THE
ECONOMIC
JOURNAL

The Economic Journal, **136** (*August*), 2092–2130 https://doi.org/10.1093/ej/ueaf105
Advance Access Publication Date: 25 October 2025

MANIPULATION AND SELECTION IN UNEMPLOYMENT INSURANCE*

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

JEL codes: E24, J64, J65, J68

The targeting of public policies on the basis of observable characteristics is widespread in OECD countries. Governments tax individuals by marital status, provide welfare based on household size, and tie disability insurance to 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 efficiency of targeting based on immutable tags is well understood (Akerlof, 1978), in practice policymakers often have to rely 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 behaviour. 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 may value the additional benefits tremendously or act

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This paper was received on 7 August 2023 and accepted on 7 October 2025. The Editor was Barbara Petrongolo.

The authors were granted an exemption to publish their data because access to the data is restricted. However, the authors provided a simulated or synthetic dataset that allowed the Journal to run their codes. The synthetic/simulated data and codes are available on the Journal repository. They were checked for their ability to generate all tables and figures in the paper, however, the synthetic/simulated data are not designed to reproduce the same results. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.17288377.

This paper is a revised version of the paper 'Happy Birthday? Manipulation and Selection in Unemployment Insurance'. We are extremely thankful to our advisers 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 data used in this paper are the property of the Italian Social Security Institute (INPS) and are accessible by researchers at INPS premises through the VisitINPS programme. We are 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.

out of desperation. Manipulators might also be relatively less responsive to benefits once they qualify for them. Understanding the underlying *motivation* and subsequent behavioural changes is essential for assessing the welfare consequences of manipulation for designing optimal policies. 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 examine these issues in the context of unemployment insurance (UI), where differentiated policies and manipulation are widespread (see Spinnewijn, 2020, for a survey; see Van Doornik *et al.*, 2023; 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 total 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 lay-off. 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 lay-offs.

While it is relatively straightforward to quantify the impact of such manipulation on public spending, providing a comprehensive analysis of the motivations behind it is more complex. We distinguish two drivers of manipulation: moral hazard and long-term non-employment risk. To fix ideas, consider two extreme cases. First, suppose manipulators are individuals who would have found a job exactly eight months after lay-off, but *because* they are now self-selecting into a more generous scheme, they are staying in non-employment for four additional months, as a consequence of a decrease 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 non-employment 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, these are paid mechanically due to longer statutory coverage, without any change in job search effort and total non-employment duration. In this latter case, it is individuals' long-term non-employment 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 and compare them to non-manipulators, i.e., individuals 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 lay-offs right before age fifty and a spike of equal size right after that age. Using bunching techniques (Kleven and Waseem, 2013) we estimate that over 15% of all lay-offs 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.

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

On average manipulators spend fourteen more weeks on benefits and collect an additional EUR 2,239 each, 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 non-employment regardless of the length of UI coverage. Disentangling the relative importance of selection on non-employment risk and selection on moral hazard requires constructing manipulators' counterfactual non-employment 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 (2017) and the untreated outcome test of Kowalski (2023). The intuition behind these methods is that abnormal changes in the outcome variables (e.g., non-employment probabilities at different points in time during the non-employment 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 lay-off), absent manipulation and the absence of extensive margin responses. Under these assumptions, and owing to the granularity of the admin data, our strategy allows us to study both how manipulators' monthly survival rates in non-employment 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 (2017) and Kowalski (2023) to study at the same time selection on untreated outcomes and selection on treatment effects in a regression discontinuity (RD) setting with manipulation.

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), defined as the ratio between the changes in UI expenditure and tax revenues arising from changes in the search behaviour of workers (behavioural cost, BC) and the cost arising from longer coverage without changes in the search behaviour of workers (mechanical cost, MC). We show that for one euro of mechanical UI transfer towards manipulators the government pays an additional 24 cents due to behavioural responses during non-employment. 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 non-employment risk. Even absent manipulation, manipulators would have exhausted eight months of UI benefits with a 16.8 percentage points (p.p.) higher probability than non-manipulators.

The firm–worker collusion to delay lay-offs seems to act as an effective screening mechanism for long-term non-employment 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 more prevalent among female, part-time, white-collar workers and in smaller firms. Manipulators' careers are similar to non-manipulators', but they exhibit higher pre-lay-off absence rates (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 favour disadvantaged workers with poor labour market prospects.

Our results carry broader implications for the design of targeted UI programmes 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 characterisation 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 non-employment risk, which we estimate in the paper, play a crucial role, because they contribute to enlarge or shrink manipulators' impact on the government budget. Since manipulation is localised 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 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 policymakers 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 (Mueller and Spinnewijn, 2023; Wunsch and Zabrodina, 2023) show that job histories predict long-term non-employment risk and moral hazard responses particularly well. While these may be information-rich tags, they are also highly endogenous and selection into behavioural responses and associated fiscal externalities could invalidate or reinforce any beneficial effects from differentiation along these informative tags. Our methods allow to measure these fiscal externalities, offering insights on how optimal second-best policies should account for these effects. A full welfare evaluation would require additional data on manipulators' value for longer UI, which remains an open question.

