI (Chetty, 2006; Chetty et al., 2011). Note that this strategy identifies the average response of a manipulator without recovering by how many weeks each individual manipulator delayed their layoff.

The procedure illustrated in Figure 1b also lets us study selection into manipulation. This can be done by comparing manipulators' counterfactual outcomes to non-manipulators realized outcomes, both of which are "untreated", i.e. occur under the shorter 8-month scheme. Our comparison is very similar in nature to the "untreated outcome test" by (Kowalski, 2021), who compares compliers' counterfactual and never takers' realized outcomes in an instrumental variable framework.[27]

Concretely, notice that non-manipulators' untreated outcome in the missing region $\bar{Y}_{\text{non-mani}}^{\text{missing}}$ is observed (equation 8), while manipulators' untreated outcome $\bar{Y}_{\text{mani}}^{\text{missing}}$ was obtained by recognizing that the counterfactual outcome in the missing region is a weighted average of non-manipulators' untreated outcomes and manipulators' untreated outcomes (equations 9 and 10). Given that we know the share of manipulators in the missing region, we can easily compute the difference between manipulators' and non-manipulators' untreated outcomes. Thus, we have

$$Y_{\text{mani/non-mani}}^{\text{untreated selection}} \equiv \bar{Y}_{\text{mani}}^{\text{missing}} - \bar{Y}_{\text{non-mani}}^{\text{missing}}. \tag{16}$$

If this difference is non zero, then manipulators select into manipulation based on their baseline nonemployment risk. Figure 1b 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 empirically in Section 4.4.

In the paper we abstract from the exact microfoundation behind individuals' high baseline nonemployment risk, which could be due to intrinsic worker types, but also to optimal *choices*. For example baseline nonemployment risk could be equally high for a wealthy worker who is less attached to the labor market and decides not to look for a job and for a disadvantaged unskilled worker. In Section 5 we show that baseline

[27]The analogy with the instrumental variable framework studied in Kowalski (2021) goes as follows: in our setting the instrument is the availability of manipulation opportunities, which is switched on in the observed data and switched off in the counterfactual data that is reconstructed through the polynomial. Non-manipulators can be seen as "never takers", as they do not obtain longer coverage neither when manipulation is allowed nor when it is "forbidden". On the other hand, manipulators can be seen as "compliers", because they are induced by manipulation possibilities to switch treatment status and obtain four months of extra coverage.

nonemployment risk is sufficient for studying the welfare effects of local policy changes, regardless of the underlying primitives – even when these reflect choices. Choices occurring at baseline, are conceptually different from the moral hazard *response* defined in equation 15, as they occur *regardless* of how much PBD workers receive.

3.3 Recovering Responses of Non-manipulators

Having obtained an estimate of manipulators' response, we can benchmark it against the implied response of non-manipulators. As noted above, $\hat{\beta}_0^{>50}$ is 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, instead of through manipulation, we can decompose the response for the average individual as follows:

$$s^{\text{missing}} \cdot Y_{\text{mani}}^{TE} + (1 - s^{\text{missing}}) \cdot Y_{\text{non-mani}}^{TE} = \hat{\beta}_0^{>50}. \tag{17}$$

A fraction of s^{missing} of the estimated jump in the polynomial $\hat{\beta}_0^{>50}$ is due to the response of manipulators, the remaining $(1 - s^{\text{missing}})$ has to be due to the response of non-manipulators. Rearranging thus gives us an estimate for non-manipulators' response:

$$Y_{\text{non-mani}}^{TE} = \frac{\hat{\beta}_0^{>50} - s^{\text{missing}} \cdot Y_{\text{mani}}^{TE}}{1 - s^{\text{missing}}}. \tag{18}$$

3.4 The Cost of Providing Longer Potential Benefit Duration

To capture the efficiency cost of longer PBD for manipulators and non-manipulators separately, we rely on the BC/MC ratio, a standard measure of the disincentive or moral hazard effect of UI (Schmieder et al., 2012). This measure relates the size of fiscal externalities that are due to changes in job search behaviour (behavioral cost, BC) to the cost arising from providing longer coverage without changes in search behaviour (mechanical cost, MC). Intuitively, statutory coverage may mechanically lead to higher benefit receipts if individuals are unemployed during the additional months with or without the extra coverage. This cost increase for the government is not due to distorted job search incentives but simply reflects nonzero exhaustion risks during the relevant months of nonemployment.

More formally, the behavioral to mechanical cost ratio for individual i induced by a marginal increase in PBD P is:

$$\frac{BC_P^i}{MC_P^i} = \frac{b \cdot \int_0^P \frac{dS_t^i}{dP} dt + \tau \cdot \int_0^T \frac{dS_t^i}{dP} dt}{b \cdot S_P^i}. \tag{19}$$

where b is the benefit amount and τ is a lump-sum tax needed to finance UI. Note that unemployment benefits are collected up to the horizon P corresponding to the PBD, while taxes are collected throughout the observation period T. S_t^i is the survival probability in nonemployment at time t for individual i, S_P represents the survival probability in nonemployment in the last period of UI coverage under the baseline PBD P, and the derivative $\frac{dS_t}{dP}$ indicates by how much the survival probability at horizon t changes due to a marginal increase in PBD (P). The above BC/MC ratio has a classical leaking bucket interpretation. It captures by how many additional dollars total government expenditure goes up for each dollar of mechanical transfer from the government to the unemployed. BC/MC ratios allow inter-group comparisons of the moral hazard cost of providing insurance. This is especially important because individuals might have heterogeneous exhaustion risk regardless of coverage length and thus face different incentives to respond to PBD extensions. As in previous work, it turns out that it is precisely this re-scaled moral hazard effect that is relevant for optimal policy in our setting, something which we highlight in Section 5.

We graphically illustrate the components of the BC/MC ratio for our setting in Figure 2 and refer to it simply as moral hazard response throughout. In the Figure we plot two hypothetical survival curves for manipulators, one for when PBD is equal to 8 months (solid line), and one for when PBD is equal to 12 months (dashed line). Under the assumption that longer PBD decreases job search effort, the second survival curve lies above the first. The mechanical cost (MC) of increasing PBD from 8 to 12 months is represented by the light-shaded area beneath the solid survival curve between months 8 and 12 of nonemployment. MC arises from covering four extra months with positive nonemployment risk, with no change in behavior. The behavioral cost (BC) of increasing PBD from 8 to 12 months corresponds to the dark-shaded area between the solid and dashed survival curves between months 0 and 12 of nonemployment. BC arises from changes in the search behaviour along the entire period during which UI is paid. After having described the main elements of our analysis, we now turn to our empirical findings.

4 Results

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 nonemployment duration that arises from the change in manipulators' job search behavior that is caused by longer coverage (moral hazard response). We highlight that most of the increase is mechanically the result of higher coverage due to relatively high long-term nonemployment risk on which manipulators are adversely selected. The implied responsiveness to UI is modest and, in particular, not higher than for non-manipulators. Lastly, we probe the robustness of our findings and examine observable characteristics on which manipulators are selected.

4.1 Evidence of manipulation

To provide graphical evidence of manipulation, Figure 3 plots the relative frequency of layoffs against workers' age at layoff for the full sample of workers in the private sector, including all workers laid off in a four-year window around the age-fifty threshold. The Figure shows a clear decline in the density in the weeks just before workers' fiftieth birthday and a corresponding increase in the weeks following it.

In Figure 4 we then split the full sample of private sector workers in two groups depending on the type of contract they had *before* layoff: permanent or temporary. As for workers employed on permanent contracts, we can clearly notice that the density of layoffs (reported in Figure 4a) drops just before, and spikes after, the age-fifty threshold. This pattern is consistent with a strategic re-timing of the layoff in order to obtain a longer PBD.[28] The corresponding figure for temporary contracts (Figure 4b) does not reveal any evidence of strategic behavior. Since workers previously on temporary contracts would not provide useful information to assess the drivers and the impact of manipulation, we confine our analysis to workers on a permanent contract before layoff. Notice that this restriction concerns only the type of contract the worker is on when laid off, while we do not place any restriction on the type of contract at reemployment.

Two main reasons may explain why we do not observe manipulation among workers with temporary contracts. First, the Italian legislation generally prevents employers to lay off workers before contract termination. Hence, it would be very difficult for an employer to hire a worker with a temporary contract with a longer contract duration and then lay the worker off as soon as the worker is eligible for the longer PBD. This implies that workers should strategically delay their starting date or look for a contract with a long enough duration to go beyond the age threshold, which may be costlier. Second, workers on temporary contracts find a job much *earlier* than workers on permanent contracts on average. Hence, they would not need additional PBD as much. We provide additional details and discussion on this in Appendix B.

We also repeat the same analysis for workers coming from the public sector in Figure A1

[28]It is unlikely that observed manipulation patterns are the results of differential take-up in UI. If differential take-up was occurring, it would cause an upward shift in the *entire* distribution, rather than the observed pattern of manipulation near the age 50 cutoff. However, Figures 4a, 5, and the tests in Section F.1 of the Appendix show no evidence of such a shift, ruling out differential take-up is the driver of the results.

in the Appendix. Only a minority of workers are laid off from permanent positions in the public sector while a larger number of separations comes from temporary contracts.[29] In neither case, we see any evidence of strategic delays of layoffs.

Figure 5 further describes the frequency of layoff for workers aged between 26 and 64, working in the private sector and from permanent contracts. The absence of missing and excess mass at round ages (30 and 40 years of age) rules 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.[30]

Following our estimation strategy outlined in Section 3.1, we find the manipulation region to consist of all age 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. 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.

The highly localized nature of manipulation suggests that allowing it may be quite costly or offer minimal benefits to firms, which have full discretion over the timing of layoffs. Although we lack direct evidence on the costs of manipulation, the degree observed here is consistent with other studies. Khoury (2023) finds that 10% of layoffs in a 4-week window of a tenure threshold in France are strategically delayed. Huang and Yang (2021) study a different policy where the government rebates 50% of the remaining UI upon job finding if the worker is laid off after age 45. While they do not quantify the share of manipulators explicitly, they find manipulation to be very local and concentrated in the 2 weeks preceding the layoff. Le Barbanchon et al. (2019) study an increase in PBD at the age-fifty threshold, whereby workers get 30% longer PBD on average. They show that the density is 8% higher at the cutoff. Schmieder et al. (2012) look at age cutoffs in Germany and find manipulation regions to be confined to two weeks and bunching is small in magnitude. Other studies have found larger responses in tenure at layoff when incentives are very large. For example Baguelin and Remillon (2014) study a setting where workers laid off after age 60 obtain unlimited PBD up until their retirement.

[29] The school sector plays an important role in this case, as testified by the share of workers in this group going into unemployment in June, about 56.5% of the total number of workers receiving UI from temporary contracts from the public sector (and an additional 17.5% in July and August).

[30] Section 4.6 presents additional robustness checks.

Such increases of PBD of *multiple years* generates delays in the timing of layoffs just up to four months. In Section 4.5, we characterize manipulators based on observable characteristics and examine the economic mechanisms that may facilitate or constrain manipulation. Additionally, we explore patterns in the literature to understand why evidence of manipulation emerges in some settings but not in others.

4.2 Effects of manipulation: UI benefit receipt and duration

Manipulation provides workers with four additional months of UI coverage. To study the effect of extra coverage on manipulators' benefit receipt and nonemployment duration we begin by plotting these outcomes against workers' age at layoff in Figure 6. For each outcome we see visible changes around the age threshold indicating that both respond to manipulation. As outlined in Section 3.2, we combine these changes with the share estimate from the previous section to retrieve manipulators' as well as non-manipulators' responses. We report all estimates with associated bootstrapped 95% confidence intervals in Tables 3 and 4.[31]

Our results indicate that manipulators would have collected 5,814.2 Euro, and spent 27.8 weeks on UI benefits, had they not manipulated (column 1). Through manipulation these numbers increase to 8053.6 Euro and 41.8 weeks (column 3), resulting in an additional cost of 2,239 Euro per manipulator (column 5). In order to benchmark these estimates, we compute the same numbers for non-manipulators following the strategy outlined in Section 3.3. We find that non-manipulators generate a total cost of 1,636.9 Euro (column 6) when receiving additional coverage.

As highlighted in Section 3.4, these numbers alone are not directly welfare relevant, because they reflect both the mechanical transfer as well as possible distortions in job search caused by longer coverage. The next section provides a decomposition into these two components.

4.3 Distinguishing behavioral responses from mechanical effects

To decompose behavioral and mechanical cost increases associated with manipulation, we repeat the preceding estimation using survival rates in nonemployment at different months after layoff as an outcome. This allows to trace out *when* manipulators and non-manipulators respond to additional coverage.

We start by plotting nonemployment survival rates against age at layoff at various months after layoff in Figure 7. Qualitatively, we observe bigger jumps around the thresholds precisely during the months with extra coverage. Similarly to before, we

[31] All confidence intervals in the paper are obtained by simple non-parametric bootstrapping: we operationalize this by resampling UI events and re-estimating the entire procedure, including the share of manipulators, 5000 times.

combine these changes with the estimated share of manipulators causing them, to trace out monthly survival curves for both manipulators and non-manipulators.

Figure 8a presents our estimated nonemployment survival curves of manipulators under the eight and twelve months PBD schemes. Figure 8b reports the difference between the two curves at any point, with associated bootstrapped 95% confidence intervals. The difference between the two curves reveals the effect of longer PBD along manipulators' survival curve which appears concentrated precisely in the months of extra UI coverage. We replicate the same analysis for non-manipulators and report its findings in Figure 9. The qualitative picture is similar, although confidence bands are much narrower in large part due to the fact that non-manipulators' survival curve under the eight month PBD scheme is observable rather than estimated.

We translate the survival rate responses into BC/MC ratio estimates for manipulators and non-manipulators following equation (19). To do so, we rely on numerical integration and weight responses by statutory benefit rates.[32] We report our BC/MC ratio estimates in Table 5. Because there is some disagreement in the literature as to what the appropriate tax rate is this context, columns 1 and 2 provide BC/MC ratios for a no tax $\tau = 0$ and a commonly used UI tax of $\tau = 3\%$, see e.g. Schmieder and von Wachter (2016) and Lawson (2017). As discussed in Section 3.4 an estimate of 0.24 for manipulators in column 1 of Table 5 implies that the government pays an additional 24 cents for each Euro of UI transfer. The estimated BC/MC ratios for manipulators and non-manipulators are strikingly similar suggesting that there is no selection on moral hazard responses. From a positive perspective this finding also mitigates concerns that an anticipated moral hazard response is a prime motive to engage in manipulation. Interestingly, the reported BC/MC ratios are in the lower range of estimates in the previous literature, see Schmieder and von Wachter (2016) for an overview.

As a final remark, notice that the BC/MC ratios that we estimate will also include the fiscal externalities deriving from workers finding irregular jobs rather than regular jobs. This is not a source of bias, nor it alters the conclusions of the paper. What matters for computing the effective moral hazard cost is the (rescaled) fiscal externality associated with longer UI, regardless of whether this occurs because people stay in nonemployment or because people find informal jobs that pay no taxes, as shown in Gerard and Gonzaga (2021). While interesting in their own right, the BC/MC ratios that we estimate in the paper still measure the effective moral hazard cost and are still the sufficient statistics for local policy recommendations.[33]

[32]We perform integration using the midpoint rule and impose a non-negativity constraint on the behavioral cost at any point in time. Note that in the first few months the point estimates of the survival rate response is negative for manipulators which would imply that longer PBD increases job finding rates. However, this finding is likely due to noise. As these negative contributions to the overall integral leads us to underestimate BC/MC ratios for manipulators, our estimates are conservative. Results are qualitatively unaltered without imposing the non-negativity constraint.

[33]Using the words of the authors "one does not need to separate informality and nonemployment

4.4 Selection on long-term nonemployment risk

The remainder of our empirical analysis provides additional evidence to shed light on the drivers behind manipulation in our context. The previous section mitigated concerns that anticipated moral hazard responses are a key motivation to engage in manipulation. In this section we show that exhaustion risk is a strong predictor of manipulation.

To do so, Figure 10 combines manipulators' and non-manipulators' eight months PBD survival curves from Section 4.3. A clear difference emerges: manipulators exhibit an almost 20 p.p. higher (counterfactual) exhaustion risk under the less generous eight months PBD scheme. This finding provides compelling evidence that anticipated exhaustion risk is a strong motive for manipulation. The large exhaustion risk is also (partly) responsible for making most of the increase in benefit receipt mechanical, thus lowering the BC/MC ratio in Section 4.3.

The fact that manipulators are selected on long-term nonemployment risk speaks, though indirectly, to the asymmetric information problems that could arise in a hypothetical market where individuals insure against nonemployment duration at the moment of layoff, which has been untested so far. In this context, we reveal significant adverse selection on baseline long-term unemployment risk, but no selection on moral hazard responses.

Two caveats are worth mentioning. First, our findings are based on variation around the fifty-year threshold, while no variation is available at younger ages. This concern could be minor as prior studies have not found significant differences in individual abilities to predict unemployment risk by age (Hendren, 2017).

Second, our selection patterns are not driven by explicit prices, nor we are able to observe manipulation costs. While this presents a challenge, future research could explore methods to indirectly infer "prices" from manipulation patterns or examine alternative settings where explicit prices are observed. Following the methods in Landais et al. (2021), this would allow researchers to bound the value of insurance even in the absence of consumption data. Given these caveats, it is hard to provide explicit policy messages as to whether choice is preferable with respect to a universal mandate, as it is hard to determine whether a choice-based UI system would be unsustainable due to market unravelling. Despite these limitations, our findings provide a deeper understanding of selection dynamics in UI.

4.5 Characterizing manipulators

In this final section of our empirical analysis, we provide suggestive evidence regarding the mechanisms behind manipulation. First, we identify observable characteristics that

responses" (Gerard and Gonzaga, 2021, p. 169).

correlate with manipulation in our data. Second, we review the literature to explore why manipulation may be more prevalent in some contexts than in others.

Selection on observables. We already showed in Section 4.1 that manipulation is confined to individuals employed in the private sector on permanent contracts, which motivated the choice of our main sample. Turning to observable worker and firm characteristics for our main sample, Table 6 reports a selection on observables analysis.[34] Column 1 and 2 report estimated mean characteristics for manipulators and non-manipulators, respectively. Column 3 calculates their difference together with bootstrapped 95% confidence intervals. Some differences immediately stand out. 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. Manipulators' wages are 6% lower, although this latter estimate is relatively imprecise. These findings would imply a lack of rent sharing on the job, while leaving open the possibility of rent sharing through bribes; while we lack direct evidence, it remains theoretically plausible and financially feasible.[35] Firm size also plays an important role for manipulation: manipulators come from firms that are about 40% smaller. Firms laying off manipulators also appear to a better contract composition: indeed, they have a slightly higher, but not significantly different, share of permanent contracts, which is generally very high for these firms (95% in firms laying off manipulators vs 93% for non manipulators) and a higher share of white collars (about 35% in firms laying off manipulators with respect to 21% for firms laying off non manipulators). Finally, both groups of firms show a similar presence of workers classified as managers. Overall, these findings seem to suggest that adjustment costs, bargaining power and proximity to managers play a role in workers' ability to engage in manipulation.

There is an important question as to whether firms are able to identify workers with a higher long term nonemployment risk or whether workers explicitly reveal private information to their employers because there is a tangible benefit from doing so.

Mueller and Spinnewijn (2023) shows that recent employment and unemployment histories are good predictors of long-term unemployment risk. If firms were aware of this, they could utilize observable data to make strategic decisions about which workers to support. We do not find large differences in the career of manipulators and non-manipulators over then 10 years prior to layoff: manipulators worked 5 more

[34] The analysis closely follows Section 6.2 of Diamond and Persson (2016).

[35] The additional unemployment insurance (UI) collected by manipulators is 2,239 Euro on average. In the most extreme case, a firm would pay additional wages equal to 61 Euro per day over the six weeks in the missing region, totaling around 2,196 Euro. This suggests that, in extreme cases, the extra wages could nearly offset the UI payments. However, most manipulators are closer to the cutoff, resulting in smaller payments of about 732 or 1,460 Euro for delays of 2 or 4 weeks, respectively. Thus, it seems plausible that workers could fully compensate the employer for the extra employment days, although this assumes no productivity from the match.

months than non manipulators (93 months, 88 for non-manipulators) but still earned slightly less (about 900 Euro less, out of about 136,600 Euro for non manipulators). This would point to the fact that workers are communicating *residual* private information to firms in order to gain advantages.