Our empirical results show that manipulation is highly concentrated around the age-fifty threshold, potentially limiting its welfare impact in our setting. However, our results remain relevant for three reasons. First, similar selection patterns may exist in other settings with more ambitious policy differentiation, where ignoring manipulation could have larger consequences. Our methodology could be readily and fruitfully applied to these cases. Second, we show that individuals possess relevant information concerning their long-term non-employment risk, which could improve targeting. Last, 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 workers with prior health-related absences who are at a higher risk of long-term unemployment. Interestingly, these measures are also observed by the Social Security Institute, which could 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 effects of UI on job search and post-re-employment outcomes, exploiting similar policy variation (Card *et al.*, 2007; Lalive, 2007; Schmieder *et al.*, 2012; Landais, 2015; Nekoei and Weber, 2017; Johnston and Mas, 2018). 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.

In this sense, we also contribute to the small, but growing, literature on manipulation in social insurance. Two recent contributions by Khoury (2023) and Van Doornik *et al.* (2023) document manipulation in UI systems around eligibility and seniority thresholds in Brazil and France, respectively. Van Doornik *et al.* (2023) provide evidence of strategic collusion between workers and firms who time lay-offs for UI eligibility, while Khoury (2023) exploits a discontinuity in benefit levels to estimate an elasticity of employment spell duration with respect to UI benefits of 0.008. However, neither study examines the selection patterns we analyse in our work nor the implications for optimal UI through a model. In the last part of the paper we also provide a review of the literature to rationalise why manipulation may be present in some settings, but not others.

Although our primary contribution is empirical, we do relate to the literature on the theoretical desirability of *tagging* (Akerlof, 1978). We show that bargaining over lay-off timing serves as a screening mechanism for long-term non-employment risk. In recent work, Michelacci and Ruffo (2015) argue for higher UI benefits for young workers by analysing the canonical Baily–Chetty (Baily, 1978; Chetty, 2006) trade-off from a life cycle perspective. Age as a useful tag for redistribution has also been studied in the context of taxation by, e.g., Weinzierl (2011) and Best and Kleven (2013). Our setting integrates selection on risk and moral hazard, traditionally studied separately.[3] Recent efforts to unify these concepts include Hendren *et al.* (2021), Landais *et al.* (2021) and Marone and Sabety (2022), who analyse the welfare implications of choice in regulated insurance markets.[4] Unlike our study, 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]

Our findings on positive selection on long-term non-employment risk contributes to the literature on private information and (*ex post*) adverse selection in insurance markets, see, e.g., Hendren (2017) for unemployment insurance and Cabral (2017) for dental insurance. Our results indicate that individuals hold information about their expected unemployment duration, which firms may infer from proxies like health indicators.

From a methodological perspective, our empirical strategy is most closely related to recent work by Diamond and Persson (2017), who study manipulation of test scores in Swedish high-

[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.

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 (2023) in the instrumental variable setting, which we apply to regression discontinuity. We borrow several ideas from standard bunching techniques surveyed by Kleven (2016).

The remainder of the paper is organised as follows: Section 1 presents the institutional setting and data, Section 2 outlines the empirical strategy, Section 3 reports our results and robustness checks. We present the main insights from our theory in Section 4 and in Section 5 we conclude.

1. The Italian Unemployment Insurance Scheme

1.1. *Institutional Setting*

1.1.1. *Unemployment insurance in Italy*

We study manipulation in the Italian Ordinary Unemployment Benefits scheme (OUB).[7] OUB was in place from the late 1930s until its replacement in January 2013. The structure of the benefit scheme changed over the years and we limit our attention to the 2009–12 period, when the characteristics of the OUB were stable. OUB covered private non-farm and public sector employees who lost their job due to temporary contract termination, lay-off, or resignation for just cause (e.g., unpaid wages or harassment). Others voluntary quit, and the self-employed are not eligible.[8]

Eligibility required prior labour market attachment: workers needed to have started their first employment spell at least two years before the date of lay-off, and have fifty-two weeks of work in the preceding two years.[9]

Benefit levels were based on the average monthly wage, calculated over the three months before lay-off. The replacement rate declined over time: 60% for the first six months; 50% for the next two and 40% thereafter. OUB featured no experience rating.

Potential Benefit Duration (PBD) depended on age at lay-off: eight months if below fifty years, twelve months if above. This discontinuity created incentives to delay lay-offs past the fiftieth birthday.

1.1.2. *Lay-offs in Italy*

During the period under study, the Italian employment protection legislation (EPL) allowed firms to lay off individual workers only under specified circumstances:[10] for economic reasons

[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.