In order to enrich our analysis, we also considered covariates related to absences from work. Thus we check whether manipulators received more transfers related to events such as sickness, work accidents, disability of the worker or of a relative/dependent and leave for a sick children in the four years preceding the layoff (Table 6).[36,37]

Generally, all of these events are relatively rare at this age. Overall, the evidence suggests that manipulators exhibit greater signs of vulnerability compared to non-manipulators: they received more transfers related to work accidents, disability, disability of a relative and for child leave. Unfortunately, the estimates are noisy and we are able find a statistically significant difference between the two groups only in the case of child leave, which encompasses both absences for childbirth and for sickness of children. This latter finding aligns with the higher proportion of women among manipulators. Overall, these results suggest that the firm might be using some observable factors, not accounted for by policymakers, to provide additional benefits to workers who require extra insurance.

In conclusion, traditional measures from matched employer-employee data, such as work histories and past earnings, might suggest that workers are conveying residual private information to firms. However, the detailed nature of our data allows us to examine less conventional outcomes, such as transfers for accidents, disability, and other absences. These data indicate that differences between manipulators and non-manipulators may stem from *health*-related factors typically unobserved in standard datasets. This represents a considerable improvement over previous literature on manipulation, which could only rely on labor-related variables. We hope that future research with more detailed information on these dimensions will shed further light on their role and how they might be used to improve worker screening the UI setting.

Literature review. One remaining question is why some of the previous studies that use similar variation in PBD and for similar age groups have not found comparable manipulation patterns (Schmieder et al., 2012; Schmieder and von Wachter, 2016, 2017). While we cannot provide definitive answers, a review of the relevant literature allows us to identify some common threads across various studies. Below, we argue that both the size of the economic incentive for workers, and the institutional setting play an important role.

[36]We only have information on sickness and work injury events leading to absences longer than seven days due to privacy reasons

[37]For each of these categories, we compute the cumulative transfers received by the worker within the firm over the four years leading up to the layoff and normalize this amount by the worker's average wage in the three months preceding the layoff. The resulting measures can thus be interpreted as the number of monthly salaries received by the worker due to these transfers.

For example, Table 2 of Schmieder et al. (2012) reports a treatment effect of longer PBD on benefit duration of about 1.4 months for 49 year old individuals. In our setting the same effect is 3.2 months for manipulators and 2.3 months for non-manipulators. Notice that these effects are made of both a mechanical and a behavioral effect. However by knowing BC/MC ratios in the two settings it is easy to recover what share of the total increase is mechanical and what is due to behavioral responses. Average BC/MC ratios are remakably similar in the German and Italian case and equal to 0.3 on average. Mechanical effects can be obtained by multiplying the total effect by $\frac{MC}{BC+MC} = \frac{1}{1+BC/MC} = \frac{1}{1.3} \approx 0.77$. It follows that the mechanical effect is roughly $1.4 \times 0.77 \approx 1.1$ in the German case and $3.2 \times 0.77 \approx 2.5$ in Italy. A similar reasoning applies to other age groups. A lower mechanical transfer could explain why workers at the threshold have lower incentives to manipulate in the German case. There are different reasons why the mechanical transfer can be lower in the German setting. One reason has to do with the levels of PBD: German 49 year old individuals face a PBD extension from 22 to 26 months (from 8 to 12 in the Italian case). Even if nothing else was different between Italy and Germany, it naturally follows that the population at risk of still being nonemployed at point of exhaustion is considerably lower in the German case, which lowers the mechanical transfer and the incentives to manipulate the date of layoff, *ceteris paribus*.

Another possibility that still relates to economic incentives has to do with age. Workers at age 50 or higher may be at higher risk of not returning to employment fast, and so have more incentive to manipulate. Nekoei and Weber (2017), Ahammer and Packham (2023) and Guglielminetti et al. (2024) all study the age-forty threshold in Austrian UI where workers become entitled to 39 instead of 30 weeks of benefits and find no evidence of manipulation. In this setting, no information on survival curves is provided so we cannot compute BC/MC ratios to perform a similar analysis as above. It is however possible to assess the incentives to manipulate based on average non-employment duration and responses to additional UI. Table 2 in Nekoei and Weber (2017) reports that on average individuals spend about 114 days (16.2 weeks) in nonemployment before finding a new job, which is substantially less compared to the 30 weeks that are granted to workers eligible to the shortest UI duration in the Austrian setting. This suggests that in general workers find a new job well before running out of the baseline UI and they have little incentives to manipulate in order to gain additional coverage, differently from our setting. In addition, workers show a very small response to longer UI, equal to 1.9 additional days of nonemployment (that is 3% of the additional coverage, substantially less with respect to change in search found, for example, in Schmieder et al. (2012), which range from 10 to 20%). This suggests that on average workers have little incentives to exert effort to be eligible to longer UI benefits. To the contrary, large PBD extensions for older workers induce strong responses. Jäger et al. (2023) finds that layoffs and quits respond strongly when PBD in Austria increases from 1 to 4 years, and workers use it as a bridge to retirement. This finding is also similar in Ahlqvist and Borén (2017) who finds strategic timing of UI claims as a bridge to retirement in Sweden.

Last but not least, in some settings there are particular rules that limit manipulation possibilities. For example Caliendo et al. (2013) study PBD extensions around an age-at-unemployment threshold in Germany. The authors report that workers must register at an unemployment office 3 months before the end of an employment contract and within 3 days of the advance layoff notice if the notice occurs later than 3 months before the date of layoff. This would leave little time for trying to engage the employer for manipulation. Also, they study a setting where UI jumps from 12 to 18 months and explicitly state that since many workers find a job within 12 months, it is unlikely that workers have big incentives to manipulate – which is similar to the point we make above when discussing the Schmieder et al. (2012) and Nekoei and Weber (2017) studies. In the same spirit, other studies using discontinuities in reform timing also found absence of manipulation (Johnston and Mas, 2018b; Garcia and Hansch, 2024), which points to the idea that limiting the time window that workers have at their disposal to act has an influence on manipulation opportunities.

There is a last point to be considered. In the German case what matters is age on the day of the UI claim rather than age-at-layoff. As noted by the authors, it is then surprising that no manipulation is found, as age-at-claim is completely in the workers' hands. The authors justify this by referring to some form of present-bias (liquidity constraints could also be at play) by which workers would easily trade-off receiving benefits *earlier* against the total potential benefit duration. In the Italian setting that we study there is no such trade-off, as workers receive wages until their fiftieth birthday and then go on to claim UI. If present bias or liquidity constraints are really a concern, then we would expect manipulation to be more likely in the Italian setting, which is what we see.

All of these explanations cannot exclude that firms value their reputation among workers and the community at large, nor that managers may benefit from warm glow. We hope that more research will help elucidate these important aspects about manipulation.

4.6 Robustness

This section probes the robustness of two identifying assumptions underlying our empirical analysis. First, we provide evidence that manipulation is indeed the result of additional UI coverage around the age at layoff threshold. Second, the empirical analysis assumes that the discontinuity in PBD around the age threshold affects layoff decisions in exactly one way, namely, through a delay in an otherwise earlier occurring layoff.

By plotting layoffs across the entire age distribution Figure 5 already ruled out several alternative explanations such as e.g. round birthday effects. To provide further supporting evidence Figure 11 plots layoff densities for two Italian UI schemes that replaced the OUB scheme after January 2013 (MiniASpI and NASpI) and did not feature any discontinuity in generosity at the age-fifty threshold.[38] Reassuringly, we do not find any discontinuity at the age-fifty threshold under either of the these alternative schemes[39]

The second concern is related to the possible presence of *extensive margin* job separation effects of UI and merits special attention in the light of recent evidence by Albanese et al. (2020) and Jäger et al. (2023). The former documents layoff responses at the eligibility threshold (52 weeks of contributions) in the same Italian OUB scheme we study. Although theoretically possible, we find no empirical evidence of any extensive margin job separation responses in our context through a series of robustness tests presented in detail in Appendix F. Intuitively, the layoff density shown in Figure 3, shows no indication of any additional layoffs to the right of the cutoff that are not explained by missing layoffs in the missing region. We discuss this point as well as a series of other robustness tests exhaustively in Appendix F and find no evidence for a violation of our identification assumption.

5 Selection in Optimal Targeted Social Insurance

The empirical results described thus far are not only interesting from a positive perspective but also have theoretical and policy relevance. To see this, the following section lays out a simple model for the design of optimal differentiated social insurance in the presence of manipulation opportunities. By explicitly linking manipulation patterns to fiscal externalities, we demonstrate that manipulation responses carry significant welfare implications. Our main proposition shows that the welfare effect of local policy changes can be expressed as a function of key "sufficient statistics" (Chetty, 2006). We stay deliberately close to our empirical setting to facilitate the connection between the theoretical and empirical part of the paper. Although the model is derived in the context of unemployment insurance duration, our results readily extend to other social insurance settings. We present only the main insight and refer to Appendix C for the full model setup and derivation.

From the model, we derive both positive and normative conclusions. From a positive perspective, the model shows that manipulation is welfare improving as long as manipulators are (positively) selected on their consumption smoothing gain, but this improvement is mitigated depending on how much their are selected on their effective moral hazard cost. From a normative perspective, a full empirical implementation of our main proposition would reveal whether or not further differentiation w.r.t. to a *given* tag – inclusive of manipulation – has any potential benefit or if the optimal policy

[38]For institutional details regarding both UI schemes see Appendix E.

[39]We use the density tests proposed by Cattaneo et al. (2020) and implement them through the `rddensity` command in Stata. The p-values of the corresponding density tests are 0.23 for the MiniASpI and 0.133 for the NASpI, respectively.

is in fact undifferentiated.

Of course the policymaker could always try to solve the information asymmetry *directly*, by choosing more informative tags. We come back to this point and discuss it at the end of this section.

5.1 The Model

5.1.1 Setting

We assume there are two groups of individuals, referred to as the "young" and the "old". Young and old individuals differ in their utility of consumption, job search costs and their ability to manipulate. The government provides unemployment benefits b, financed through a lump-sum UI tax τ. The government sets two separate UI schemes of varying generosity characterized by two different potential benefit durations P_y and P_o, with $P_o \geq P_y$. It targets the longer potential UI benefit duration P_o to the old. When doing so it faces a challenge: young individuals have the ability to manipulate their eligibility status (at some cost) and might endogenously select into the more generous scheme intended for the old.

5.2 Characterizing Optimal Policies

In Appendix C we set up the formal job search problem for the old and the young and then solve the planner's maximization problem. We derive the following proposition characterizing the optimal policy (under some simplifying assumptions).

Proposition 1. *Compared to the optimum without manipulation opportunities, the optimal policy under manipulation prescribes under-insuring or over-insuring the young by a factor that depends on (i) the responsiveness of manipulation to baseline and extra coverage (ii) manipulators' selection on risk, consumption smoothing value and moral hazard costs. Formally, the optimal policy satisfies:*

$$\frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} - \frac{BC^y}{MC^y} = \underbrace{\epsilon_{1-M,\Delta P}}_{\substack{\textit{manipulation externality} \\ \textit{of extra coverage}}} - \underbrace{\epsilon_{1-M,P}}_{\substack{\textit{manipulation externality} \\ \textit{of baseline coverage}}} + M \cdot \underbrace{\left(\frac{S^m_{P_y}}{S^y_{P_y}}\right)}_{\substack{\textit{selection on risk} \\ \textit{scale factor}}} \cdot \left\{ \underbrace{\left(\frac{\tilde{u}'_m - \tilde{u}'_n}{\bar{u}'}\right)}_{\substack{\textit{selection on consumption} \\ \textit{smoothing value}}} - \underbrace{\left(\frac{BC^m}{MC^m} - \frac{BC^n}{MC^n}\right)}_{\substack{\textit{selection on moral} \\ \textit{hazard cost}}} \right\}, \tag{20}$$

where $\frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} - \frac{BC^y}{MC^y}$ is the gap between the young's consumption smoothing value from longer UI and the young's behavioral to mechanical cost ratio. If this gap is positive (negative), the planner is willing to under-insure (over-insure) the young, compared to a world with no manipulation opportunities. In order to build intuition for the sign and degree of over or under-insurance, let's consider the following special cases in turn:

First, consider the case without manipulation, i.e. $M = 0$ for all policies. In this case the right-hand side of equation 20 equals zero and the optimal policy nests the standard Baily-Chetty formula for optimal unemployment insurance, see e.g. Schmieder et al. (2012): the optimal policy equates marginal utilities and moral hazard costs.

Second, consider the case in which the young are homogeneous but we allow them to manipulate. In this case both the "selection on consumption smoothing value" and the "selection on moral hazard cost" terms become equal to zero. The elasticity terms are the only ones that matter and they do so through a standard fiscal externality logic. If one assumes that additional coverage weakly increases the share of manipulators and that additional baseline coverage weakly decreases it, then the wedge of the young is unambiguously negative, calling for over-insurance. Intuitively, it is optimal for the planner to grant the young additional surplus, above and beyond their manipulation-free level, because of their manipulation threat. To the extent that additional coverage mitigates manipulation the planner finds it optimal to provide such insurance to the young.

Third, consider a case where we also add heterogeneity among the young. In this case the selection terms could be positive or negative depending on who among the young selects into manipulation. If manipulators are particularly selected on the consumption smoothing value they assign to UI, and not particularly selected (or even negatively selected) on their behavioral to mechanical cost ratio, then this induces the planner to under-insure the young, to cause more manipulation. Proposition 1 shows that optimal policy is affected in so far as selection occurs on marginal utitilies and/or BC/MC ratios. For a formal definition of all of these terms see Appendix C.[40] In sum, Proposition 1 shows that selection on BC/MC ratios also play a role for the design of optimal policy, although they are far from the only force that determines the optimal level of UI in this context. However, due to a lack of relevant within-group policy variation necessary to identify all the relevant forces, a full welfare assessment is out of scope of this work.

Notice that Proposition 1 would hold also in the limiting case where the average young and the old are completely identical and policy differentiation is nonsensical. This is because the optimal degree of over or under-insurance for the young just depends on differences between manipulators and non-manipulators, rather than between young

[40]Proposition 1 combines insights from Schmieder and von Wachter (2017) and Hendren et al. (2021). Schmieder and von Wachter (2017) propose the use of BC/MC ratios as an efficiency cost measure for optimal potential benefit duration in unemployment insurance. Hendren et al. (2021) propose a framework to study choice in social insurance markets in which individuals can purchase additional insurance coverage at some price p. Although our setting does not feature a direct financial transfer from the individual to the government - like a price - Proposition 1 reveals analogous selection effects as in their setting.

and old.

We acknowledge that our normative implications are conditional on the specific tag used. Our theoretical framework does not provide a prescription for choosing among multiple potential tags or assessing their general appropriateness. This limitation arises from the inherent nature of the sufficient statistics approach, which offers local policy recommendations for a given tag rather than enabling global comparisons (Chetty, 2006; Spinnewijn, 2020). In principle, it is very well possible that there are other tags that the planner might use to directly target "deserving" manipulators.

In practice, finding credibly *exogenous* tags in the realm of social insurance has been very challenging (Spinnewijn, 2020). For example, consider the case of a policymaker who wants to target individuals with high long-term unemployment risk. Recent empirical evidence suggests that unemployment risk and associated moral hazard responses can be reasonably predicted at the beginning of the unemployment spell using recent *job histories* (Mueller and Spinnewijn, 2023; Wunsch and Zabrodina, 2023). These are much more predictive than basic sociodemographic characteristics like gender or *age-at-layoff*, reinforcing their relevance as potential tags. While an individual's past job history is an information-rich tag, it is also very much *endogenous* and prone to manipulation, which highlights the relevance of our methods. Concretely, if all of a sudden more discontinuous careers made workers eligible for longer UI, it is likely that some workers would alter their behavior in anticipation of qualifying for longer UI coverage. The selection into these behavioral responses could invalidate or reinforce any beneficial effects from differentiation along these informative tags, which is precisely the economic mechanism we highlight in the paper and which makes our methods highly relevant.

Therefore, the optimality of job histories as tag would constitute an exciting research topic where our theory and empirical methods could be fruitfully applied. Provided that researchers are equipped with the suitable data, setting and identifying variation, one could empirically implement proposition 1 to check whether more differentiation – inclusive of manipulation – along this newly found tag is welfare improving or not.[41] All in all, finding out which heterogeneities allow welfare-improving targeting in different policies is a fruitful avenue for future research, although policy makers might ultimately refrain from exploiting some of them, because of e.g. transparency, administrative costs, horizontal equity and fairness concerns.

6 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

[41]Of course our empirical strategy is only suited to study cutoff rules in some cardinal individual characteristic, as it would be the case with patterns in job histories.

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 responses being the prime motive for manipulation.

We also lay out a simple, yet robust theoretical framework to guide the design of differentiated social insurance under manipulation. We identify a set of sufficient statistics and illustrate how some of the key moments of the model can be estimated in practice. Our empirical strategy builds on and extends recently proposed bunching techniques which do not require rich policy variation for estimation.

We believe that our empirical methodology might be fruitfully applied in other contexts and, although a full welfare assessment is beyond the scope of this paper, we deem it an interesting area for future research. As pointed out by Spinnewijn (2020) there remains important work to be done in understanding, analysing and justifying frequently used tags in social insurance. We think that our framework and methodology provide an important first step.

References

Ahammer, A. and Packham, A. (2023). Effects of unemployment insurance duration on mental and physical health. *Journal of Public Economics*, 226:104996.

Ahlqvist, V. and Borén, E. (2017). Leaving already? the swedish unemployment insurance as a pathway to retirement.

Akerlof, G. A. (1978). The Economics of "Tagging" as Applied to the Optimal Income Tax, Welfare Programs, and Manpower Planning. *American Economic Review*, 68(1):8–19.

Albanese, A., Picchio, M., and Ghirelli, C. (2020). Timed to Say Goodbye: Does Unemployment Benefit Eligibility Affect Worker Layoffs? *Labour Economics*, 65:101846.

Anastasia, B., Mancini, M., and Trivellato, U. (2009). Il Sostegno al Reddito dei Disoccupati: Note sullo Stato dell'Arte: tra Riformismo Strisciante, Inerzie dell'Impianto Categoriale e Incerti Orizzonti di Flexicurity. I Tartufi 32, Veneto Lavoro.

Baguelin, O. and Remillon, D. (2014). Unemployment insurance and management of the older workforce in a dual labor market: Evidence from france. *Labour Economics*, 30:245–264.

Baily, M. N. (1978). Some Aspects of Optimal Unemployment Insurance. *Journal of Public Economics*, 10(3):379–402.

Barnichon, R. and Zylberberg, Y. (2022). A Menu of Insurance Contracts for the Unemployed. *The Review of Economic Studies*, 89(1):118–141.

Barreca, A. I., Guldi, M., Lindo, J. M., and Waddell, G. R. (2011). Saving babies? Revisiting the effect of very low birth weight classification. *Quarterly Journal of Economics*, 126(4):2117–2123.

Best, M. and Kleven, H. J. (2013). Optimal Income Taxation with Career Effects of Work Effort. *mimeo*.

Cabral, M. (2016). Claim Timing and Ex post Adverse Selection. *The Review of Economic Studies*, 84(1):1–44.

Cahuc, P., Charlot, O., and Malherbet, F. (2016). Explaining the spread of temporary jobs and its impact on labor turnover. *International Economic Review*, 57(2):533–572.

Caliendo, M., Tatsiramos, K., and Uhlendorff, A. (2013). Benefit duration, unemployment duration and job match quality: a regression-discontinuity approach. *Journal of applied econometrics*, 28(4):604–627.

Card, D., Chetty, R., and Weber, A. (2007). Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market. *Quarterly Journal of Economics*, 122(4):1511–1560.

Cattaneo, M. D., Jansson, M., and Ma, X. (2020). Simple local polynomial density estimators. *Journal of the American Statistical Association*, 115(531):1449–1455.

Chetty, R. (2006). A General Formula for the Optimal Level of Social Insurance. *Journal of Public Economics*, 90(10):1879–1901.

Chetty, R., Friedman, J. N., Olsen, T., and Pistaferri, L. (2011). Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records. *Quarterly Journal of Economics*, 126(2):749–804.

Dee, T. S., Dobbie, W., Jacob, B. A., and Rockoff, J. (2019). The Causes and Consequences of Test Score Manipulation: Evidence from the New York Regents Examinations. *American Economic Journal: Applied Economics*, 11(3):382–423.

Diamond, R. and Persson, P. (2016). The long-term consequences of teacher discretion in grading of high-stakes tests. Technical report, National Bureau of Economic Research.

Einav, L., Finkelstein, A., Ryan, S. P., Schrimpf, P., and Cullen, M. R. (2013). Selection on Moral Hazard in Health Insurance. *American Economic Review*, 103(1):178–219.

Feldstein, M. (1976). Temporary Layoffs in the Theory of Unemployment. *Journal of Political Economy*, 84(5):937–957.

Feldstein, M. (1978). The Effect of Unemployment Insurance on Temporary Layoff Unemployment. *American Economic Review*, 68(5):834–846.

Garcia, S. C. and Hansch, M. (2024). The effect of unemployment insurance benefits on (self-) employment: Two sides of the same coin? *Journal of Human Resources*.

Gerard, F. and Gonzaga, G. (2021). Informal Labor and the Efficiency Cost of Social Programs: Evidence from Unemployment Insurance in Brazil. *American Economic Journal: Economic Policy*, 13(3):167–206.

Gerard, F., Rokkanen, M., and Rothe, C. (2020). Bounds on Treatment Effects in Regression Discontinuity Designs with a Manipulated Running Variable. *Quantitative Economics*, 11(3):839–870.

Guglielminetti, E., Lalive, R., Ruh, P., and Wasmer, E. (2024). Job search with commuting and unemployment insurance: A look at workers' strategies in time. *Labour Economics*, 88:102537.

Hackmann, M. B., Kolstad, J. T., and Kowalski, A. E. (2015). Adverse Selection and an Individual Mandate: When Theory Meets Practice. *American Economic Review*, 105(3):1030–66.

Heckman, J. J., Urzua, S., and Vytlacil, E. (2006). Understanding Instrumental Variables in Models with Essential Heterogeneity. *The Review of Economics and Statistics*, 88(3):389–432.

Hendren, N. (2017). Knowledge of Future Job Loss and Implications for Unemployment Insurance. *American Economic Review*, 107(7):1778–1823.

Hendren, N., Landais, C., and Spinnewijn, J. (2021). Choice in Insurance Markets: A Pigouvian Approach to Social Insurance Design. *Annual Review of Economics*, 13:457–486.

Huang, P.-C. and Yang, T.-T. (2021). The welfare effects of extending unemployment benefits: Evidence from re-employment and unemployment transfers. *Journal of Public Economics*, 202:104500.

Ichino, A. and Riphahn, R. T. (2005). The effect of employment protection on worker effort: Absenteeism during and after probation. *Journal of the European Economic Association*, 3(1):120–143.

Jäger, S., Schoefer, B., and Zweimüller, J. (2023). Marginal Jobs and Job Surplus: a Test of the Efficiency of Separations. *The Review of Economic Studies*, 90(3):1265–1303.

Johnston, A. C. and Mas, A. (2018a). Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-Level Response to a Benefit Cut. *Journal of Political Economy*, 126(6):2480–2522.

Johnston, A. C. and Mas, A. (2018b). Potential unemployment insurance duration and labor supply: The individual and market-level response to a benefit cut. *Journal of Political Economy*, 126(6):2480–2522.

Khoury, L. (2023). Unemployment Benefits and Redundancies: Incidence and Timing Effects. *Journal of Public Economics*, 226:104984.

Kleven, H. J. (2016). Bunching. *Annual Review of Economics*, 8(1):435–464.

Kleven, H. J. and Waseem, M. (2013). Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan. *Quarterly Journal of Economics*, 128(2):669–723.

Kowalski, A. E. (2021). Reconciling Seemingly Contradictory Results from the Oregon Health Insurance Experiment and the Massachusetts Health Reform. *Review of Economics and Statistics*, pages 1–45.

Lalive, R. (2007). Unemployment Benefits, Unemployment Duration, and Post-Unemployment Jobs: A Regression Discontinuity Approach. *American Economic Review*, 97(2):108–112.

Landais, C. (2015). Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design. *American Economic Journal: Economic Policy*, 7(4):243–278.

Landais, C., Nekoei, A., Nilsson, P., Seim, D., and Spinnewijn, J. (2021). Risk-Based Selection in Unemployment Insurance: Evidence and Implications. *American Economic Review*, 111(4):1315–55.

Lawson, N. (2017). Fiscal Externalities and Optimal Unemployment Insurance. *American Economic Journal: Economic Policy*, 9(4):281–312.

Le Barbanchon, T., Rathelot, R., and Roulet, A. (2019). Unemployment insurance and reservation wages: Evidence from administrative data. *Journal of Public Economics*, 171:1–17.

Marone, V. R. and Sabety, A. (2022). When Should There Be Vertical Choice in Health Insurance Markets? *American Economic Review*, 112(1):304–342.

Michelacci, C. and Ruffo, H. (2015). Optimal Life Cycle Unemployment Insurance. *American Economic Review*, 105(2):816–859.

Mueller, A. I. and Spinnewijn, J. (2023). The nature of long-term unemployment: Predictability, heterogeneity and selection. Technical report, National Bureau of Economic Research.

Nekoei, A. and Weber, A. (2017). Does Extending Unemployment Benefits Improve Job Quality? *American Economic Review*, 107(2):527–561.

Nichols, A. L. and Zeckhauser, R. J. (1982). Targeting transfers through restrictions on recipients. *The American Economic Review*, 72(2):372–377.

Rosolia, A. and Sestito, P. (2012). The Effects of Unemployment Benefits in Italy: Evidence from an Institutional Change. *Bank of Italy Temi di Discussione (Working Paper) No*, 860.

Saez, E. (2010). Do Taxpayers Bunch at Kink Points? *American Economic Journal: Economic Policy*, 2(3):180–212.

Schivardi, F. and Torrini, R. (2008). Identifying the effects of firing restrictions through size-contingent differences in regulation. *Labour Economics*, 15(3):482–511.

Schmieder, J. F. and von Wachter, T. (2016). The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation. *Annual Review of Economics*, 8:547–581.

Schmieder, J. F. and von Wachter, T. (2017). A Context-Robust Measure of the Disincentive Cost of Unemployment Insurance. *American Economic Review*, 107(5):343–348.

Schmieder, J. F., von Wachter, T., and Bender, S. (2012). The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates Over 20 Years. *Quarterly Journal of Economics*, 127(2):701–752.

Scrutinio, V. (2018). The Medium-Term Effects of Unemployment Benefits. *INPS working paper no. 18*.

Spinnewijn, J. (2020). The Trade-Off between Insurance and Incentives in Differentiated Unemployment Policies. *Fiscal Studies*, 41(1):101–127.

Topel, R. H. (1983). On Layoffs and Unemployment Insurance. *American Economic Review*, 73(4):541–559.

Van Doornik, B., Schoenherr, D., and Skrastins, J. (2023). Strategic formal layoffs: Unemployment insurance and informal labor markets. *American Economic Journal: Applied Economics*, 15(1):292–318.

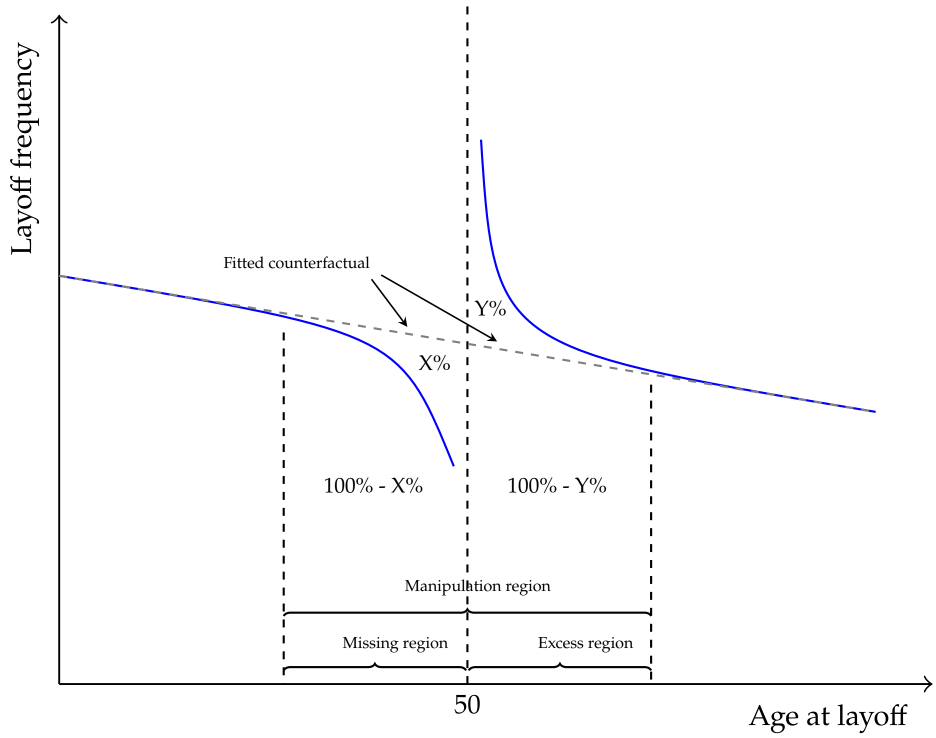
Weinzierl, M. (2011). The Surprising Power of Age-Dependent Taxes. *The Review of Economic Studies*, 78(4):1490–1518.

Wunsch, C. and Zabrodina, V. (2023). Unemployment insurance with response heterogeneity. *CESifo Working Paper N. 10704*.

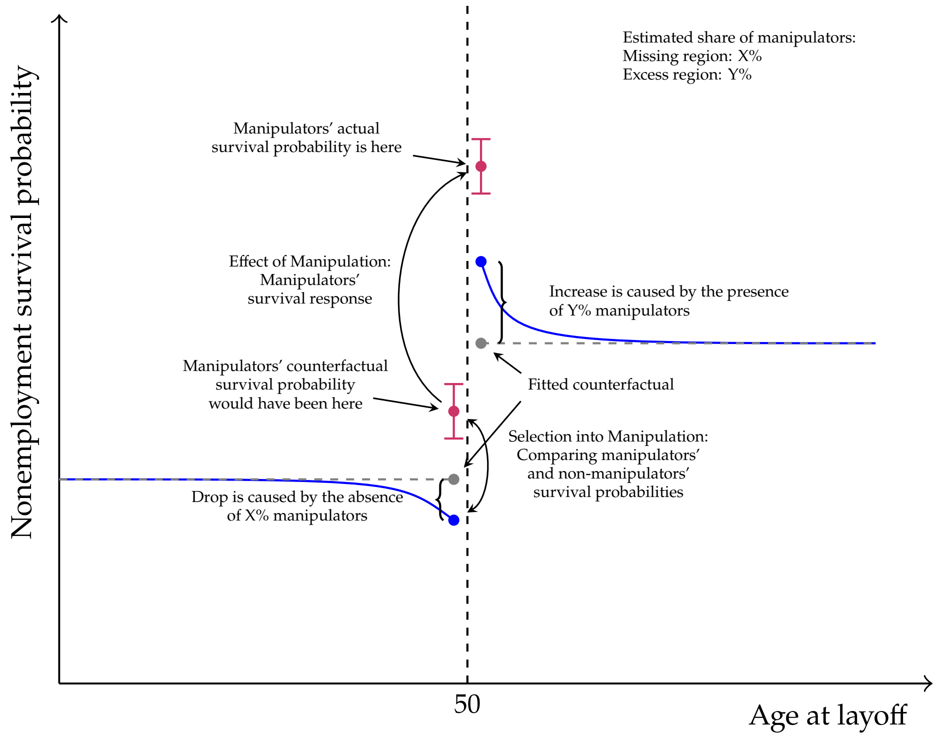
Figures

Figure 1. Illustration of identification strategy

(a) Quantifying manipulation

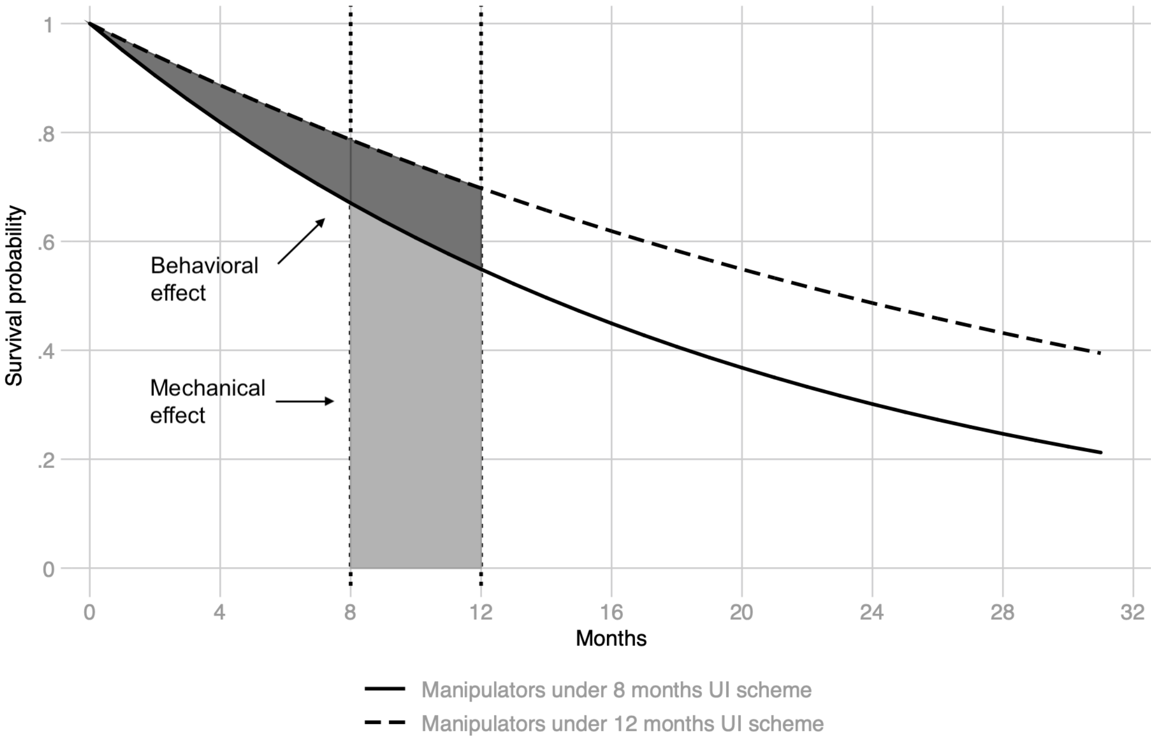


(b) Effect of and selection into manipulation



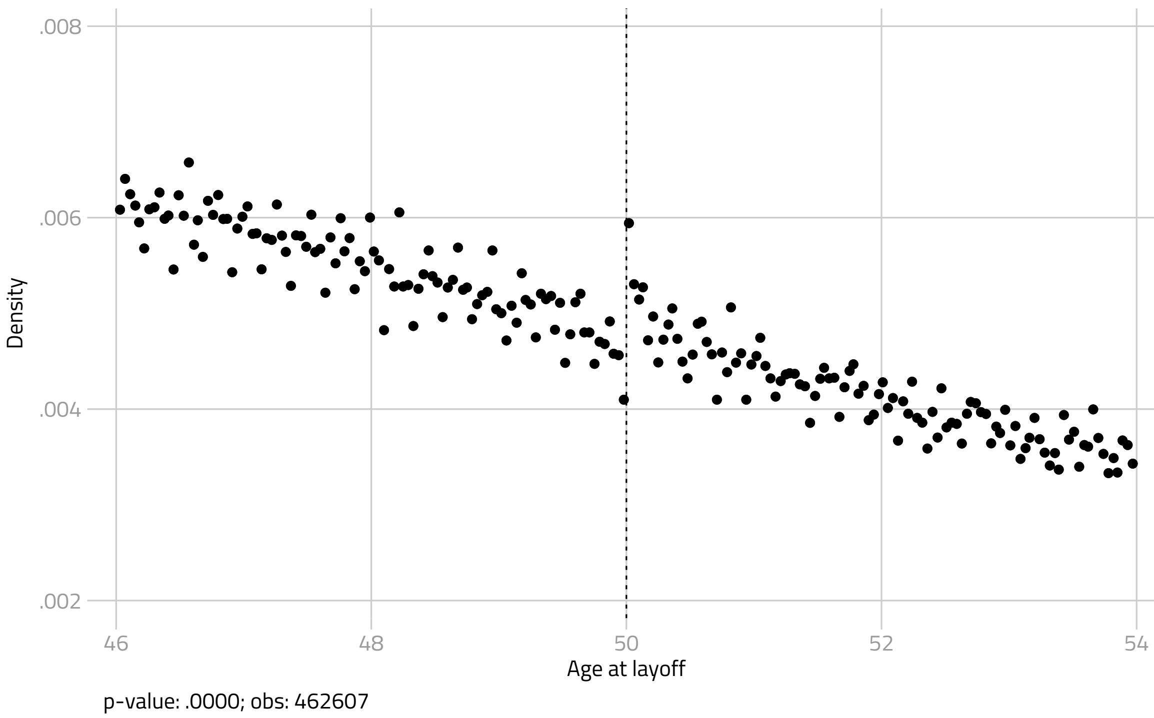
Note: The figure visualizes our identification strategy. Panel (a) illustrates how we estimate the number and respective share of manipulators in both the missing and excess region. Panel (b) constructs manipulators' survival response and illustrates the relevant comparison when studying selection into manipulation. Section 3 lays out how we estimate the fitted counterfactuals in practice.

Figure 2. The moral hazard cost of extended UI coverage



Note: The figure displays two hypothetical nonemployment survival curves for manipulators, namely, under eight months of PBD (solid line) and twelve months of PBD (dashed line). The dashed line is above the solid line assuming that higher PBD lowers the exit hazard rate from nonemployment. The curves are simulated as negative exponentials with a constant hazard rate of 5% and 3%, respectively. The total increase in UI benefit receipt due to higher coverage (shaded areas) consists of two components: (1) a mechanical part (light grey area) which captures additional UI benefit payments that would occur even absent any behavioral change; (2) a behavioral component (dark grey area) which is due to a shift in the survival curve. The BC/MC ratio defined in equation 22 is given by the ratio of (2) and (1).

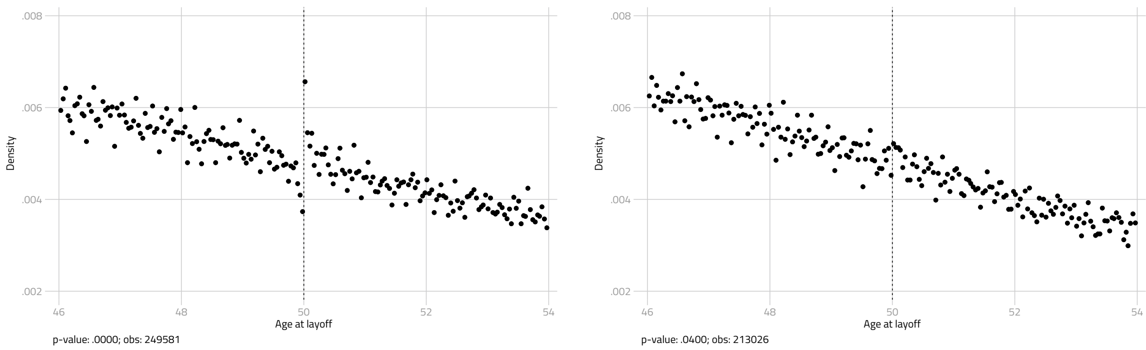
Figure 3. Layoff frequency for workers in the private sector between 46 and 54 years of age



Note: The figure shows the density of layoffs for the full sample of workers in the private sector claiming regular UI (OUB) for the age range from 46 to 54 years. The data cover the period from Feb 2009 to Dec 2012. P-values are from density tests proposed by Cattaneo et al. (2020) and implemented through the `rddensity` command in Stata.