[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 Online Appendix F.

[8] For convenience, in the rest of the paper we will use the term 'lay-off' to indicate all job terminations that are eligible for UI.

[9] Two other UI benefit schemes were in place in Italy at the same time of our analysis: reduced unemployment benefits (RUB) and mobility allowance (MA). 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 Online Appendix E.

[10] Contrary to individual dismissals, collective dismissals make workers eligible for more generous benefit schemes not studied in this paper (mobility allowance, MA). We provide more background details on these in Online Appendix E.

such as restructuring or downsizing (*giustificato motivo oggettivo*), for not performing job tasks (*giustificato motivo soggettivo*) or for outright misbehaviour (*giusta causa*). Unlike *employment at will*, firms could not dismiss workers in order to substitute them with other workers.[11]

Employers had to provide written notice, stating the reasons for the lay-off. Except in the case of misbehaviour, advance notice was required, with length determined by collective bargaining agreements and vary by tenure and occupation.[12]

Laid-off workers could contest dismissals in court and the employer bore the burden of proof. If the lay-off is deemed legitimate, no firing costs or compensation is due. In case of an illegitimate lay-off, firms must pay penalties based on firm size. Firms with up to fifteen employees pay a severance (*tutela obbligatoria*) ranging from 2.5 to 6 months' salary. Larger firms cover all lost wages until court resolution, which could take years, and had to reinstate the worker unless they accepted an additional 15-month severance (Schivardi and Torrini, 2008).[13]

1.2. *Data*

We use confidential administrative data from the Italian Social Security Institute (INPS) on the total of UI claims in Italy between 2009 and 2012, merged with employer–employee records covering private sector careers. Claims data come from the SIP database,[14] which records income support measures following job separations. For every claim we observe the UI scheme type, start date, duration and amount paid, along with job and firm details, including contract type and broad occupation category. Unfortunately, the data does not extend before February 2009 and a UI scheme change in January 2013 prevents us from including later years.

Re-employment dates are retrieved from the matched employer-employee database (UNIEMENS), allowing us to construct non-employment durations as the gap between the lay-off date in SIP and the first re-employment date in UNIEMENS after UI benefits end. We exclude a few short-term jobs compatible with ongoing UI receipt. The UNIEMENS database tracks workers until 2016, ensuring at least four years of post-lay-off observations. It includes detailed information on wages and the type of contract.

As it is common in many other administrative databases, our non-employment measure combines true non-employment and informal jobs, which we do not observe.[15] In Section 3.3 we discuss why the presence of informal jobs does not bias our empirical results nor why 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 non-employment 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 non-employment and informality simply as 'non-employment' throughout the rest of the paper.

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

[12] For example, the wholesale and retail trade Collective Bargaining Agreement requires two weeks of advance notice for low-ranked workers with less than five years of tenure, but four months for high-ranked with more than ten years of tenure.

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

[14] Sistema Informativo Percettori in Italian.

[15] According to the Italian National Institute of Statistics (Istat), in the years under study (2009–12) the share of informal workers oscillated between 12.2 and 12.6% of total employment.

Finally, we supplement career data with INPS records on transfers for absences covered by benefits and social security contributions,[16] such as sickness, work accidents, disability (self or relatives), and child leave. Notably, these transfers are observable by the firm. We observe the total euro amount received for each absence type in the four years before lay-off at the separating firm and normalise it by the worker's average pre-lay-off monthly wage (as from SIP). Absences under seven days (for sickness and work accidents) are not included due to privacy restrictions.

Our initial sample includes individuals who lost their jobs between February 2009 and December 2012, were aged 46–54 at lay-off, worked 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 3.1 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.[17] Claims by public sector workers are excluded as their career outcomes are not observable in UNIEMENS. 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 a late stage in their careers.

Table 1 reports summary statistics for the initial and restricted samples. In the initial sample, on average UI recipients collect benefits for twenty-six weeks (six months), roughly corresponding to one-third of the seventy weeks (twenty-one months) of average non-employment duration. About 50% and 39% of workers are still in non-employment after eight and twelve months, respectively, implying substantial exhaustion risk. Workers in this sample are predominately male, blue collar, and working full time, with an average experience of 27.5 years and almost 4.3 of tenure at the separating firm. Geographically, 43% of workers are laid off in the south or the islands.[18] The gross average daily wage is EUR 70 ($70 \times 26 = 1{,}820$ euro per month for full-time workers).[19] Separating firms are relatively old (fifteen years) and large (eighty-five employees), but this is driven by a few very large firms. Most workers come from firms with fewer than fifteen employees. About 65% of employees in these firms are on permanent contracts and 22% are white-collar workers. Managers are relatively rare and only 0.2% of employees are classified as managers in the firms laying off the workers.[20] Absences due to sickness, work accidents, disability or child leave at the separating firm are rare.[21] Absences due to sickness are relatively more frequent, with workers receiving an average of 0.27 months of wages over four years (0.063 for accidents). Other events are even less frequent. In terms of past careers, workers were employed for about eighty-four months on average in the ten years before the UI spell and earned about EUR 125,000.