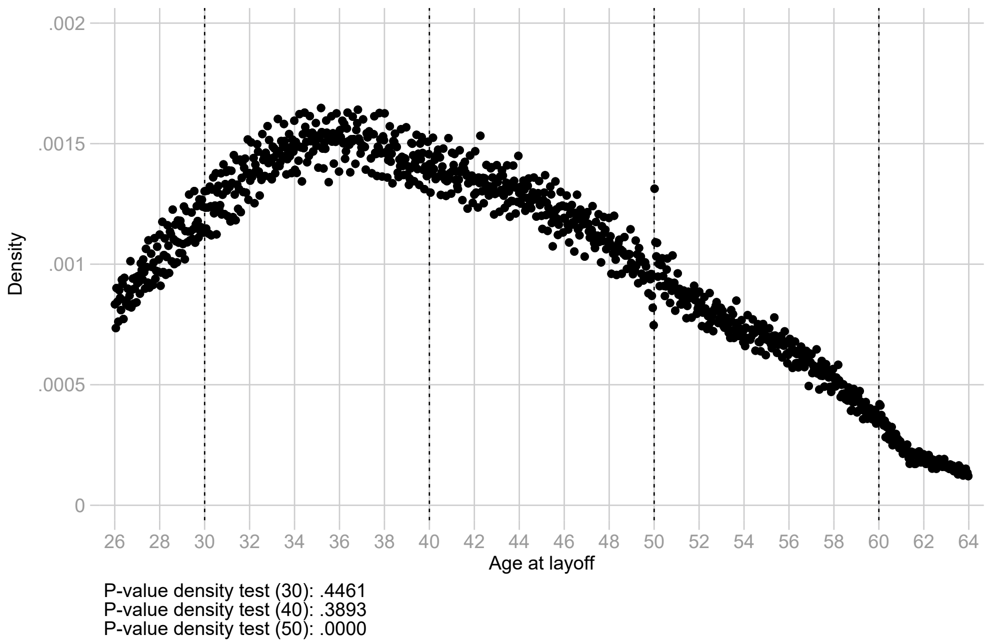
Figure 4. Layoff frequency in the private sector by contract type

(a) Private sector: Permanent contracts (b) Private sector: Temporary contracts



Note: The figure shows the density of layoffs by contract type for individuals claiming regular UI (OUB). The data cover the period from Feb 2009 to Dec 2012. In all panels each dot represents a two-week bin. Individuals are classified as "private sector" workers if they can be matched to an employment spell in the private sector matched employer-employee database (UNIEMENS). P-values are from density tests proposed by Cattaneo et al. (2020) and implemented through the `rddensity` command in Stata.

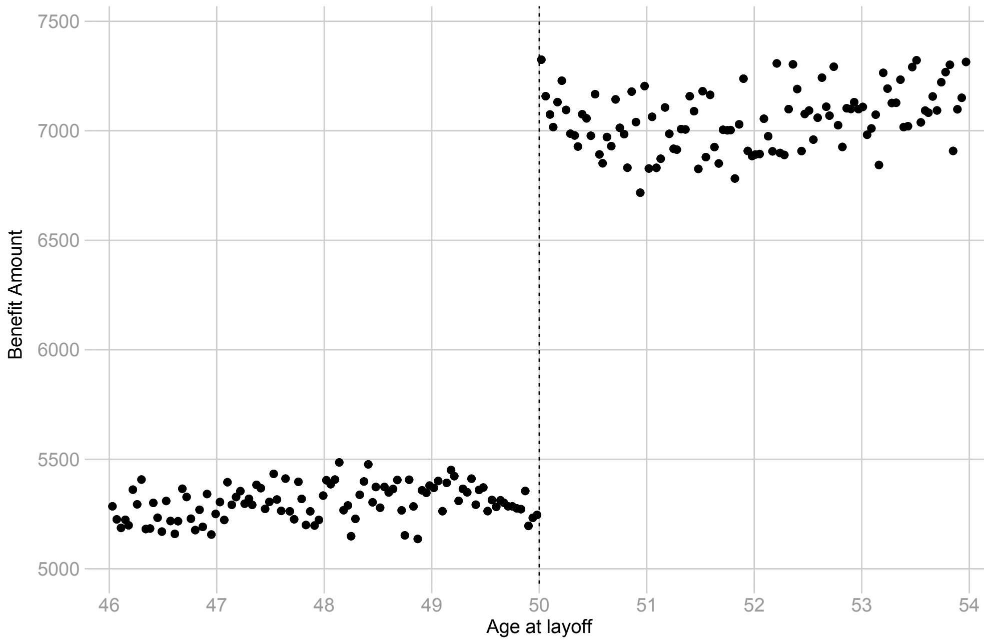
Figure 5. Layoff frequency in the private sector for permanent workers with age-at-layoff between 26 and 64 years



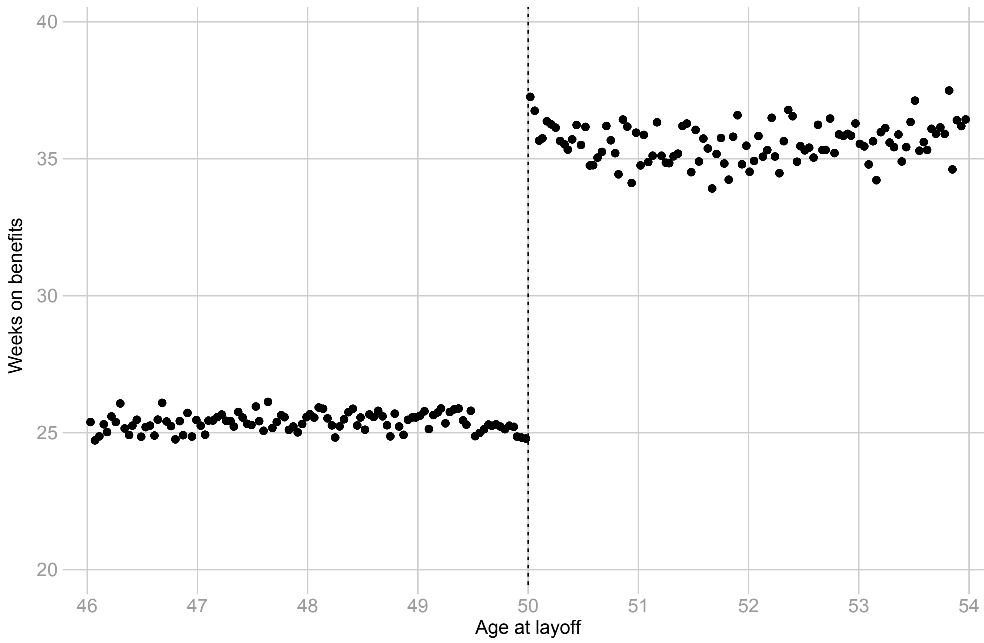
Note: The figure shows the density of layoffs in the private sector, for individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period from Feb 2009 to Dec 2012. The figure plots the density of layoffs for the entire age range from 26 to 64 years of age. Each dot represents a two-week bin.

Figure 6. Benefit receipt and duration

(a) average UI receipt (in Euro)



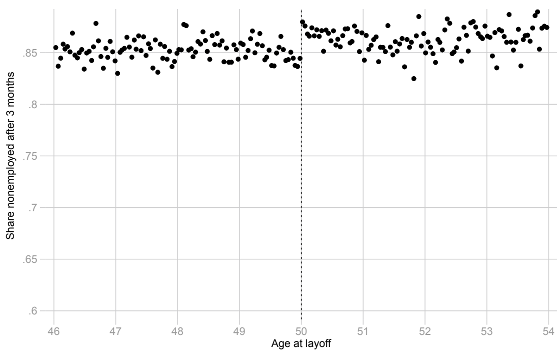
(b) average UI benefit duration (in weeks)



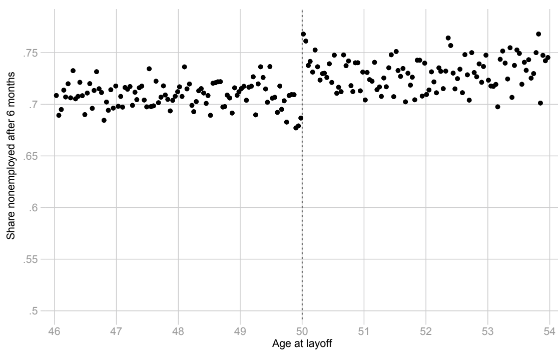
Note: The figure displays the average UI receipt in Euro (panel (a)) and average UI benefit duration in weeks (panel (b)) by age-at-layoff. In both panels each dot represents a two week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover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

Figure 7. Nonemployment survival probabilities

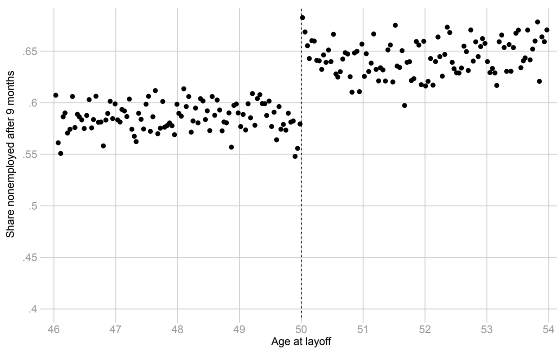
(a) Probability of still not being in employment at 3 months



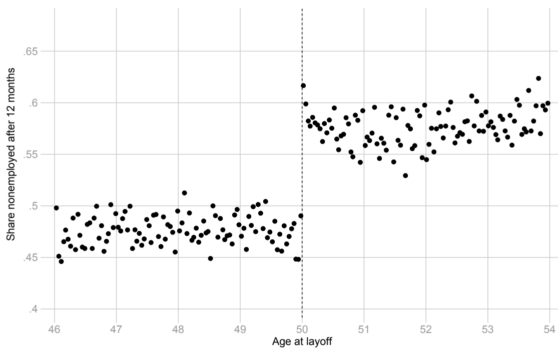
(b) Probability of still not being in employment at 6 months



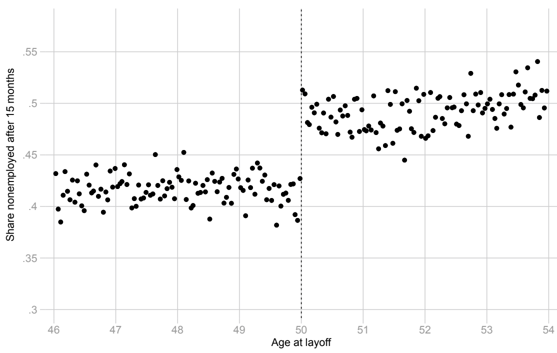
(c) Probability of still not being in employment at 9 months



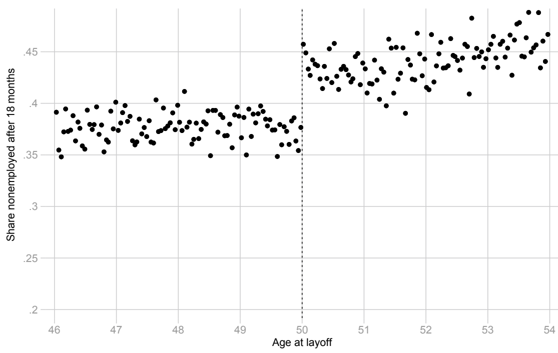
(d) Probability of still not being in employment at 12 months



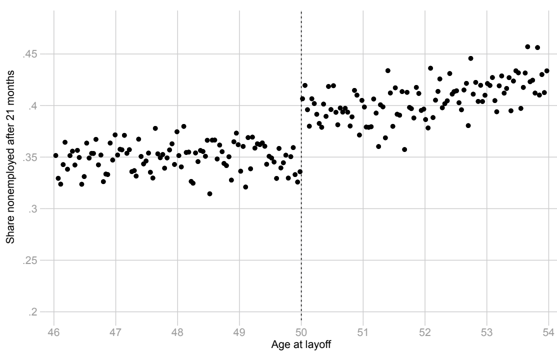
(e) Probability of still not being in employment at 15 months



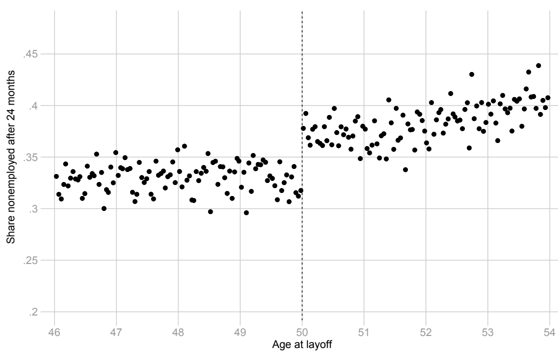
(f) Probability of still not being in employment at 18 months



(g) Probability of still not being in employment at 21 months



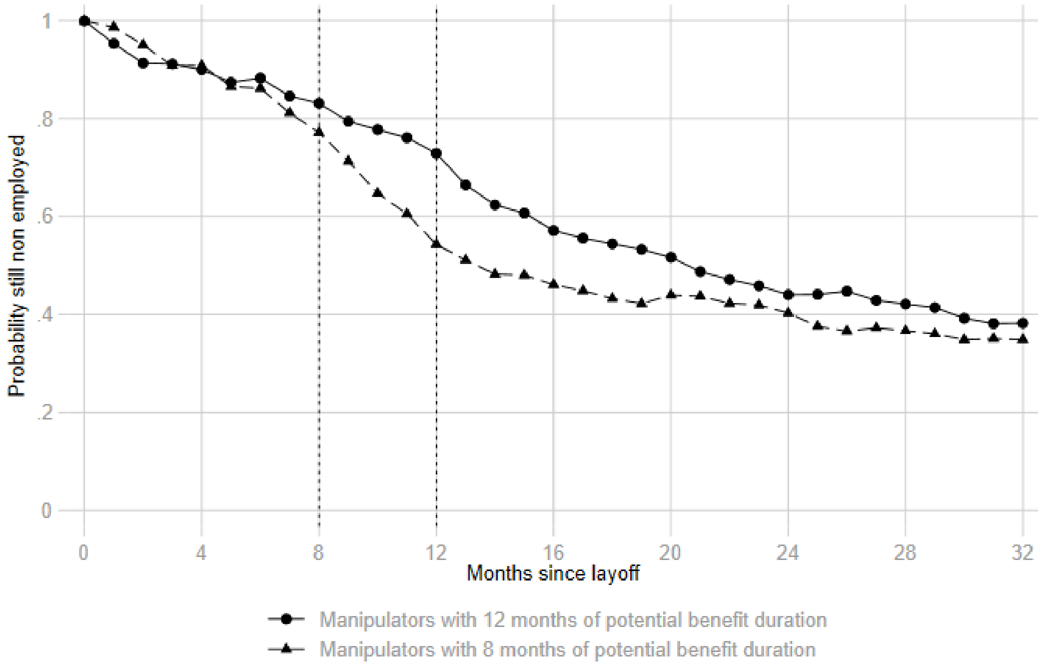
(h) Probability of still not being in employment at 24 months



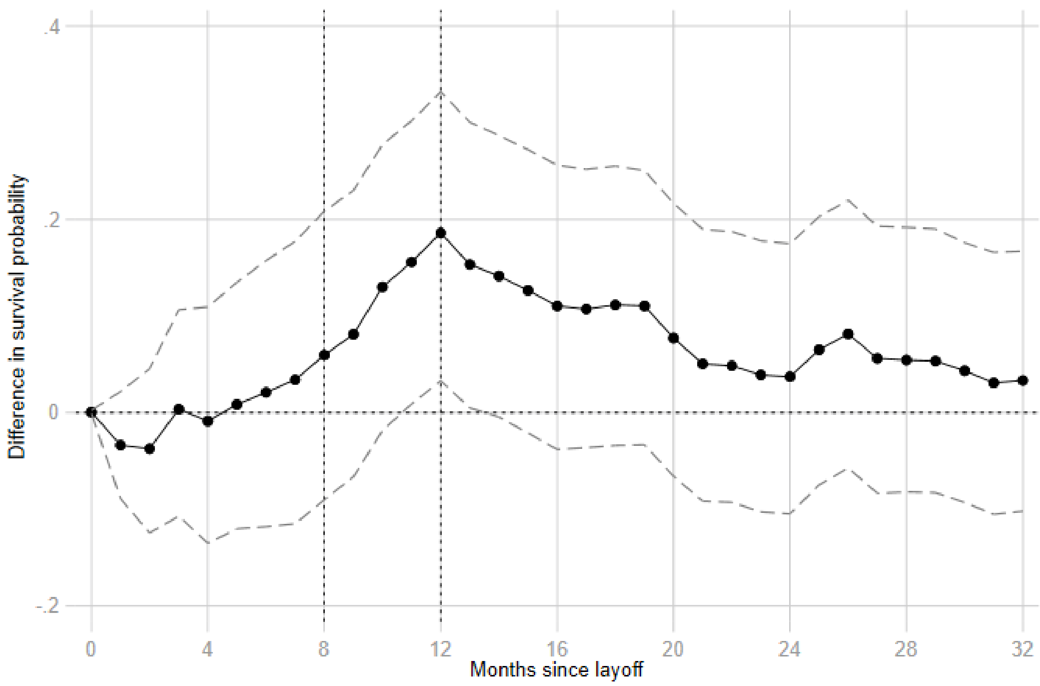
Note: The figures show the share of laid off workers, who are still not in employment after 3, 6, ..., 24 months. In all panels each dot represents a two week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period from Feb 2009 to Dec 2012. The underlying data consists of 249,581 layoffs.

Figure 8. Manipulators with 8 and 12 months of potential benefit duration

(a) Nonemployment survival rates



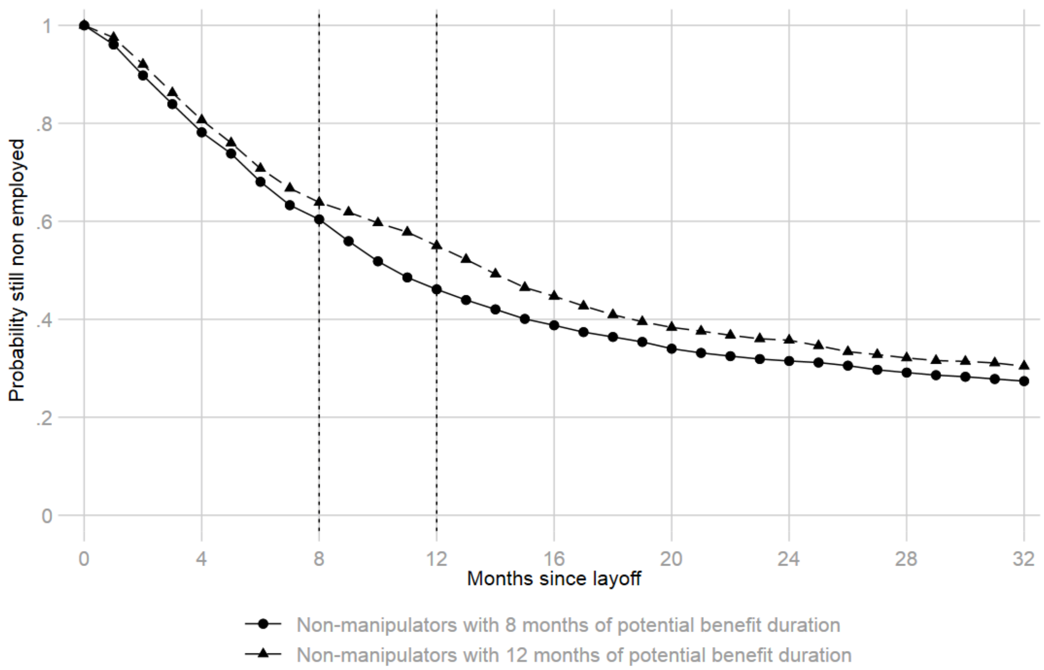
(b) Difference in survival rates



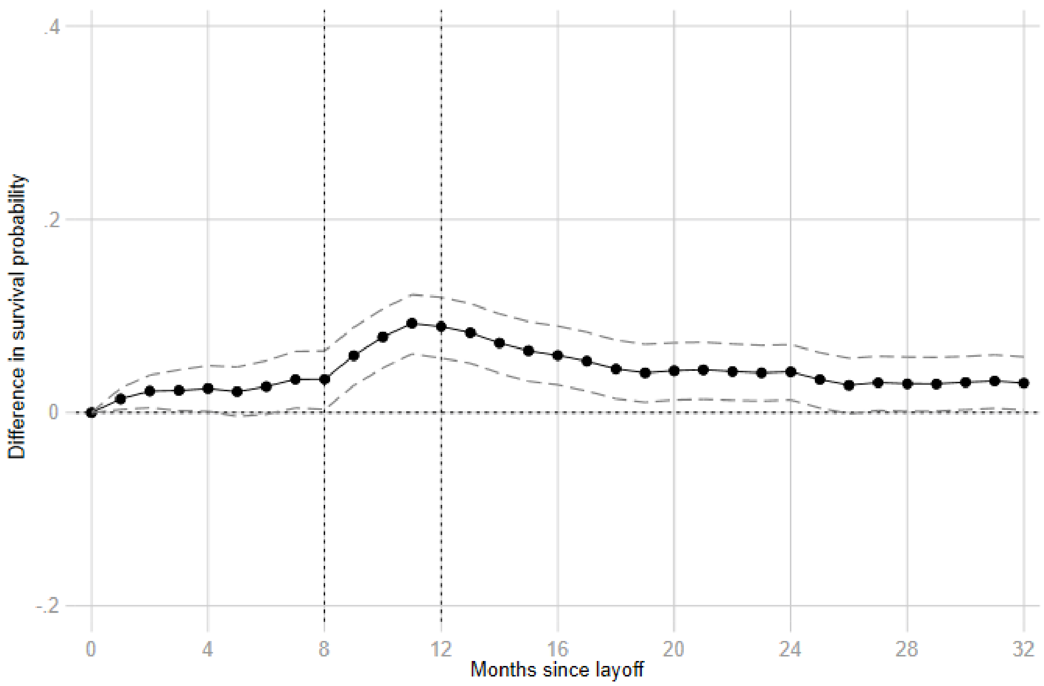
Note: Panel (a) plots point estimates of manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in Section 3. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 9. Non-manipulators with 8 and 12 months of potential benefit duration