[16] This information is available in the Differenze Accredito database.

[17] 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.

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

[19] This information is consistent with the monthly wage reported in SIP: EUR 1,735 in the three months preceding the lay-off.

[20] The share of workers for each occupation is computed at municipality-firm level to better capture the characteristics of the local unit while the size of the firm is computed at the aggregate level.

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

Table 1. *Summary Statistics.*

	Private sector				Permanent contracts in the private sector			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Non-employment outcomes								
UI Benefit receipt duration (in weeks)	26.181	15.908	0.143	52	29.853	15.923	0.143	52
Non-employment duration (in weeks)	70.464	74.198	0	208	89.995	79.092	0	208
Non-employment survival prob. 8 months	0.502	0.500	0	1	0.644	0.479	0	1
Non-employment survival prob. 12 months	0.388	0.487	0	1	0.518	0.500	0	1
Individual characteristics								
Female (share)	0.377	0.485	0	1	0.311	0.463	0	1
Experience (in years)	27.443	8.854	2	40	27.656	8.552	2	40
North (share)	0.425	0.494	0	1	0.367	0.482	0	1
Centre (share)	0.181	0.385	0	1	0.174	0.379	0	1
South and islands (share)	0.394	0.489	0	1	0.459	0.498	0	1
Previous job characteristics								
Full time (share)	0.803	0.397	0	1	0.807	0.395	0	1
White collar (share)	0.183	0.387	0	1	0.208	0.406	0	1
Tenure (years)	4.351	5.291	0.083	30	5.931	6.113	0.083	30
Daily wage (past 6 months)	69.899	70.300	0.038	13,981.006	73.965	90.583	0.038	13,981.006
Firm characteristics								
Firm age (in years)	15.314	12.696	0	109.833	14.367	12.115	0	109.833
Firm size	84.915	608.022	1	14.222	28.158	259.005	1	14,103.167
Firm size below 15 (share)	0.606	0.489	0	1	0.765	0.424	0	1
Firm size between 15 and 49 (share)	0.213	0.409	0	1	0.164	0.370	0	1
Firm size above 49 (share)	0.181	0.385	0	1	0.071	0.257	0	1
Share permanent	0.682	0.372	0	1	0.934	0.145	0	1
Share white collar	0.224	0.301	0	1	0.232	0.319	0	1
Manager in firm	0.002	0.021	0	1	0.002	0.027	0	1
Absences								
Sickness (4 years)	0.270	0.976	0	18.366	0.420	1.251	0	18.366
Work accident (4 years)	0.063	0.523	0	18.277	0.096	0.679	0	18.277
Disability (4 years)	0.001	0.042	0	5.208	0.001	0.056	0	5.208
Disability others (4 years)	0.021	0.452	0	22.285	0.036	0.603	0	22.285
Child leave (4 years)	0.011	0.327	0	21.717	0.018	0.430	0	21.717
Past career in private sector								
Total months employed (10 years)	83.506	31.626	0	120	89.525	30.911	0	120
Total income (10 years)	124,673.883	90,928.203	0	3,985,142.500	138,451.125	104,694.781	0	3,985,142.500

Notes: 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 and 54 years of age at the time of lay-off. The sample contains a total of 462,607 non-employment spells and 332,899 individuals for the 'private sector' sample and 249,581 non-employment spells from 210,041 individual workers for the 'permanent contracts in the private sector' sample. Non-employment duration is censored at four years and defined as the time distance between the date of lay-off 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 lay-off (or the closest within six months of the lay-off). Size of the firm is computed accounting for all workers in Italy. Manager in firm is the share of managers at the firm municipality level. Variables in 'Absences' are computed as the total amount received by the worker in the four years before lay-off at the firm laying them off. The total amount received by the worker is normalised by the average wage in the three months prior to lay-off as reported by the SIP database. These measures are winsorised at 0.1% to reduce the influence of extreme values. Months of employment and total income are limited to the private sector.

Workers in the restricted sample exhibit longer non-employment durations (ninety weeks on average) with 52% remaining non-employed after twelve months. They are more often male (68.9%) and based in the south (46%). These workers have higher tenure (almost six years) and come from smaller firms (twenty-eight employees on average). Their separating firms have a larger share of permanent contracts (93%), but similar proportions of white-collar workers (23%) and managers (0.2%). Absences are more frequent for this group, likely due to job security facilitating time off (Ichino and Riphahn, 2005). In addition, they have longer employment histories, averaging six additional months of work and EUR 14,000 in extra earnings over the ten years before the lay-off.

Concerns about transitions into retirement are minimal, as only 0.6% of the restricted sample (1,500 workers) claim pensions within four years after lay-off. We now turn to a description of our objects of interest and identification strategy.

2. Empirical Strategy

This section outlines our empirical strategy and the sources of variation in the data that allow us to estimate key 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. Once we establish the manipulation window, we fit polynomials to the unmanipulated part of the data and interpolate inward. This enables us to construct a counterfactual lay-off 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 non-employment survival probabilities. Intuitively, any unusual change in these outcomes near the cut-off, 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 (2017) and Kowalski (2023).

2.1. *Quantifying Manipulation*

Consider a hypothetical manipulated lay-off density as in Figure 1(a). Absent any manipulation we would expect the frequency of lay-offs to be smooth in the neighbourhood of the cut-off. Manipulation instead causes a sharp drop in the number of lay-offs right before the age of 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 lay-offs by fitting a polynomial to the unmanipulated parts of the data (on the left and right of the cut-off) 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 lay-offs that would have occurred anyway. We provide supporting evidence that this is indeed the case in Section 3.6.

We operationalise this identification strategy following standard bunching techniques, e.g., Saez (2010), Chetty *et al.* (2011) and Kleven and Waseem (2013). First, we group all lay-offs

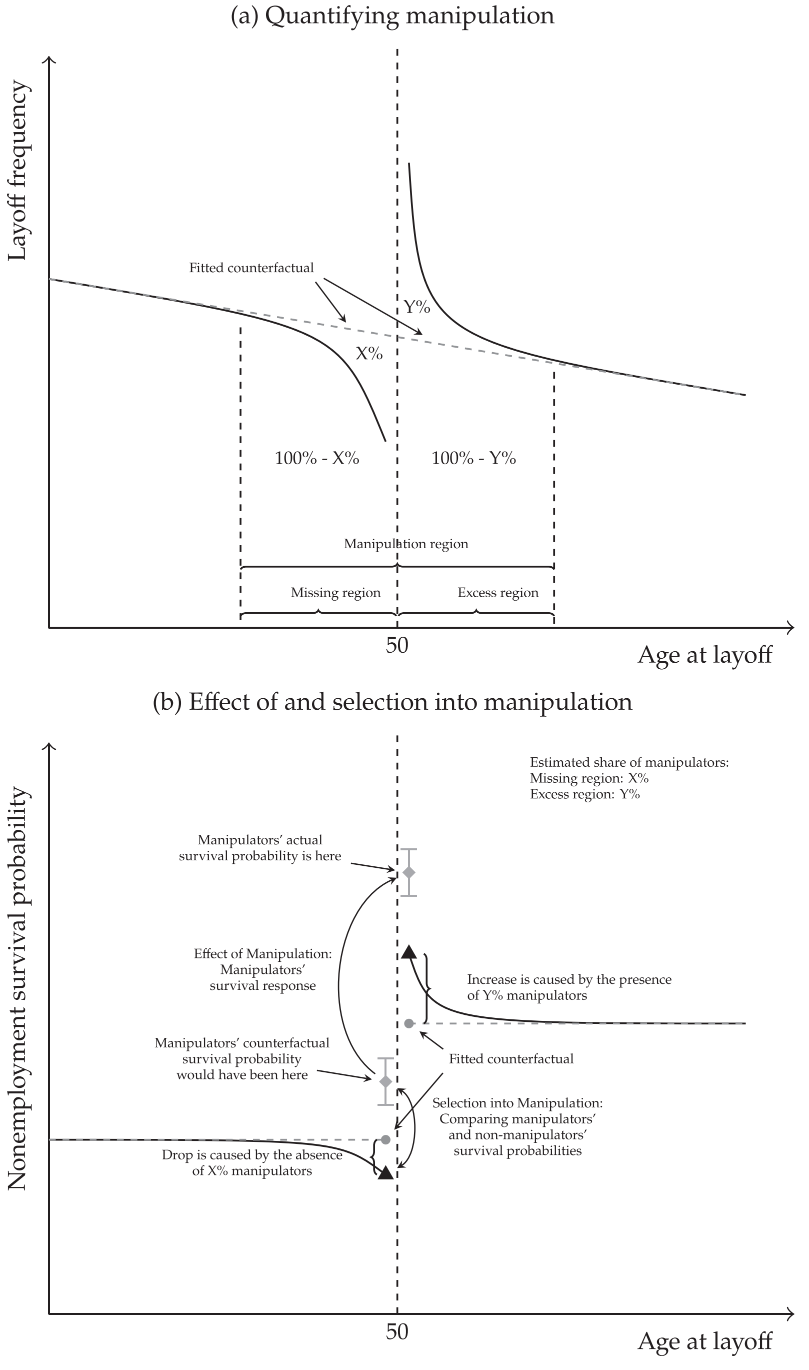


Fig. 1. *Illustration of Identification Strategy.*
Notes: The figure visualises our identification strategy. Panel (a) illustrates how we estimate the number and respective share of manipulators in both the missing and excess regions. Panel (b) constructs manipulators' survival response and illustrates the relevant comparison when studying selection into manipulation. Section 2 discusses how we estimate the fitted counterfactuals in practice.