(a) Nonemployment survival rates



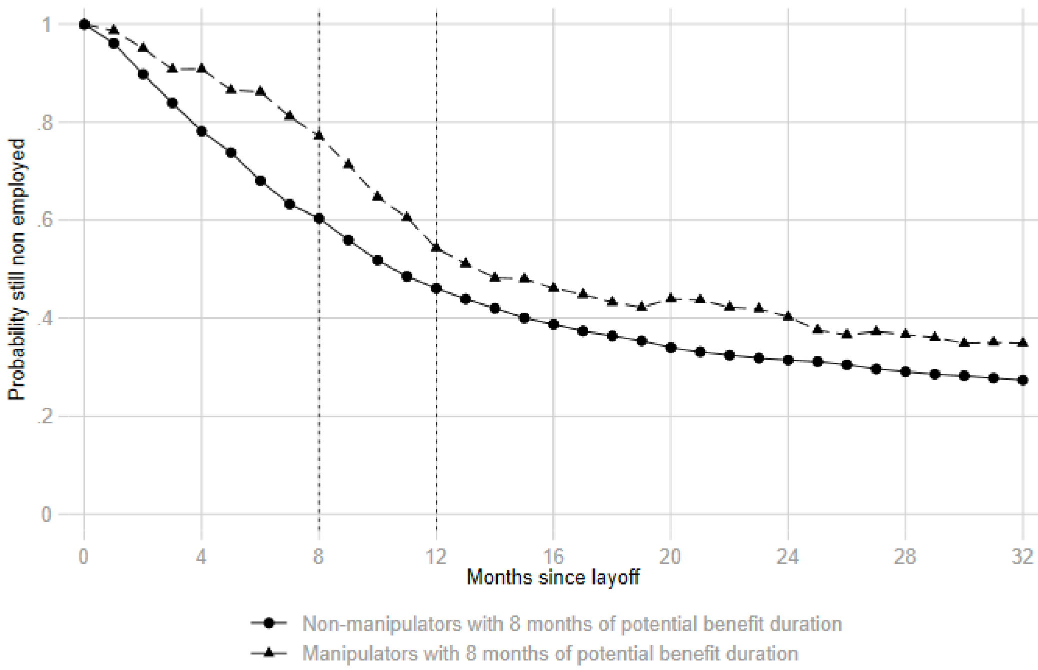
(b) Difference in survival rates



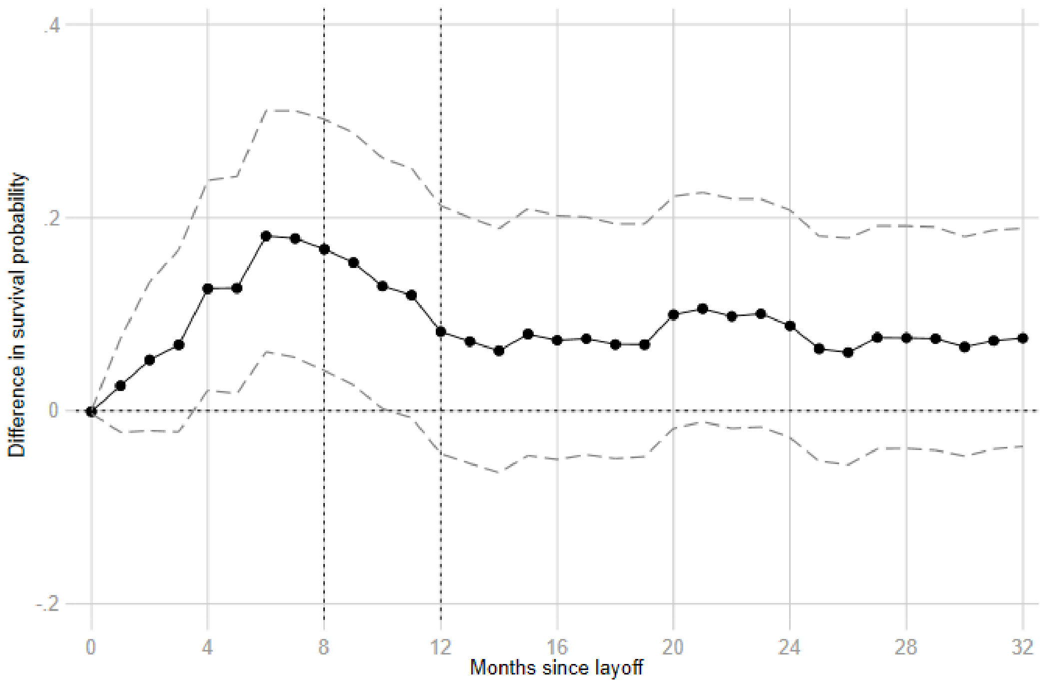
Note: Panel (a) plots point estimates of non-manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in Section 3. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 10. Manipulators and non-manipulators with 8 months of potential benefit duration

(a) Nonemployment survival rates



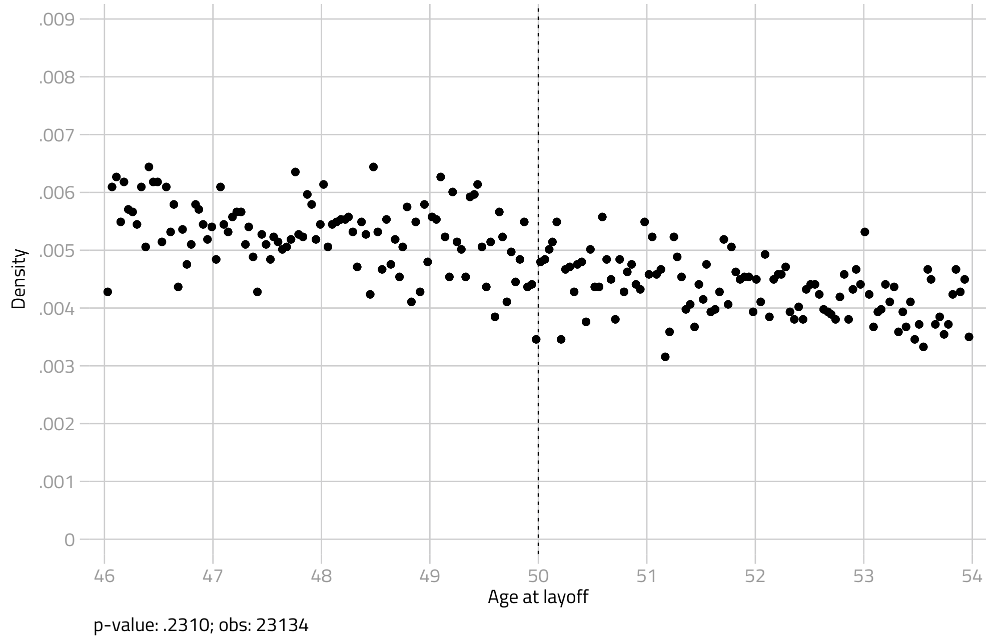
(b) Difference in survival rates



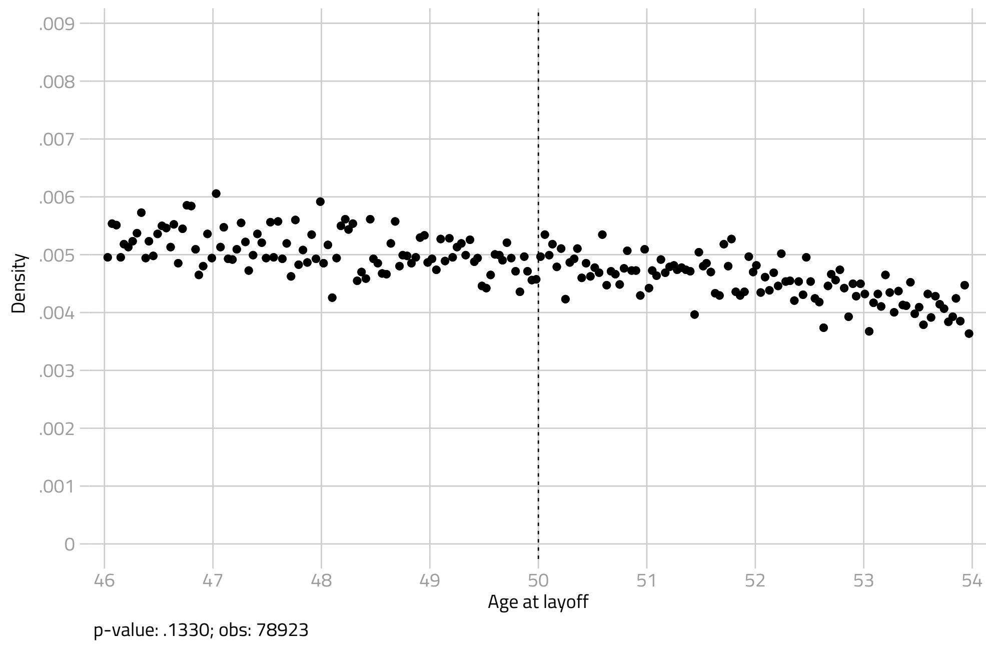
Note: Panel (a) plots point estimates of manipulators' and non-manipulators' nonemployment survival over the first 32 months after layoff under eight months of PBD. The estimation of the former is outlined in Section 3. The latter represents the observed mean survival rate in the missing region. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 11. Placebo checks: Layoff frequency for workers claiming MiniASpI and NASpI between 46 and 54 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 (Mar 2013 to Apr 2015) or NASpI (from Jan 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. P-values are from density tests proposed by Cattaneo et al. (2020) and implemented through the `rddensity` command in Stata.

Tables

Table 1. Summary statistics

	Private sector				Permanent contracts in the private sector			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Nonemployment outcomes								
UI Benefit receipt duration (in weeks)	26,181	15,908	0,143	52,000	29,853	15,923	0,143	52,000
Nonemployment duration (in weeks)	70,464	74,198	0,000	208,000	89,995	79,092	0,000	208,000
Nonemployment survival prob. 8 months	0,502	0,500	0,000	1,000	0,644	0,479	0,000	1,000
Nonemployment survival prob. 12 months	0,388	0,487	0,000	1,000	0,518	0,500	0,000	1,000
Individual characteristics								
Female (share)	0,377	0,485	0,000	1,000	0,311	0,463	0,000	1,000
Experience (in years)	27,443	8,854	2,000	40,000	27,656	8,552	2,000	40,000
North (share)	0,425	0,494	0,000	1,000	0,367	0,482	0,000	1,000
Center (share)	0,181	0,385	0,000	1,000	0,174	0,379	0,000	1,000
South and islands (share)	0,394	0,489	0,000	1,000	0,459	0,498	0,000	1,000
Previous job characteristics								
Full time (share)	0,803	0,397	0,000	1,000	0,807	0,395	0,000	1,000
White collar (share)	0,183	0,387	0,000	1,000	0,208	0,406	0,000	1,000
Tenure (years)	4,351	5,291	0,083	30,000	5,931	6,113	0,083	30,000
Daily wage (past 6 months)	69,899	70,300	0,038	13981,006	73,965	90,583	0,038	13981,006
Firm Characteristics								
Firm age (in years)	15,314	12,696	0,000	109,833	14,367	12,115	0,000	109,833
Firm size	84,915	608,022	1,000	14222,000	28,158	259,005	1,000	14103,167
Firm size below 15 (share)	0,606	0,489	0,000	1,000	0,765	0,424	0,000	1,000
Firm size between 15 and 49 (share)	0,213	0,409	0,000	1,000	0,164	0,370	0,000	1,000
Firm size above 49 (share)	0,181	0,385	0,000	1,000	0,071	0,257	0,000	1,000
Share Permanent	0.682	0.372	0.000	1.000	0.934	0.145	0.000	1.000
Share White Collar	0.224	0.301	0.000	1.000	0.232	0.319	0.000	1.000
Manager in Firm	0.002	0.021	0.000	1.000	0.002	0.027	0.000	1.000
Absences								
Sickness (4 years)	0.270	0.976	0.000	18.366	0.420	1.251	0.000	18.366
Work Accident (4 years)	0.063	0.523	0.000	18.277	0.096	0.679	0.000	18.277
Disability (4 years)	0.001	0.042	0.000	5.208	0.001	0.056	0.000	5.208
Disability others (4 years)	0.021	0.452	0.000	22.285	0.036	0.603	0.000	22.285
Child Leave (4 years)	0.011	0.327	0.000	21.717	0.018	0.430	0.000	21.717
Past Career in private Sector								
Total Months Employed (10 years)	83.506	31.626	0.000	120.000	89.525	30.911	0.000	120.000
Total Income (10 years)	124673.883	90928.203	0.000	3985142.500	138451.125	104694.781	0.000	3985142.500

Note: The table reports summary statistics for both the sample of OUB claims linked to separations in the private sector, and the subsample of OUB claims linked to separations from permanent contracts in the private sector. The observation window is February 2009 to December 2012. All workers are between 46-54 years of age at the time of layoff. The sample contains a total of 462,607 nonemployment spells and 332,899 individuals for the "private sector" sample and 249,581 nonemployment spells from 210,041 individual workers for the "permanent contracts in the private sector" sample. 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. Experience is equal to the number of years since the first social security contribution. 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, Sardegna and Sicilia. Firm characteristics are computed at the firm municipality level in the month of the layoff (or the closest within 6 months of the layoff). Variables in Absences are computed as the total amount received by the worker in the 4 years before layoff at the firm laying her off. The total amount received by the worker is normalized by the average wage in the three months prior to layoff as reported by the SIP. These measures are winsorized at 0.1% to reduce the influence of extreme values. Months of employment and total income are limited to the private sector.

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. We formally define all quantities in Section 3. All results are based on our main sample consisting of 249,581 observations. Bootstrapped 95% confidence intervals are in parentheses.

Table 3. UI Benefit receipt estimates (in Euro)

(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. We formally define all quantities in Section 3. All results are based on our main sample consisting of 249,581 observations. Bootstrapped 95% confidence intervals are in parenthesis.

Table 4. Benefit duration estimates (in 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. We formally define all quantities in Section 3. All results are based on our main sample consisting of 249,581 observations. Bootstrapped 95% confidence intervals are in parenthesis.

Table 5. BC/MC ratio estimates

	(1) without taxes ($\tau = 0\%$)	(2) with taxes ($\tau = 3\%$)
(a) Manipulators	0.24	0.32
	(0.02, 0.89)	(0.03, 1.13)
(b) Non-manipulators	0.26	0.32
	(0.12, 0.41)	(0.15, 0.50)

Note: The table reports BC/MC ratio estimates for (a) manipulators and (b) non-manipulators. BC/MC ratios are defined in equation (22). Bootstrapped 95% confidence intervals in parentheses.

Table 6. Selection on observables

	(1) Manipulators	(2) Non-Manipulators	(3) Difference (1)-(2)
Female (share)	0.450	0.270	0.180
			(0.100, 0.281)
White Collar (share)	0.351	0.180	0.170
			(0.101, 0.239)
Southern Region (share)	0.483	0.471	0.012
			(-0.072, 0.098)
Full Time (share)	0.754	0.822	-0.067
			(-0.134, -0.000)
Tenure (in years)	6.577	5.718	0.859
			(-0.142, 1.853)
Daily Wage (in logs)	4.115	4.176	-0.0610
			(-0.142, 0.023)
Months Employed (10 years)	93.174	88.131	5.043
			(-0.327,10.339)
Total Wage (10 years)	135688.203	136566.043	-877.84
			(-18998.984,16703.102)
Firm Age (in years)	14.546	14.335	0.211
			(-1.945, 2.320)
Firm Size (in logs)	1.862	2.258	-0.395
			(-0.640, -0.155)
Share Permanent	0.95	0.931	0.019
			(-0.005;0.044)
Share White Collar	0.349	0.209	0.14
			(0.086;0.194)
Manager in Firm	0.002	0.002	0.001
			(-0.003;0.004)
Sickness (4 years)	0.428	0.443	-0.015
			(-0.238,0.196)
Work Accident (4 years)	0.148	0.082	0.066
			(-0.047,0.181)
Disability (4 years)	0.002	0.000	0.002
			(-0.002,0.006)
Disability others (4 years)	0.051	0.036	0.0153
			(-0.100,0.128)
Child Leave (4 years)	0.034	0.001	0.033
			(0.004,0.065)

Note: The table reports differences in observable characteristics between manipulators and non-manipulators in our main sample. Column 1 and 2 report estimated means of observable characteristics for manipulators and non-manipulators, respectively. Column 3 reports their difference and associated 95% bootstrapped confidence intervals in parentheses. Sickness, Work Accidents, Disability, Disability others, Child Leave represent the amount paid to the worker (normalized by the wage of the worker) due to absences or work reduction related to these events. The events are considered only if they take place within the firm laying off the worker in the four years prior to layoff. The total amount received by the worker is normalized by the average wage in the three months prior to layoff as reported by the SIP. Sickness includes only amounts paid as a consequence of absences longer then 7 days, similarly to Work Accidents which covers only payments for absences related to work injuries lasting more than 7 days. Disability concerns absences due to certified disability of the worker while Disability others relates to absences to take care of disabled relatives or children. Child leave covers absences related to children both due to the birth of a child and to sickness of the child.

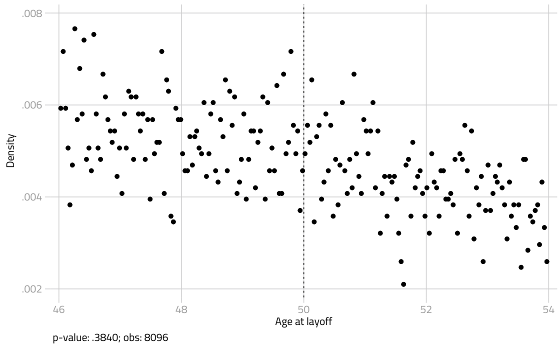
Appendices

For online publication

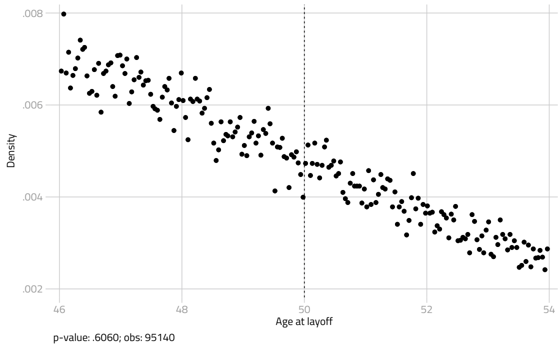
A Additional Figures

Figure A1. Density of layoff from the public sector by contract type

(a) Public sector: Permanent



(b) Public sector: Temporary



Note: The figure shows the density of layoffs by contract type for workers laid off from the public sector. The data cover the period from Feb 2009 to Dec 2012. In all panels each dot represents a two-week bin. Individuals are classified as "public sector" workers if they cannot be matched to an employment spell in the private sector database (UNIEMENS).

B Manipulation in Temporary Contracts

By their very nature, workers on temporary contracts are particularly exposed to the risk of nonemployment and are indeed largely over-represented among UI recipients. According to INPS data (UNIEMENS and SIP databases), in our period of interest (2010-2012)[42] they made up about 53% of UI spells, while they represented 17% of employed individuals in the private sector (about 2.5 million workers out of 14 millions). [43] These patterns are visible also in our sample, as we find that workers on temporary contracts represent about 45% of all UI spells in our data.

Despite the higher probability to be in nonemployment, Figure 4b shows that these workers do not re-time their layoff to obtain longer PBD. This can be rationalized by three sets of explanations.