into two-week bins based on the workers' age at lay-off. 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 lay-off 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:

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

where c_j denotes the absolute frequency of lay-offs 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:

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

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:

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

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:

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

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

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

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

2.2. *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 1(b) illustrates the idea for one of our outcomes of interest: non-employment survival rates. Manipulation provides workers with additional UI coverage from month eight to twelve. Thus, it is likely that non-employment survival rates respond to the increase in coverage. Consider a hypothetical statistical relationship between non-employment survival and age at lay-off, as in Figure 1(b). 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 1(b), 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 unmanipulated region to the right only due to manipulation. The extent to which observed and predicted non-employment survival rates differ, together with an estimate of how many manipulators are causing this difference, let us recover manipulators' actual non-employment survival probability. We use analogous arguments to back out manipulators' counterfactual non-employment survival probability on the left-hand side of the threshold.

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

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

where y_i is the outcome of interest, e.g., weeks of UI benefit receipt or probability of still being non-employed eight months after the lay-off, $\beta_p^{\leq 50}$ 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 (1) 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).[22] 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 1(b).

The central idea of our estimation strategy is the rescaling 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

[22] 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.

difference in average outcomes between manipulators and non-manipulators as:[23]

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} - \bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\hat{\delta}_k}{s^{\text{missing}}_k}. \tag{2}$$

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

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k}, \tag{3}$$

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

$$\bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} - \frac{\hat{\delta}_k}{s^{\text{missing}}_k}, \tag{4}$$

and manipulators average counterfactual outcome over the entire missing region as

$$\bar{Y}^{\text{missing}}_{\text{mani}} = \frac{1}{N^{\text{missing}}_{\text{mani}}} \sum_k |\gamma_k| \cdot \bar{Y}^{\text{missing}}_{\text{mani},k}. \tag{5}$$

The logic behind this rescaling 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 1,000 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 cut-off, we first rescale the regression coefficient for bin k to obtain

$$\bar{Y}^{\text{excess}}_{\text{mani},k} - \bar{Y}^{\text{excess}}_{\text{w/o mani},k} = \frac{\hat{\delta}_k}{s^{\text{excess}}_k}. \tag{6}$$

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

$$\bar{Y}^{\text{excess}}_{\text{observed},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} = \frac{\hat{\gamma}_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k} + (c_k - \hat{\gamma}_k) \cdot \bar{Y}^{\text{excess}}_{\text{w/o mani},k}}{c_k}. \tag{7}$$

Combining the two expressions in (6) and (7), and rearranging terms gives us an estimate of manipulators' average actual outcome in the form of

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

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

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

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

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

which after some rearrangement leads to our equation (2).

which, together with (5), lets us define manipulators' moral hazard response (or treatment effect) as

$$Y_{\text{mani}}^{TE} \equiv \bar{Y}_{\text{mani}}^{\text{excess}} - \bar{Y}_{\text{mani}}^{\text{missing}}. \tag{8}$$

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 lay-off.

The procedure illustrated in Figure 1(b) also lets us study selection into manipulation. This can be done by comparing manipulators' counterfactual outcomes to non-manipulators realised outcomes, both of which are 'untreated', i.e., occur under the shorter eight-month scheme. Our comparison is similar in nature to the 'untreated outcome test' by Kowalski (2023), who compares compliers' counterfactual and never takers' realised outcomes in an instrumental variable framework.[24]

Concretely, notice that non-manipulators' untreated outcome in the missing region $\bar{Y}_{\text{non-mani}}^{\text{missing}}$ is observed in (3), while manipulators' untreated outcome $\bar{Y}_{\text{mani}}^{\text{missing}}$ was obtained by recognising that the counterfactual outcome in the missing region is a weighted average of non-manipulators' untreated outcomes and manipulators' untreated outcomes in (4) and (5). 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

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

If this difference is non-zero, then manipulators select into manipulation based on their baseline non-employment risk. Figure 1(b) highlights this comparison and would suggest that even absent manipulation, manipulators would have had a higher non-employment survival rate than non-manipulators due to the drop in the outcome variable to the left of the cut-off. This is indeed what we show empirically in Section 3.4.

In this paper, we abstract from the exact micro-foundation behind individuals' high baseline non-employment risk, which could be due to intrinsic worker types, but also to optimal *choices*. For example, baseline non-employment risk could be equally high for a wealthy worker who is less attached to the labour market and decides not to look for a job and for a disadvantaged unskilled worker. In Section 4 we show that baseline non-employment 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 (8), as they occur *regardless* of how much PBD workers receive.

[24] The analogy with the instrumental variable framework studied in Kowalski (2023) 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'. However, manipulators can be seen as 'compliers', because they are induced by manipulation possibilities to switch treatment status and obtain four months of extra coverage.

2.3. *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:

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

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:

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

2.4. *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 (behavioural 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 non-zero exhaustion risks during the relevant months of non-employment.

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

$$\frac{BC^i_P}{MC^i_P} = \frac{b \cdot \int_0^P \frac{dS^i_t}{dP} dt + \tau \cdot \int_0^T \frac{dS^i_t}{dP} dt}{b \cdot S^i_P}, \tag{9}$$

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^i_t is the survival probability in non-employment at time t for individual i, S_P represents the survival probability in non-employment 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 intergroup 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 rescaled moral

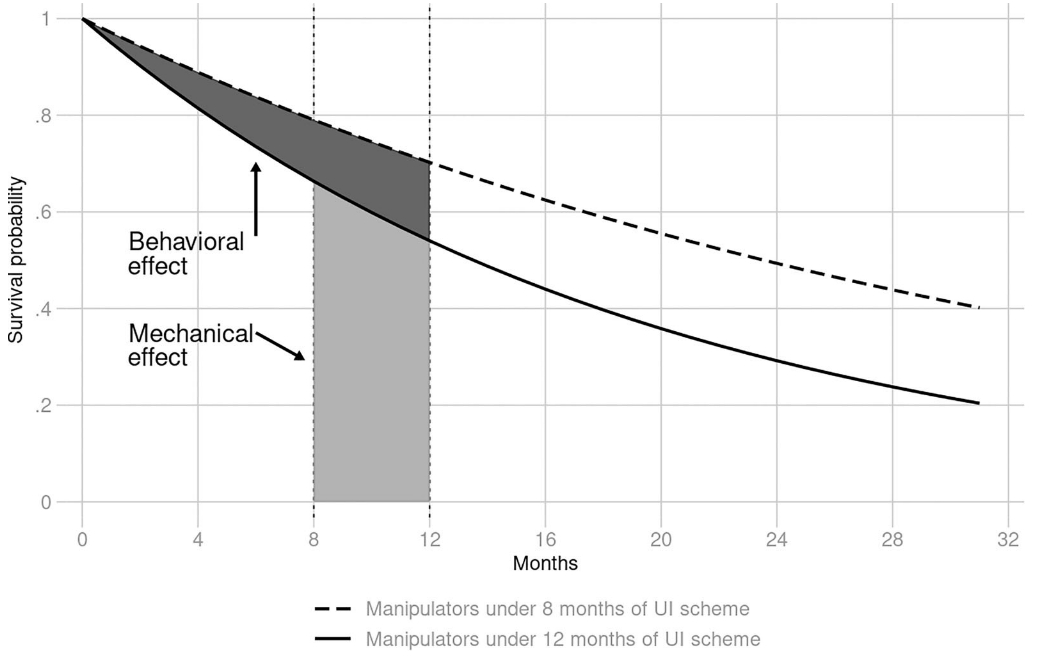


Fig. 2. *The Moral Hazard Cost of Extended UI Coverage.*
Notes: The figure displays two hypothetical non-employment survival curves for manipulators, namely, under eight months of PBD (solid line) and twelve months of PBD (dashed line). The dashed line is above the solid line assuming that higher PBD lowers the exit hazard rate from non-employment. 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: (i) a mechanical part (light grey area) which captures additional UI benefit payments that would occur even absent any behavioural change; (ii) a behavioural component (dark grey area) which is due to a shift in the survival curve. The BC/MC ratio defined in equation (9) is given by the ratio of (ii) and (i).

hazard effect that is relevant for optimal policy in our setting, something which we highlight in Section 4.

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 eight months (solid line), and one for when PBD is equal to twelve 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 eight to twelve months is represented by the light-shaded area beneath the solid survival curve between months eight and twelve of non-employment. MC arises from covering four extra months with positive non-employment risk, with no change in behaviour. The behavioural cost (BC) of increasing PBD from eight to twelve months corresponds to the dark-shaded area between the solid and dashed survival curves between months 0 and 12 of non-employment. 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.