First, there are some institutional barriers. Indeed, according to the Italian legislation, workers on temporary contracts cannot be laid off before the end of their contract, which makes it difficult to time their layoff exactly after their fiftieth birthday.[44] They may quit but quits are not eligible for UI. There are only two ways in which the worker or firm can terminate a temporary contract before the end date such that the worker is still eligible to UI. These would be either through a quit or layoffs for "just cause" (*giusta causa*), which is triggered only by very serious misconduct either from the worker or the employer side (e.g. theft or physical violence). However, these kinds of separation also require a judicial action from the side of the employee in case of quit for "just cause" or the notification of an irregular behavior for the layoff for "just cause". This is costly and could generate stigma on the employee, affecting her future career prospects. As this kind of separation would be unattractive to the worker and the firm in absence of a strong reason to terminate the job, it would be unlikely that workers are able to exactly time the layoff when the contract is ongoing.

Second, we argue that workers on temporary contracts have lower incentives to manipulate. This is because these workers face a much lower risk of exhausting the shorter PBD. If we consider workers with between 46 and 50 years of age in our sample, only 20% of the workers previously employed on temporary contracts still receive UI 8 months after their layoff, as shown in Figure B1. The same share is close to 50% for workers previously employed on permanent contracts. This provides clear evidence that the mechanical transfer would be lower for workers on temporary contracts. However, it may be argued

[42]The year 2009 was excluded from this calculation since we have only data from February onwards.

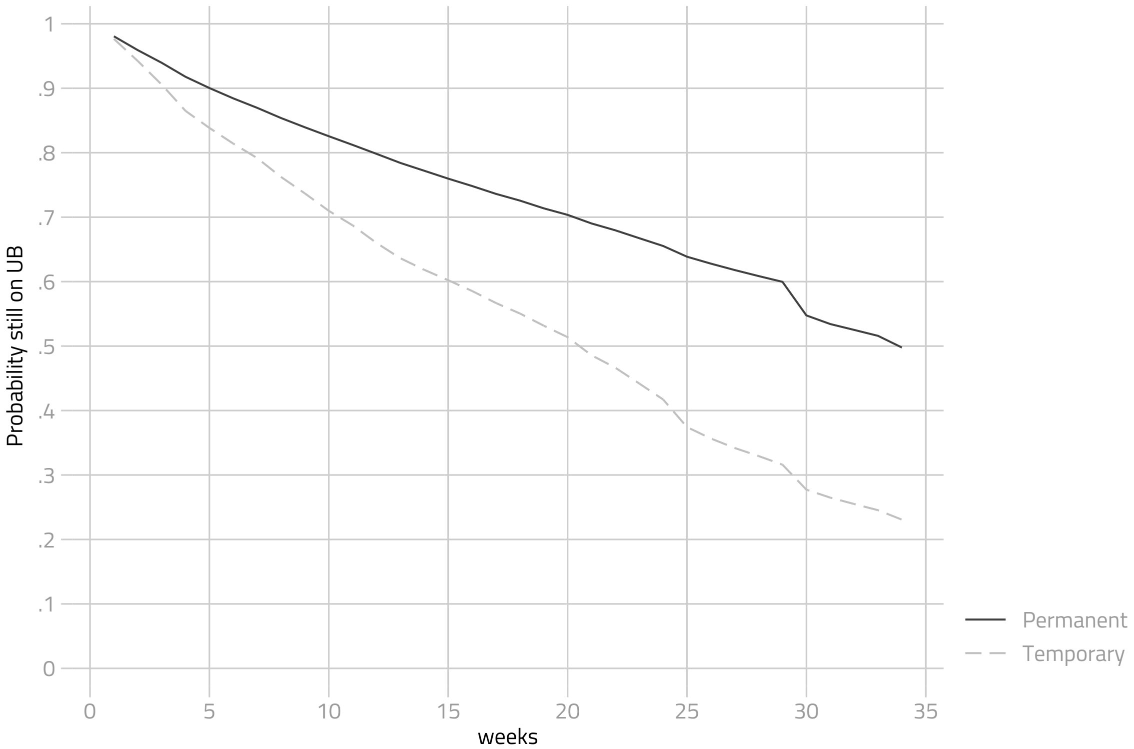
[43]This number excludes workers in the public sector but including them in the analysis would likely make the imbalance even starker. According to the earliest available INPS data, in 2014 there were about 3.07 million public sector employees with permanent contracts and about 300,000 with temporary contracts (*Osservatorio Lavoratori Pubblici*, `https://servizi2.inps.it/servizi/osservatoristatistici/69`). These numbers remain relatively stable in the following years.

[44]High employment protection for temporary contracts – as long as the contract is active – is widespread in EU countries, as pointed out in Cahuc et al. (2016).

that workers do not have the ability to predict their path to reemployment. In this sense, having (recent) past experience with UI may provide workers with useful information. In particular, in many cases workers close to the cutoff on temporary contracts have been exposed to UI a lot more in the past, when compared to workers on permanent contracts. As described in Figure B2, only about 25% of workers on permanent contracts had received UI at least once over the the 2 years preceding the layoff, while this number jumps to about 70% for workers on temporary contracts. In 40% of the cases workers on temporary contracts had already obtained UI two or more times in the two years before, while this share is less than 10% for workers on permanent contracts. It seems reasonable to think that workers who are laid off from a temporary contract both face a much lower risk of long term unemployment and have a much more recent experience with job search and unemployment.

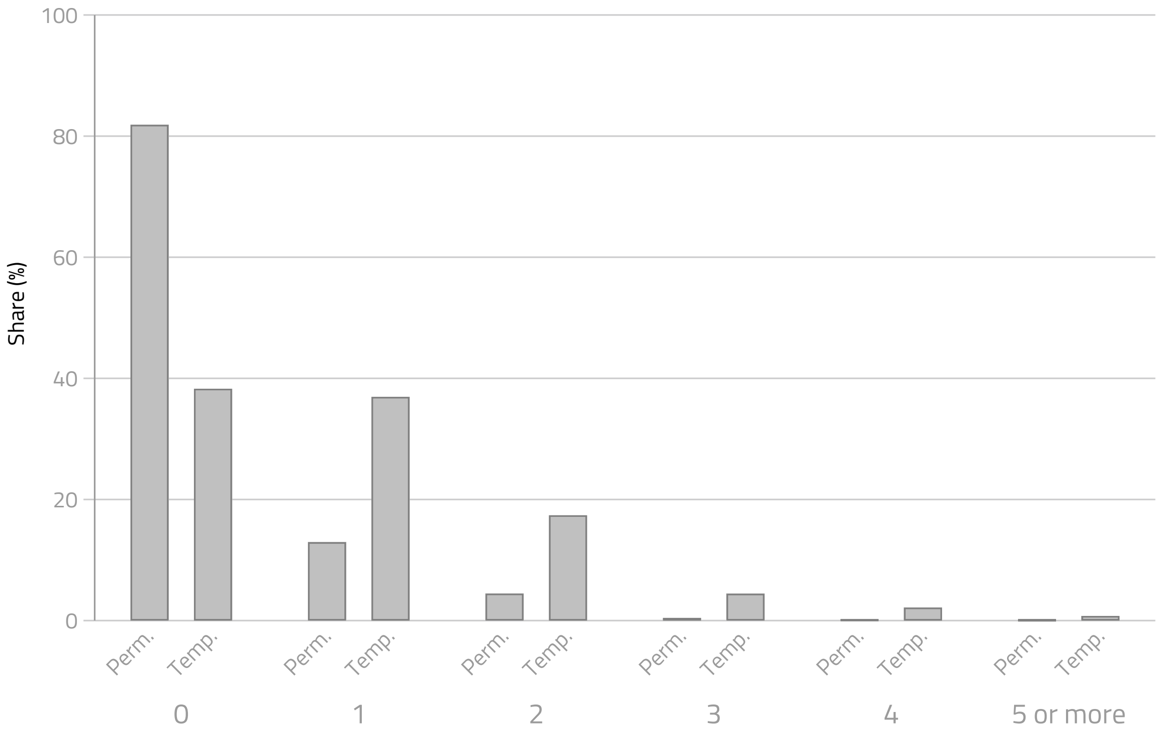
Finally, it may still be possible that workers time the *start* of their temporary contract, so that it ends after the fiftieth birthday. In this case the worker would be trading off a longer job search before starting the "right" temporary contract and longer PBD afterwards. If this is true, we would expect pervasive manipulation and extensive margin responses among temporary workers. Furthermore, the manipulation region below the threshold would likely be larger, as workers may move from several months before the age-fifty threshold up to several months after the threshold, depending on the duration of the contract they select. The distribution reported in Figure 4b does not provide evidence suggestive that this kind of behavior might be in place.

Figure B1. Survival in UI by contract type for workers in the private sector



Note: The figure shows the survival in UI for workers on temporary and permanent contracts at the time of layoff. The data includes workers from the private sector, laid off between Feb 2009 to Dec 2012 and between 46 and 50 years of age at the time of layoff.

Figure B2. Number of times workers received UI in the two years before layoff



Note: The figure reports how many times workers received UI in the two years preceding the current layoff by contract type. The data includes workers from the private sector, laid off between Feb 2009 to Dec 2012 and between 49 and 50 years of age at the time of layoff, to focus more on individuals who might engage in manipulation.

C Theory

This appendix lays out our full model of differentiated unemployment insurance with manipulation.

C.1 Setting

We assume there are two groups of individuals, referred to as the "young" and the "old" and denote their exogenous share in the population by G and $1 - G$, respectively. Young and old individuals differ in their utility of consumption, job search costs and their ability to manipulate (more on this below). All individuals are unemployed in $t = 0$, retire at a finite time horizon T and are hand-to-mouth consumers.[45]

The government provides unemployment benefits b, financed through a lump-sum UI tax τ. Young and old individuals enjoy consumption $c_u + b$ when unemployed and covered by UI, c_u when unemployed and not covered, and $c_e = w - \tau$ when employed, where w denotes the exogenous wage rate. The governments sets two separate UI schemes of varying generosity characterized by two different potential benefit durations P_y and P_o, with $P_o \geq P_y$. It targets the longer potential UI benefit duration P_o to the old. When doing so it faces a challenge: young individuals have the ability to manipulate their eligibility status (at some cost) and might endogenously select into the more generous scheme intended for the old.[46] In order to study how a benevolent government should optimally set P_y and P_o in this context, we begin by formally stating individuals' job search problems.

C.2 The Old

Preferences and Job Search. Old workers are homogeneous, always eligible for longer potential benefit duration P_o, and face the standard job search problem. They enjoy flow utility $u^o(c)$ at consumption level c and choose job search intensity s_t^o at time t, normalized to the arrival rate of job offers, at utility cost $\phi_t^o(s_t^o)$. Formally, old individuals maximize:

$$V^o(P_o) = \max_{s_t^o} \left\{ \int_0^{P_o} S_t^o u^o(c_u + b) + \int_{P_o}^{T} S_t^o u^o(c_u) + \int_0^{T} \left(1 - S_t^o\right) u^o(c_e) - \int_0^{T} S_t^o \phi_t^o(s_t^o) \right\},$$

where $S_t^o = exp\left(-\int_0^t s_{t'}^o dt'\right)$ denotes the nonemployment survival probability at time

[45] The model setup closely follows previous work on optimal potential benefit duration in UI, e.g. Schmieder et al. (2012) and Gerard and Gonzaga (2021).

[46] Since the two policies differ only in terms of their potential benefit duration, with $P_o \geq P_y$, we w.l.o.g. restrict attention to one-sided manipulation.

t and all integrals are taken w.r.t. dt. We denote the old's implied benefit and nonemployment duration by

$$B^o(P_o) = \int_0^{P_o} S_t^o(P_o)dt \quad \text{and} \quad D^o(P_o) = \int_0^{T} S_t^o(P_o)dt.$$

C.3 The Young

Preferences and Job Search. Young individuals have heterogeneous preferences and are characterized by utility of consumption u^i, job search cost function ϕ^i and fixed cost q^i. Conditional on eligibility for potential benefit duration $\tilde{P}$, young individuals maximize search effort as follows:

$$\tilde{V}^i(\tilde{P}) = \max_{s_t^i} \left\{ \int_0^{\tilde{P}} S_t^i u^i(c_u + b) + \int_{\tilde{P}}^{T} S_t^i u^i(c_u) + \int_0^{T} \left(1 - S_t^i\right) u^i(c_e) - \int_0^{T} S_t^i \phi_t^i(s_t^i) \right\},$$

where $S_t^i = exp\left(-\int_0^t s_{t'}^i dt'\right)$ denotes individuals' nonemployment survival probability at time t and all integrals are w.r.t. dt. Denote an individual's implied benefit and nonemployment duration by:

$$B^i(\tilde{P}) = \int_0^{\tilde{P}} S_t^i(\tilde{P})dt \quad \text{and} \quad D^i(\tilde{P}) = \int_0^{T} S_t^i(\tilde{P})dt.$$

Manipulation. At time zero, young individuals can engage in manipulation by incurring a fixed cost $q^i \geq 0$ to become eligible for potential benefit duration P_o rather than P_y, with $P_o \geq P_y$. Formally, a young individual i with fixed cost q^i maximizes:

$$\begin{aligned} V^i(P_o, P_y) &= \max_{a^i \in \{0,1\}} \left\{ \left(\tilde{V}^i(P_o) - q^i\right) \cdot \mathbb{1}_{a^i=1} + \tilde{V}^i(P_y) \cdot \mathbb{1}_{a^i=0} \right\} \\ &= \tilde{V}^i(P_y) + \max_{a^i \in \{0,1\}} \left\{ \left(\tilde{V}^i(P_o) - \tilde{V}^i(P_y) - q^i\right) \cdot \mathbb{1}_{a^i=1} \right\}, \end{aligned}$$

where a^i encodes the choice of whether ($a^i = 1$) or not ($a^i = 0$) to manipulate. Thus, young individual i manipulates if and only if

$$q^i \leq \bar{q}^i(P_o, P_y) \equiv \tilde{V}^i(P_o) - \tilde{V}^i(P_y). \tag{21}$$

Preferences and fixed costs are distributed according to a continuously differentiable pdf $f(u^i, \phi^i, q^i)$. We denote the share of young individuals who manipulate – henceforth manipulators – by $M(P_o, P_y))$ and the benefit and nonemployment durations of manipulators and non-manipulators respectively by:

$$B^m(P_o, P_y) = \mathbb{E}\left[B^i(P_o)|a^i(P_o, P_y)) = 1\right] \quad \text{and} \quad D^m(P_o, P_y)) = \mathbb{E}\left[D^i(P_o)|a^i(P_o, P_y)) = 1\right],$$
$$B^n(P_o, P_y)) = \mathbb{E}\left[B^i(P_y)|a^i(P_o, P_y)) = 0\right] \quad \text{and} \quad D^n(P_o, P_y)) = \mathbb{E}\left[D^i(P_y)|a^i(P_o, P_y)) = 0\right].$$

The average benefit and nonemployment durations for the young are

$$B^y(P_o, P_y)) = M(P_o, P_y)) \cdot B^m(P_o, P_y)) + \left(1 - M(P_o, P_y))\right) \cdot B^n(P_o, P_y))\big)$$
$$D^y(P_o, P_y)) = M(P_o, P_y)) \cdot D^m(P_o, P_y)) + \left(1 - M(P_o, P_y))\right) \cdot D^n(P_o, P_y))\big),$$

and we denote by $V^y(P_o, P_y) = \mathbb{E}\left[V^i(P_o, P_y)\right]$ the average utility of the young and use superscripts to denote conditional expectation operators.

C.4 The Planner's Problem

A benevolent social planner sets (P_o, P_y) to maximize ex-ante social welfare taking into account the incentive constraints, including the fact that manipulation might occur. Concretely, the planner's objective is given by:

$$W(P_o, P_y) = (1 - G) \cdot V^o(P_o) + G \cdot V^y(P_o, P_y),$$

subject to the budget constraint:

$$L \cdot \tau = U \cdot b + R,$$

with total labor supply $L = (1-G)(T - D^o(P_o)) + G(T - D^y(P_y)) + GM(D^m(P_y) - D^m(P_o))$, total unemployment covered by unemployment benefits $U = (1 - G)B^o(P_o) + GB^y(P_y) + GM(B^m(P_o) - B^m(P_y))$ and exogenous government spending R.

C.5 Simplifying Assumptions

We assume that the planner's optimization problem is well-behaved warranting a first-order approach. In order to ease the exposition and gain tractability, we impose two additional simplifying assumptions: The first corresponds to a constant elasticity assumption while the second restricts dynamic screening opportunities. The formal derivations in Appendix D make explicit how each assumption is used and how our results generalize. To state our assumptions precisely we introduce two key concepts for the analysis.

The first is a measure of the disincentive or moral hazard effect of UI in the context of extended potential benefit duration (PBD). Note that in the case of PBD, extra statutory coverage may mechanically lead to higher benefit receipts if individuals stay unemployed during the additional months with and without extra coverage. This cost increase for the government is not due to distorted job search incentives but simply reflects nonzero exhaustion risks during the relevant months of nonemployment. Because there is no distortion, such mechanical transfer does not matter, by itself, for the efficiency cost of UI. What matters is by how much individuals change their behavior, and thereby increase the cost of UI, for each dollar of such mechanical transfers.

Concretely, we follow Schmieder and von Wachter (2017) and define the behavioral to mechanical cost ratio for individual i when marginally increasing PBD P as:

$$\frac{BC_P^i}{MC_P^i} = \frac{b \cdot \int_0^P \frac{dS_t^i}{dP} dt + \tau \cdot \int_0^T \frac{dS_t^i}{dP} dt}{b \cdot S_P^i}. \tag{22}$$

The above BC/MC ratio has a classical leaking bucket interpretation. It captures by how many additional dollars total UI expenditure goes up for each dollar of mechanical transfer from the government to the unemployed.[47] We illustrate BC/MC ratios graphically in Figure 2 and refer to it simply as moral hazard throughout.

Second, we define the "marginal" utility of individual i at the point of benefit exhaustion $\tilde{u}_i'$ as

$$\tilde{u}_i' = \frac{1}{b} \int_0^b (u^i)'(c_u + x) dx = \frac{u^i(c_u + b) - u^i(c_u)}{b}. \tag{23}$$

Since we are working with benefit duration extensions, the relevant utility gap is between receiving and not receiving UI benefits during unemployment (which are the numeraire in the right-most term in equation (23)). We conveniently recast this gap into the appropriately weighted marginal utility. Note that neither consumption nor utilities are time dependent in the current setup which makes (23) time-invariant. However, it is straightforward to allow for time dependence in utility and consumption.

Equipped with the above concepts we impose the following assumptions. First, we assume a constant, i.e. time-invariant, moral hazard cost for the young. Concretely, we assume:

Assumption 1. *Moral hazard is constant over the UI spell. Formally, for each I subset of the young we have*

$$\frac{BC_P^I}{MC_P^I} = \frac{BC_{P'}^I}{MC_{P'}^I} \quad \text{for all } P, P'.$$

[47] An important property of this measure of moral hazard is its comparability across different (groups of) individuals. This is especially important in the context of unemployment duration because individuals might have heterogeneous exhaustion risk and thus face different incentives to respond to PBD extensions. As in previous work, it turns out that it is precisely this re-scaled moral hazard effect that is relevant for optimal policy in our setting.

Second, we assume that exhaustion risks and marginal utilities are uncorrelated.

Assumption 2. *Exhaustion risks and marginal utilities are uncorrelated: Formally, for all I subset of the young we have*

$$\mathbb{C}ov^I \left[S^i_{\tilde{P}}, \tilde{u}'_i \right] = 0 \ \textit{for all} \ \tilde{P}.$$

Assumption 1 is akin to a constant elasticity assumption. It requires that the behavioral to mechanical cost ratio remains constant over the UI spell which intuitively assumes a time-invariant responsiveness to UI transfers. Assumption 2 implies that exhaustion risks are uninformative of marginal utilities. On the one hand, high-marginal utility individuals might have stronger incentives to find a job which would violate assumption 2. However, to the extent that unemployed individuals deplete their assets over the UI spell, marginal utilities might in fact increase over the spell which would push the correlation in the opposite direction. Assumption 2 thus requires that such forces exactly offset each other. Both assumptions are assumptions on individual behavior but also implicitly restrict the space of possible selection pattern among the young because they have to hold for each subset of the young.[48] This makes the analysis considerably more tractable but rules out dynamic screening possibilities. For instance, exhaustion risks cannot be used to dynamically screen high marginal utility individuals. We regard our simplified setup as a natural starting point for the analysis and leave its generalization to future work.

C.6 Characterizing Optimal Policies

We parameterize policy $(P_o, P_y) = (P + \Delta P, P)$, such that P represents the level of baseline coverage and $\Delta P \geq 0$ reflects the amount of extra coverage. Before turning to the full optimum, we briefly focus on two related (sub-)problems that help building intuition. First we look at the case without manipulation.

Optimum without Manipulation. In Appendix D we show that the optimal policy in the absence of manipulation opportunities is given by:

Proposition 2 (Optimum without manipulation)**.** *The optimal policy (P_o^*, P_y^*) without manipulation, i.e. $M \equiv 0$, satisfies:*

$$\frac{\tilde{u}'_o - \bar{u}'}{\bar{u}'} = \frac{BC^o}{MC^o} \quad \textit{and} \quad \frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} = \frac{BC^y}{MC^y},$$

where $\bar{u}' = (1-G) \cdot (T - D^o) \cdot (u^o)'(c_e) + G \cdot (T - D^y) \cdot (u^y)'(c_e)$ is the average marginal utility of the employed and $\tilde{u}'_j$ and $\frac{BC^j}{MC^j}$ defined in (23) and (22) for $j = y, o$.

Proposition 2 follows previous results in the literature on optimal UI benefit duration,

[48]It suffices if assumptions 1 and 2 hold for all possible sets of manipulators and non-manipulators.

e.g. Schmieder and von Wachter (2017). As in the classical Baily-Chetty formula, the optimal policy without manipulation equates consumption smoothing benefits with moral hazard costs for the old and the young *separately*.[49]

Introducing Manipulation. To build further intuition, we now study the introduction of manipulation by first imagining a world without the old, i.e. $G = 1$. The extra coverage ΔP now simply represents an alternative contract into which some of the young might self-select. As we show in Appendix D, the (re-scaled) welfare effect of marginally increasing extra coverage ΔP starting from the case where there is none $\Delta P = 0$ is given by:

$$\frac{1}{M \cdot MC^m_{P_y} \cdot \bar{u}} \cdot \left.\frac{dW}{d(\Delta P)}\right|_{\Delta P=0} = \frac{\tilde{u}'_m - \bar{u}'}{\bar{u}'} - \frac{BC^m}{MC^m} \tag{24}$$

What matters for welfare at the margin is the insurance surplus, that is the difference between the consumption smoothing benefits and the moral hazard cost, of manipulators. It is instructive to evaluate this expression at the optimal manipulation-free policy P^*_y from Proposition 2.

Proposition 3 (The marginal welfare effect of manipulation at P^*_y)**.** *The marginal budget-balanced welfare effect of increasing extra coverage at P^*_y from Proposition 2 is given by:*

$$\frac{1}{M \cdot MC^m_{P_y} \cdot \bar{u}} \cdot \left.\frac{dW(P^*_y)}{d(\Delta P)}\right|_{\Delta P=0} = \underbrace{\left(\frac{\tilde{u}'_m - \tilde{u}'_y}{\bar{u}'}\right)}_{\substack{\text{selection on consumption} \\ \text{smoothing value}}} - \underbrace{\left(\frac{BC^m}{MC^m} - \frac{BC^y}{MC^y}\right)}_{\substack{\text{selection on moral} \\ \text{hazard cost}}}$$

Proposition 3 shows that the welfare effect of additional coverage depends on the extent to which manipulators are selected on consumption smoothing value and moral hazard cost at the optimally set manipulation-free policy P^*_y. If manipulators have higher insurance surplus than the average young individual, manipulation increases welfare and vice versa. This result mimics that of Hendren et al. (2021) who study the welfare effect of allowing for choice in a social insurance context.[50]

It turns out that selection effects, like the one in Proposition 3, remain crucial for determining the full optimal policy with manipulation which we turn to next.

Optimum with Manipulation. We now analyze the design of optimal policy in the presence of both groups young and old, i.e. $G \in (0, 1)$ *and* with (potential) nonzero manipulation.

[49]Note that the current setup imposes a common tax rate for the old and the young and the problem is thus not entirely separable across groups. It is straightforward to allow for different tax schedules across groups.

[50]While Hendren et al. (2021) are interested in price surcharges required for extra coverage, a feature one could also include in our setup, we model manipulation as an entirely private choice without any *direct* financial implications for the government. The manipulation fixed cost q^i is relevant for individual utilities but not for government revenue.

At the optimum, small budget-neutral changes $d\Delta P$ in extra coverage ΔP which cannot increase welfare. In Appendix D we show that this implies

$$(1-G)\cdot S^{o}_{P_o}\cdot\left[\frac{\tilde{u}'_o-\bar{u}'}{\bar{u}'}-\frac{BC^o}{MC^o}\right]+G\cdot M\cdot S^{m}_{P_o}\cdot\left[\frac{\tilde{u}'_m-\bar{u}'}{\bar{u}'}-\frac{BC^m}{MC^m}\right]$$
$$+G\cdot(1-M)\cdot S^{n}_{P_y}\cdot\epsilon_{1-M,\Delta P}=0, \tag{25}$$

where all variables are defined as above and $\epsilon_{1-M,\Delta P}$ refers to the cost-weighted elasticity of manipulation w.r.t. extra coverage ΔP which we define formally below. Equation (25) generalizes equation (24) by introducing two additional terms (the first and third term). The first term takes into account that the old, who are always entitled to receiving higher coverage P_o, have a direct welfare effect from increases in extra coverage. The third terms captures the fact that marginal manipulators might cause non-marginal changes in the government budget, because we are no longer starting at a point without any additional coverage. Concretely, define the fiscal externality from manipulation, that is the budgetary cost arising from higher benefit receipt and lower tax revenue, of all individuals of type $i=(u_i,\phi_i)$ as

$$FE^i=\Big(B^i(P+\Delta P)-B^i(P)\Big)\cdot b+\Big(D^i(P+\Delta P)-D^i(P)\Big)\cdot\tau \tag{26}$$

and the share of these individuals who end up manipulating because their fixed cost falls below the threshold $\bar{q}^i$ in equation (21), as

$$M^i=\int_0^{\bar{q}^i} f(q|u_i,\phi_i)dq. \tag{27}$$

Equipped with these two quantities we formally define the cost-weighted elasticity of manipulation w.r.t. extra coverage introduced in equation (25) as follows

$$\epsilon_{1-M,\Delta P}=\mathbb{E}^n\left[\frac{FE^i}{MC^n_{P_y}\cdot\Delta P}\cdot\epsilon_{1-M^i,\Delta P}\right]. \tag{28}$$

Thus the elasticity term captures by how much each share M^i, as measured by $1-M^i$, responds to increases in extra coverage weighted by the cost that such changes impose on the government budget.

Turning to the optimal level of baseline coverage, we again have that at the optimum, marginal budget-neutral changes dP in baseline coverage P cannot increase welfare. As shown in Appendix D, by the envelope theorem this implies

$$
\begin{aligned}
&(1-G)\cdot S^o_{P_o}\cdot\left[\frac{\tilde{u}'_o-\bar{u}'}{\bar{u}'}-\frac{BC^o}{MC^o}\right]+G\cdot S^y_{P_y}\cdot\left[\frac{\tilde{u}'_y-\bar{u}'}{\bar{u}'}-\frac{BC^y}{MC^y}\right]\\
&+G\cdot M\cdot\left(S^m_{P_o}-S^m_{P_y}\right)\cdot\left[\frac{\tilde{u}'_m-\bar{u}'}{\bar{u}'}-\frac{BC^m}{MC^m}\right]+G\cdot(1-M)\cdot S^n_{P_y}\cdot\epsilon_{1-M,P}=0,
\end{aligned}
\tag{29}
$$

where $\epsilon_{1-M,P}$ is the cost-weighted elasticity of manipulation w.r.t. baseline coverage P, defined analogously as in equation (28) but with respect to baseline coverage P. Intuitively, when deciding how much baseline coverage to provide, the planners weighs the surplus from the old (first term), the young (second term), an adjustment accounting for the fact that a subset of the young are in fact manipulators with now different exhaustion risk (third term) and the effect of baseline coverage on the extent of manipulation (fourth term).

Combining equations (25) and (29) leads to our main proposition regarding the optimal policy under manipulation.

Proposition 4 (Optimum with manipulation)**.** *The optimal policy with manipulation satisfies:*

$$
\begin{aligned}
\frac{\tilde{u}'_y-\bar{u}'}{\bar{u}'}-\frac{BC^y}{MC^y}=\;&\underbrace{\epsilon_{1-M,\Delta P}}_{\substack{\textit{manipulation externality}\\\textit{of extra coverage}}}-\underbrace{\epsilon_{1-M,P}}_{\substack{\textit{manipulation externality}\\\textit{of baseline coverage}}}\\
&+M\cdot\underbrace{\left(\frac{S^m_{P_y}}{S^y_{P_y}}\right)}_{\substack{\textit{selection on risk}\\\textit{scale factor}}}\cdot\left\{\underbrace{\left(\frac{\tilde{u}'_m-\tilde{u}'_n}{\bar{u}'}\right)}_{\substack{\textit{selection on consumption}\\\textit{smoothing value}}}-\underbrace{\left(\frac{BC^m}{MC^m}-\frac{BC^n}{MC^n}\right)}_{\substack{\textit{selection on moral}\\\textit{hazard cost}}}\right\}
\end{aligned}
\tag{30}
$$

and

$$
\begin{aligned}
&(1-G)\cdot S^o_{P_o}\cdot\left(\frac{\tilde{u}'_o-\bar{u}'}{\bar{u}'}-\frac{BC^o}{MC^o}\right)+G\cdot S^y_{P_o}\cdot\left(\frac{\tilde{u}'_y-\bar{u}'}{\bar{u}'}-\frac{BC^y}{MC^y}\right)\\
&=G\cdot(1-M)\cdot\left(\left(S^n_{P_o}-S^n_{P_y}\right)\cdot\epsilon_{1-M,\Delta P}-S^n_{P_o}\cdot\epsilon_{1-M,P}\right)
\end{aligned}
\tag{31}
$$

Note that Equation (30) corresponds to Equation (20) in the main text. For the case of no manipulation, i.e. $M=0$, Proposition 4 nests Proposition 2. However, the presence of manipulation induces a wedge in the provision of insurance for both young and old. Equation (20) shows that the wedge for the young is determined by two elasticities, namely that of extra and baseline coverage, and by a selection term, capturing the extent to which manipulators are selected on consumption smoothing value and moral hazard cost. Equation (31) implies that the wedge for the old is the direct counterpart of that for the young together with an effect on the overall level of insurance (RHS). In order to build intuition, it is instructive to consider two special cases.

Fixed, nonzero M. First, consider a scenario in which a fixed subset of young individuals manipulate irrespectively of policy and always obtain higher UI coverage. In this case the share M is nonzero and unresponsive to the design of UI. As a consequence, all elasticity terms in Proposition 4 are zero. It is straightforward to show that equation (20) implies that

$$\frac{\tilde{u}'_n - \bar{u}'}{\bar{u}'} - \frac{BC^n}{MC^n} = 0, \tag{32}$$

which means that consumption smoothing benefits and moral hazard cost for non-manipulators or the 'endogenous young' are equated. Similarly equation (25) shows the same holds true for the 'endogenous old', i.e. the group of the old and manipulators. Note that equation (31) implies that such manipulation induces no distortion in the desired overall level of insurance. Intuitively, this is a case of pure re-labelling, in which the planner regards a subset of the young as old because their manipulation choice is unresponsive to policy.

Homogeneous young. Suppose there is no heterogeneity among the young, except potentially in their manipulation fixed cost. In this case the selection term in equation (20) vanishes and the wedge of the young is governed only by the two elasticities. If one assumes that additional coverage weakly increases the share of manipulators and that additional baseline coverage weakly decreases it, then the wedge of the young is unambiguously negative, calling for overinsurance. Intuitively, it is optimal for the planner to grant the young additional surplus, above and beyond their manipulation-free level, because of their manipulation threat. To the extent that additional coverage mitigates manipulation the planner finds it optimal to provide such insurance to the young. Contrary, by equation (31), the old will be underinsured by more than the wedge for the young representing the fact that shifting insurance surplus is now costly due to the fiscal externality associated with manipulation.

D Proofs and Derivations

This appendix lays out the formal derivation of Proposition 4, which implies Proposition 2 for $M \equiv 0$. We further illustrate how to derive equation (24) as well as Proposition 3.

The government problem parameterized with baseline coverage P and extra coverage ΔP, such that $P_y = P$ and $P_o = P + \Delta P$, reads:

$$\max_{P,\Delta P,\tau} W = (1-G)\cdot V^o(P+\Delta P) + G\cdot \mathbb{E}^y\left[\tilde{V}^i(P)\right] + G\cdot M\cdot \mathbb{E}^m\left[\tilde{V}^i(P+\Delta P) - \tilde{V}^i(P) - q^i\right]$$

subject to the budget constraint:

$$\begin{aligned}
&\tau\cdot[(1-G)\cdot(T - D^o(P+\Delta P)) + G\cdot(T - D^y(P)) + G\cdot M\cdot(D^m(P) - D^m(P+\Delta P))]\\
&= b\cdot[(1-G)\cdot B^o(P+\Delta P) + G\cdot B^y(P) + G\cdot M\cdot(B^m(P+\Delta P) - B^m(P))] + R.
\end{aligned}$$

When considering small changes in extra coverage ΔP we may, by the envelope theorem, ignore all direct welfare effects of changes in job search intensities or manipulation choices.[51] Thus, small budget-neutral changes in ΔP have a welfare effect of:

$$
\begin{aligned}
\frac{dW}{d\Delta P} &= (1-G)\cdot\frac{dV^o(P_o)}{d\Delta P} + G\cdot M\cdot\mathbb{E}^m\left[\frac{d\tilde{V}^i(P_o)}{d\Delta P}\right] - \bar{u}'\cdot L\cdot\frac{d\tau}{d\Delta P} && (33)\\
&= (1-G)\cdot S^o_{P_o}\cdot\left(u^o(c_u+b)-u^o(c_u)\right)\\
&+ G\cdot M\cdot\mathbb{E}^m\left[S^i_{P_o}\cdot\left(u^i(c_u+b)-u^i(c_u)\right)\right]\\
&- \bar{u}'\cdot\left[(1-G)\cdot\left(BC^o_{P_o}+MC^o_{P_o}\right)+G\cdot M\cdot\left(BC^m_{P_o}+MC^m_{P_o}\right)\right]\\
&+ \bar{u}'\cdot G\cdot b\cdot(1-M)\cdot\epsilon_{1-M,\Delta P} && (34)\\
&= (1-G)\cdot\left(MC^o_{P_o}\cdot\tilde{u}'_o-\left(BC^o_{P_o}+MC^o_{P_o}\right)\cdot\bar{u}'\right)\\
&+ G\cdot M\cdot\left(\mathbb{E}^m\left[MC^i_{P_o}\cdot\tilde{u}'_i\right]-\left(BC^m_{P_o}+MC^m_{P_o}\right)\cdot\bar{u}'\right)\\
&+ G\cdot(1-M)\cdot MC^n_{P_y}\cdot\bar{u}'\cdot\epsilon_{1-M,\Delta P}, && (35)
\end{aligned}
$$

where we used the implicit differentiation of the government budget constraint $\tau\cdot L = b\cdot B + R$ and Leibniz rule to obtain:

$$
\begin{aligned}
L\cdot\frac{d\tau}{d\Delta P} &= b\cdot\frac{dB}{d\Delta P}-\tau\cdot\frac{dL}{d\Delta P} && (36)\\
&= (1-G)\cdot\left(b\cdot\frac{dB^o}{d\Delta P}+\tau\cdot\frac{dD^o}{d\Delta P}\right)\\
&+ G\cdot\frac{d}{d\Delta P}\int_i \underbrace{\left(b\cdot\left(B^i(P_o)-B^i(P_y)\right)+\tau\cdot\left(D^i(P_o)-D^i(P_y)\right)\right)}_{=FE^i\text{ by equation (26)}}\cdot\mathbb{I}_{q^i\le\bar{q}^i}\,df(u^i,\psi^i,q^i) && (37)\\
&= (1-G)\cdot\left(BC^o_{P_o}+MC^o_{P_o}\right)+G\cdot\int_i\frac{dFE^i}{d\Delta P}\cdot\mathbb{I}_{q^i\le\bar{q}^i}\,df(u^i,\psi^i,q^i)\\
&+ G\cdot\int_{u^i,\phi^i}FE^i\cdot\frac{d}{d\Delta P}\underbrace{\int_0^{\bar{q}^i}f(q|u_i,\phi_i)\,dq}_{=M^i\text{ by equation (27)}}\,df(u^i,\phi^i) && (38)\\
&= (1-G)\cdot\left(BC^o_{P_o}+MC^o_{P_o}\right)+G\cdot M\cdot\left(BC^m_{P_o}+MC^m_{P_o}\right)\\
&- G\cdot MC^n_{P_y}\cdot\int_{u^i,\phi^i}\frac{(1-M^i)\cdot FE^i}{MC^n_{P_y}\cdot\Delta P}\cdot\epsilon_{1-M^i,\Delta P}\,df(u^i,\phi_i) && (39)\\
&= (1-G)\cdot\left(BC^o_{P_o}+MC^o_{P_o}\right)+G\cdot M\cdot\left(BC^m_{P_o}+MC^m_{P_o}\right)\\
&- G\cdot(1-M)\cdot MC^n_{P_y}\cdot\epsilon_{1-M,\Delta P}, && (40)
\end{aligned}
$$

[51] These changes matter only to the extent that they operate through the government budget constraint.

by the definition in equation (28). Exploiting assumptions 1 and 2, we rewrite (35) as:

$$\begin{aligned}\frac{1}{\bar{u}\cdot b}\cdot\frac{dW}{d\Delta P} =\ & (1-G)\cdot S^{o}_{P_o}\left(\frac{\tilde{u}'_o-\bar{u}'}{\bar{u}'}-\frac{BC^o}{MC^o}\right)\\ &+\ G\cdot M\cdot S^{m}_{P_o}\left(\frac{\tilde{u}'_m-\bar{u}'}{\bar{u}'}-\frac{BC^m}{MC^m}\right)\\ &+\ G\cdot(1-M)\cdot S^{n}_{P_y}\cdot\epsilon_{1-M,\Delta P},\end{aligned} \tag{41}$$

which proves equation (25) in the main text.

Similarly, small budget-neutral changes in baseline coverage P have a welfare effect of:

$$\begin{aligned}\frac{dW}{dP} =\ & (1-G)\cdot\frac{dV^o(P_o)}{dP}+G\cdot\mathbb{E}^y\left[\frac{d\tilde{V}^i(P_o)}{dP}\right]\\ &+\ G\cdot M\cdot\mathbb{E}^m\left[\frac{d\tilde{V}^i(P_o)}{dP}-\frac{d\tilde{V}^i(P_y)}{dP}\right]-\bar{u}'\cdot L\cdot\frac{d\tau}{dP}\end{aligned} \tag{42}$$

$$\begin{aligned} =\ & (1-G)\cdot\left(MC^o_{P_o}\cdot\tilde{u}'_o-\left(BC^o_{P_o}+MC^o_{P_o}\right)\cdot\bar{u}'\right)\\ &+\ G\cdot\left(MC^y_{P_y}\cdot\tilde{u}'_y-\left(BC^y_{P_y}+MC^y_{P_y}\right)\cdot\bar{u}'\right)\\ &+\ G\cdot M\cdot\left(\mathbb{E}^m\left[MC^i_{P_o}\cdot\tilde{u}'_i\right]-\left(BC^m_{P_o}+MC^m_{P_o}\right)\cdot\bar{u}'\right)\\ &-\ G\cdot M\cdot\left(\mathbb{E}^m\left[MC^i_{P_y}\cdot\tilde{u}'_i\right]-\left(BC^m_{P_y}+MC^m_{P_y}\right)\cdot\bar{u}'\right)\\ &+\ G\cdot(1-M)\cdot MC^n_{P_y}\cdot\bar{u}'\cdot\epsilon_{1-M,P},\end{aligned} \tag{43}$$

where, again, we used the implicit differentiation of the government budget constraint $\tau\cdot L=b\cdot B+R$ and Leipniz rule to obtain:

$$L\cdot\frac{d\tau}{dP} = b\cdot\frac{dB}{dP}-\tau\cdot\frac{dL}{dP} \tag{44}$$

$$\begin{aligned} =\ & (1-G)\cdot\left(b\cdot\frac{dB^o}{dP}+\tau\cdot\frac{dD^o}{dP}\right)+G\cdot\left(b\cdot\frac{dB^y}{dP}+\tau\cdot\frac{dD^y}{dP}\right)\\ &+\ G\cdot\frac{d}{dP}\int_i FE^i\cdot\mathbb{I}_{q^i\leq\bar{q}^i}\ df(u^i,\psi^i,q^i)\end{aligned} \tag{45}$$

$$\begin{aligned} =\ & (1-G)\cdot\left(BC^o_{P_o}+MC^o_{P_o}\right)+G\cdot\left(BC^y_{P_y}+MC^y_{P_y}\right)\\ &+\ G\cdot\int_i\frac{dFE^i}{dP}\cdot\mathbb{I}_{q^i\leq\bar{q}^i}\ df(u^i,\psi^i,q^i)+G\cdot\int_{u^i,\phi^i}FE^i\cdot\frac{dM^i}{dP}\ df(u^i,\phi^i)\end{aligned} \tag{46}$$

$$\begin{aligned} =\ & (1-G)\cdot\left(BC^o_{P_o}+MC^o_{P_o}\right)+G\cdot\left(BC^y_{P_y}+MC^y_{P_y}\right)\\ &+\ G\cdot M\cdot\left[\left(BC^m_{P_o}+MC^m_{P_o}\right)-\left(BC^m_{P_y}+MC^m_{P_y}\right)\right]-G\cdot(1-M)\cdot MC^n_{P_y}\cdot\epsilon_{1-M,P},\end{aligned} \tag{47}$$

and define

$$\epsilon_{1-M,P} := \mathbb{E}^n \left[\frac{FE^i}{MC^n_{P_y} \cdot P} \cdot \epsilon_{1-M^i,P} \right]. \tag{48}$$

Under assumptions 1 and 2, we may rewrite (43) to:

$$\begin{aligned} \frac{1}{\bar{u} \cdot b} \cdot \frac{dW}{dP} = &\ (1-G) \cdot S^o_{P_o} \left(\frac{\tilde{u}'_o - \bar{u}'}{\bar{u}'} - \frac{BC^o}{MC^o} \right) + G \cdot S^y_{P_y} \left(\frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} - \frac{BC^y}{MC^y} \right) \\ &+ G \cdot M \cdot \left(S^m_{P_o} - S^m_{P_y} \right) \cdot \left(\frac{\tilde{u}'_m - \bar{u}'}{\bar{u}'} - \frac{BC^m}{MC^m} \right) + G \cdot (1-M) \cdot S^n_{P_y} \cdot \epsilon_{1-M,P}, \end{aligned} \tag{49}$$

which proves equation (29) in the main text.

To prove equation (20) in Proposition 4, we substitute equation (25) into (29), which gives:

$$\begin{aligned} &S^y_{P_y} \cdot \left(\frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} - \frac{BC^y}{MC^y} \right) - M \cdot S^m_{P_y} \cdot \left(\frac{\tilde{u}'_m - \bar{u}'}{\bar{u}'} - \frac{BC^m}{MC^m} \right) \\ &+ (1-M) \cdot S^n_{P_y} \cdot \left(\epsilon_{1-M,P} - \epsilon_{1-M.\Delta P} \right) = 0. \end{aligned} \tag{50}$$

Noting that,

$$\begin{aligned} S^y_{P_y} \cdot \left(\frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} - \frac{BC^y}{MC^y} \right) = &\ M \cdot S^m_{P_y} \cdot \left(\frac{\tilde{u}'_m - \bar{u}'}{\bar{u}'} - \frac{BC^m}{MC^m} \right) \\ &+ (1-M) \cdot S^n_{P_y} \cdot \left(\frac{\tilde{u}'_n - \bar{u}'}{\bar{u}'} - \frac{BC^n}{MC^n} \right), \end{aligned} \tag{51}$$

we rewrite (50) to obtain:

$$\frac{\tilde{u}'_n - \bar{u}'}{\bar{u}'} - \frac{BC^n}{MC^n} = \epsilon_{1-M,\Delta P} - \epsilon_{1-M,P}. \tag{52}$$

For expositional ease we define

$$s = \frac{S^m_{P_y} - S^n_{P_y}}{S^n_{P_y}}, \tag{53}$$

and introduce shorthand notation for the social surplus from insurance for group $j \in \{n, m, y, o\}$:

$$SSP^j = \left(\frac{\tilde{u}'_j - \bar{u}'}{\bar{u}'} - \frac{BC^j}{MC^j} \right). \tag{54}$$

Finally, we rewrite (51) as follows:

$$\begin{aligned}
S^y_{P_y} \cdot SSP^y &= S^n_{P_y} \cdot SSP^n + M \cdot \left[S^m_{P_y} \cdot SSP^m - S^n_{P_y} \cdot SSP^n \right] \\
&= S^n_{P_y} \cdot SSP^n + M \cdot \left[S^m_{P_y} \cdot SSP^m - S^n_{P_y} \cdot SSP^n + S^n_{P_y} \cdot SSP^m - S^n_{P_y} \cdot SSP^m \right] \\
&= S^n_{P_y} \cdot SSP^n + M \cdot \left[S^n_{P_y} \cdot (SSP^m - SSP^n) + \left(S^m_{P_y} - S^n_{P_y} \right) \cdot SSP^m \right] \\
&= S^n_{P_y} \cdot (SSP^n + M \cdot (SSP^m - SSP^n) + s \cdot M \cdot SSP^m) \\
&= S^n_{P_y} \cdot ((1 + s \cdot M) \cdot SSP^n + (1 + s) \cdot M \cdot (SSP^m - SSP^n)) , \qquad (55)
\end{aligned}$$

which, since $S^y_{P_y} = S^n_{P_y} \cdot (1 + s \cdot M)$ and $\frac{1+s}{1+s \cdot M} = \frac{S^m_{P_y}}{S^y_{P_y}}$, implies

$$SSP^y \ = \ SSP^n + M \cdot \frac{S^m_{P_y}}{S^y_{P_y}} \cdot (SSP^m - SSP^n) . \qquad (56)$$

Substituting (52) and (54) completes the proof of equation (20) in Proposition 4.

Last, we derive equation (31) in Proposition 4 by rewriting (25) as:

$$\begin{aligned}
(1 - G) \cdot S^o_{P_o} \cdot SSP^o &= -G \cdot \left[M \cdot S^m_{P_o} \cdot SSP^m + (1 - M) \cdot S^n_{P_y} \cdot \epsilon_{1-M,\Delta P} \right] \\
&= -G \cdot \left[S^y_{P_o} \cdot SSP^y - (1 - M) \cdot S^n_{P_o} \cdot SSP^n + (1 - M) \cdot S^n_{P_y} \cdot \epsilon_{1-M,\Delta P} \right] ,
\end{aligned}$$

which with equation (52) implies:

$$\begin{aligned}
(1 - G) \cdot S^o_{P_o} \cdot SSP^o + G \cdot S^y_{P_o} \cdot SSP^y &= G \cdot (1 - M) \cdot \left[S^n_{P_o} \cdot SSP^n - S^n_{P_y} \cdot \epsilon_{1-M,\Delta P} \right] \\
&= G \cdot (1 - M) \cdot \left[\left(S^n_{P_o} - S^n_{P_y} \right) \cdot \epsilon_{1-M,\Delta P} - S^n_{P_o} \cdot \epsilon_{1-M,P} \right] ,
\end{aligned}$$

and concludes the proof.

E Additional Institutional Details

This section provides additional information about the Italian unemployment insurance schemes in place from 2009. Our main sample covers the period from February 2009 until December 2012. There were two alternative UI schemes in place simultaneously to the main OUB scheme which we study in our analysis.

E.1 Alternative UI schemes in Italy from 2009 to 2012

During the years from 2009 to 2012 two other UI schemes were in place: the Reduced Unemployment Benefits (RUB) and the Mobility Allowance (MA).[52]

The RUB scheme targeted similar workers as OUB albeit different contribution requirements. While still requiring the first contribution to social security to have happened at least two years before, the RUB scheme only required 13 weeks (78 days) of contributions over the past year (instead of 52 weeks within the last two years as in OUB). The milder eligibility requirements went hand in hand with less generous benefits. 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. Because RUB is significantly less generous it is unlikely to interfere with our analysis of the OUB.[53]

The MA scheme (active until 2017) and was targeted to workers fired during mass layoffs or business re-organizations. It provided long and generous income support with active labor market reintegration and retraining programs. During the period under study the potential duration of this scheme depended on the worker's age at layoff and geography, with a maximum PBD of 48 months in the south and of 36 months in northern regions. UI benefits amounted to 80% of the salary for the first 12 months (with a cap annually set by law) and 64% during the following months. MA benefits represented a particularly attractive alternative for individuals involved in mass layoffs and could be responsible for an under-representation of these types of workers in our sample. What is more relevant for our analysis however is that selection into MA is largely beyond the control of the worker. Indeed, eligible firms needed to be undergoing significant economic restructuring and have a minimum size, while workers needed to meet additional tenure requirements.

E.2 UI schemes in Italy after 2012

The Italian welfare system underwent significant reform after 2012 all aiming at reducing the fragmentation of benefit schemes. In January 2013, both the OUB and the RUB were

[52] *Indennità di Disoccupazione Ordinaria a Requisiti Ridotti* and *Indennità di Mobilità* in Italian, respectively.
[53] For additional information, please refer to Anastasia et al. (2009).

replaced respectively by the ASpI and MiniASpI.[54]

The ASpI mimicked many aspects of the OUB both in terms of requirements and structure. Eligibility requirements of the ASpI followed those of the OUB scheme. Potential benefit duration was also identical initially, however, it was reformed several times in 2014 and 2015 which makes it difficult to include the ASpI in our analysis. Benefit levels differed with a replacement rate of 75% for the first six month, 60% for month seven to twelve and 45% thereafter (all as fractions of the average wage in the preceding two years before layoff).

The MiniASpI was aimed at workers who did not meet the requirement for the ASpI, but had accumulated at least thirteen weeks of work in the last year. Potential benefit duration was equal to half of the weeks worked over that time period. Benefit receipt was proportional to past wages: workers received 75% of the average wage received during the two previous years.

Since April 2015, both measures are replaced by a single UI scheme which provides homogeneous coverage to workers from all types of layoffs. The new scheme, the NASpI, is based on the structure of the MiniASpI. To qualify, workers need at least 78 days of contributions in the year before layoff. Potential benefit duration is equal to half of the weeks worked over the previous four years. Benefit levels are proportional to past wages following a declining profile starting at 75% replacement rate with a 3 p.p. reduction for every month after the first four. Importantly for our analysis, there is no longer a discontinuity is potential benefit duration thus removing incentives for workers to delay their layoff.

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

F Additional Robustness Tests

This section provides additional evidence in support of the identifying assumptions. Concretely, our analysis assumes that the discontinuity in PBD around the age threshold affects layoff decisions only through the delay of otherwise earlier occurring layoffs. The main threat to this assumption is the possibility of *extensive margin responses*, i.e. increases in the rate of job separations due to the incentives generated by the UI system. This is worrisome for two reasons. First we would be mis-measuring the upper bound of the manipulation region (z_U). Second, if the extra layoffs are systematically different, we would be altering the composition of layoffs in the manipulation region for reasons other than manipulation, introducing bias.

Extensive margin responses to UI have been studied both theoretically, see e.g. early work by Feldstein (1976), Feldstein (1978) and Topel (1983), as well as in recent empirical work e.g. Albanese et al. (2020) and Jäger et al. (2023).

Albanese et al. (2020) find alleviated job separation rates as a response to the same Italian OUB scheme that we study but exploit the eligibility discontinuity of 52 contribution weeks within the last two years after which a worker qualifies for *any* UI. Although closely related there are several reasons why we might not find job separation effects in our context. Their variation is from zero to some PBD, whereas we study a PBD extension from a nonzero level. Because we are exploiting intensive rather than extensive margin incentives, extensive margin responses are likely significantly smaller. This is especially true because all workers in our sample are eligible for UI and have thus already "survived" the eligibility threshold Albanese et al. (2020) exploit.

The work by Jäger et al. (2023) documents job separation effects of a large PBD reform in Austria which raised PBD from one to four years. They exploit this large variation to form a test for the efficiency of job separations by studying differences in separation rates of surviving job cohorts that were differentially treated by the reform. Again, there are several reasons to caution against extrapolating from their setting to ours. First, the sheer size of the PBD extension in Austria was unusually large. Second, it was targeted at relatively old workers who, as Jäger et al. (2023) document, used it (in part) as a gateway into early retirement. Last, their setting is likely to produce larger extensive margin responses because the Austrian UI scheme covers voluntary quits and not just layoffs as in Italy.

Although there exists recent important evidence on the extensive margin job separation effects of UI programs we see reason to believe that such effects are significantly smaller or entirely absent in our context. Of course, the presence of job separation effects is ultimately an empirical question. In the following we provide three tests all of which support the absence of extensive margin responses in our setting.

F.1 Testing for Shifts in the Layoff Density

The first test is based on the shape of the layoff density. Concretely, we investigate whether there is a persistent increase in layoffs after the age fifty threshold. One might expect a persistent increase in the density if, for instance, firms that experience negative productivity shocks, dis-proportionally lay off workers above fifty due to the extended UI coverage. We operationalize this approach by estimating versions of a classical regression discontinuity design and estimate the following specification once for the entire sample and by excluding (an extended version of) the manipulation region:

$$d_j = \alpha + \lambda \cdot a_j + \gamma \cdot \mathbb{I}[a_j \geq 50] + \delta \cdot \mathbb{I}[a_j \geq 50] \cdot a_j + \nu_j, \tag{57}$$

where d_j denote the density of layoffs in two-week age bin j, a_j denotes the mid-point age and ν_j is an error term. The coefficient of interest γ is indicative of any discontinuity in the density at the age fifty threshold. While we expect a positive γ coefficient when estimating specification (57) capturing the presence of manipulation, once we (successfully) exclude the manipulation region, γ should be close to zero in the absence of extensive margin responses. This is precisely what we find with results of all three regressions presented in Table A1. Column 1 presents estimates from the full sample where we do find a positive and significant γ coefficient of 0.027, consistent with the visual evidence in Figure 3. More importantly, once we exclude the manipulation region in column 2, the estimated γ becomes indistinguishable from zero lending support to our identifying assumption. Column 3 repeats the previous analysis but with a modified definition of the manipulation region. Concretely, we extend the manipulation region to nine age bins prior to age fifty and four age bins after the threshold. The choice of this extended region is motivated by a simple quantitative heuristic. For the missing (excess) region we include the longest sequence of age bins from the threshold that are associated with negative (positive) regression coefficients in a simple OLS regression that allows for a separate effect of each age bin on the layoff frequency.[55] Reassuringly, the estimated γ coefficient in Table A1 remains quantitatively small and insignificant.

F.2 Testing for the presence of extra excess mass

In this section we provide a second test based on the empirical layoff density. This time we investigate the possibility that extensive margin responses are concentrated right after the threshold. Rather than leading to a persistent increase in the density, which we tested for in the preceding section, we are concerned with the presence of additional layoffs just after the threshold that are not due to re-timing. Indeed such additional layoffs might occur if there are jobs that "mature" into negative surplus and

[55] 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.

such separate precisely when the worker crosses the eligibility threshold for higher UI coverage. We probe this concern with the following analysis. First, in the absence of such additional layoffs missing and excess "mass", or numbers of manipulators, should balance exactly. If there are more excess manipulators one might be worried that these are the result of an extensive margin response thus violating our identifying assumption. We thus test the extent to which missing and excess mass balance around the threshold. To do so, we rely on the same definition of an extended manipulation region as in Section F.1. Concretely, we estimate the following specification

$$c_j = \alpha + \beta \cdot a_j + \sum_{k=A}^{50^-} \tilde{\gamma_k} \cdot \mathbb{I}[a_j = k] + \sum_{k=50^+}^{B} \tilde{\delta_k} \cdot \mathbb{I}[a_j = k] + \zeta_j, \tag{58}$$

where c_j corresponds to the number of layoffs in age bin j and a_j refers to the mid-point age in bin j. The set of $\tilde{\gamma_k}$ and $\tilde{\delta_k}$ coefficients capture the estimated number of manipulators in the respective bin k in the missing and excess region, respectively. The lower and upper bounds $A < z_L$ and $B > z_U$ are set to eighteen weeks (nine bins) and four eight weeks (four bins) as in the previous section. We calculate the difference between the sum of all $\tilde{\gamma}$ coefficients and the sum of all $\tilde{\delta}$ coefficients and re-scale it by the latter. The estimated 1.3% represents the share of the estimated manipulators in the excess region which is not explained by manipulators in the missing regions. Reassuringly, this number is very small lending further support to our main identification assumption.

F.3 Testing for discontinuities in observable characteristics

Last we turn to a set of robustness tests based on observable characteristics around the age threshold. Intuitively, observable characteristics around the age cutoff should also differ due to manipulation. Similar to the density test in Section F.1 we investigate if individuals differ based on their observable characteristics outside of the manipulation region. Concretely and for comparison, we run two regression models. The first is a standard regression discontinuity specification run on the full sample:

$$x_i = \alpha + \sum_{p=1}^{P} \lambda_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i < 50] + \sum_{p=0}^{P} \lambda_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i \geq 50] + \xi_i, \tag{59}$$

where x_i denotes individual i's characteristic, a_i denotes age and P refers to the degree of the polynomial, in our case 2. In this standard RD specification the coefficient $\lambda_0^{>50}$ captures the jump at the threshold and is thus the coefficient of interest. The second model adds indicator variables for each age bin in the manipulation region and is specified as follows:

$$
\begin{aligned}
x_i = \kappa + \sum_{p=1}^{P} \theta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i < 50] + \sum_{p=0}^{P} \theta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i \geq 50] \\
+ \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \nu_i,
\end{aligned}
\tag{60}
$$

where we use the main definition of the manipulation region, namely six weeks prior and four weeks after the age cutoff.

Each row of Table A2 reports the estimated $\lambda_0^{>50}$ coefficients from specification (59) and $\theta_0^{>50}$ coefficients from specification (60) for a given observable characteristics. Consistent with our main identifying assumption we find no significant estimates of $\theta_0^{>50}$ coefficients despite several of the estimates for $\lambda_0^{>50}$ being significant. Together these results show that once manipulation is taken into account, observable characteristics appear similar on either side of the age threshold, again consistent with the absence of extensive margin job separation effects.

G Additional Tables

Table A1. Test for discontinuity in layoff density

	(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)
$\mathbb{I}[\text{age} \geq 50] \times \text{Age}$	-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)
Mean	0.48	0.48	0.48
R^2	0.866	0.898	0.904
N	208	203	195

Note: The table reports a parametric test to detect any discontinuity in the density of layoff around the 50 years of age threshold. Column (1) includes all age bins. Column (2) excludes the manipulation region which encompasses the three bins before the cutoff and the two bins after the cutoff. Column (3) excludes an extended manipulation defined in Section F.1. Robust standard errors are reported in parentheses.

Table A2. Test for discontinuity in observables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Simple RD model			"Donut" RD model			Baseline
Variable	$\lambda_0^{>50}$	s.e.	T-stat	$\theta_0^{>50}$	s.e.	T-stat	mean
Female	0.011	0.005	**2.43**	0.000	0.005	-0.03	0.31
Experience	0.177	0.095	1.85	0.093	0.107	0.87	27.34
White Collar	0.017	0.005	**3.71**	0.005	0.005	0.86	0.20
Southern Region	-0.003	0.006	-0.56	-0.005	0.007	-0.74	0.47
Full Time	0.001	0.005	0.26	0.005	0.005	1.09	0.81
Tenure (in years)	-0.040	0.063	-0.63	-0.095	0.078	-1.22	5.85
Daily Wage (in logs)	0.000	0.006	0.03	0.005	0.007	0.69	4.17
Firm Age (in years)	-0.116	0.130	-0.89	-0.122	0.137	-0.89	14.269
Firm Size (in logs)	-0.038	0.014	**-2.72**	-0.015	0.016	-0.94	2.02

Note: The table reports results for the robustness test outlined in Section F.3. Columns 1 to 3 report estimates of $\lambda_0^{>50}$ with associated standard error and t-stat from the RD specification (59). Columns 4 through 6 present the corresponding results for $\theta_0^{>50}$ from the "donut" RD model of specification (60). Each row represents a separate observable characteristic. T-stats are highlighted in bold if coefficients are significantly different from zero at the 5% level. Column 7 reports baseline averages for individuals fired between 49 and 50 years of age. The analysis is based on 249,581 spells of individuals laid off from a permanent contract from Feb 2009 to Dec 2012. Standard Errors clustered at the local labour market level.