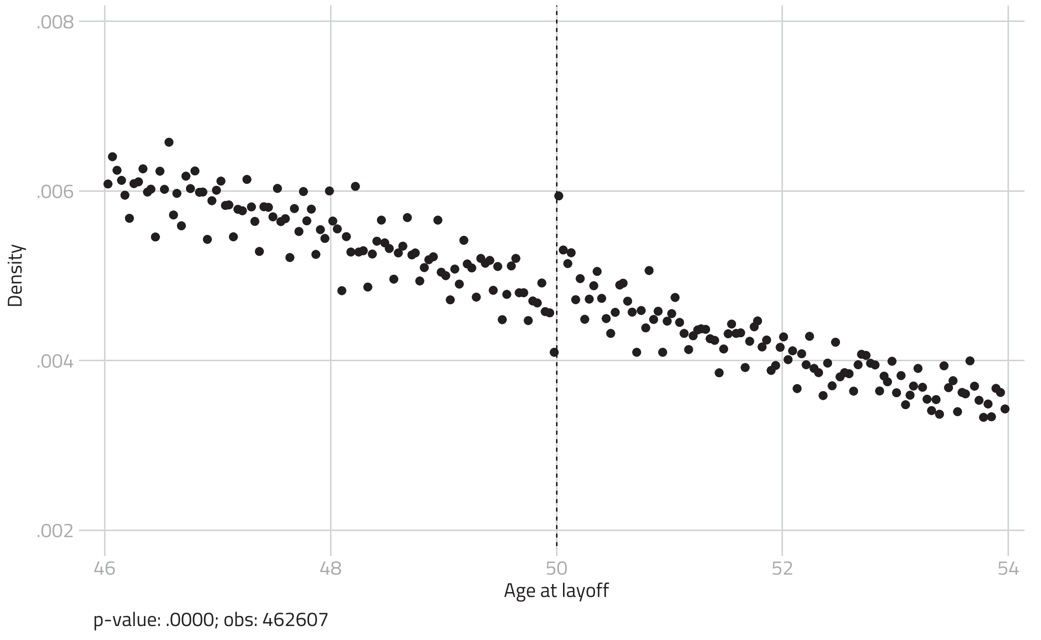


Fig. 3. *Lay-off Frequency for Workers in the Private Sector Between 46 and 54 Years of Age.*
Notes: The figure shows the density of lay-offs for the full sample of workers in the private sector claiming regular UI (OUB) for the age range from 46 to 54 years. The data cover the period from February 2009 to December 2012. The *p*-values are from density tests proposed by Cattaneo *et al.* (2020) and implemented through the `rddensity` command in Stata.

3. Results

We start by presenting graphical evidence of manipulation in the form of strategic delays in the timing of lay-offs around the fiftieth birthday threshold. After quantifying the magnitude of manipulation, we estimate the additional increase in UI receipt and non-employment duration that arises from the change in manipulators' job search behaviour 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 non-employment 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.

3.1. *Evidence of Manipulation*

To provide graphical evidence of manipulation, Figure 3 plots the relative frequency of lay-offs against workers' age at lay-off 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 reveals a clear drop in the density in the weeks just before workers' fiftieth birthday and a corresponding increase immediately after—suggesting strategic behaviour.

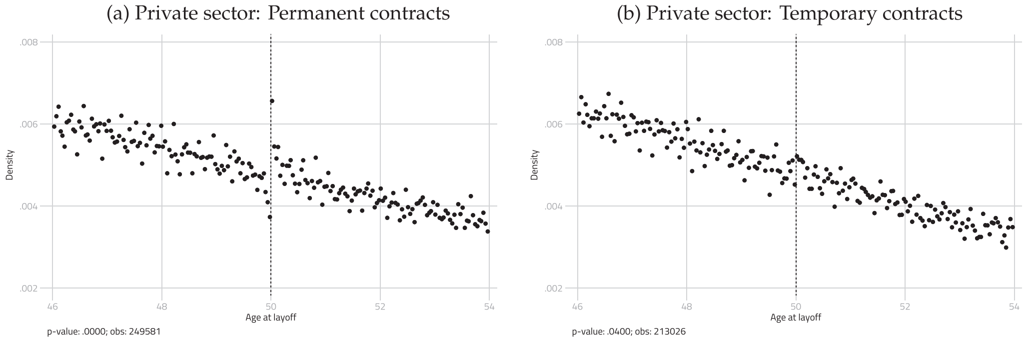


Fig. 4. *Lay-off Frequency in the Private Sector by Contract Type.*
Notes: The figure shows the density of lay-offs by contract type for individuals claiming regular UI (OUB). The data cover the period from February 2009 to December 2012. In all panels each dot represents a two-week bin. Individuals are classified as 'private sector' workers if they can be matched to an employment spell in the private sector matched employer–employee database (UNIEMENS). The *p*-values are from density tests proposed by Cattaneo *et al.* (2020) and implemented through the `rddensity` command in Stata.

In Figure 4 we then split the full sample of private sector workers in two groups, depending on their contract type *before* lay-off: permanent or temporary. The pattern for permanent contracts, reported in Figure 4(a), confirms manipulation.[25] For temporary contracts (Figure 4b) we find no evidence of manipulation. Since these workers are not useful to understanding strategic lay-off timing, we focus our main analysis on workers previously employed on permanent contracts. However, this restriction applies only to the contract type at lay-off—re-employment contracts remain unrestricted.

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. Thus, the only way to manipulate PBD eligibility would be for workers to strategically delay their starting date or secure a contract long enough to surpass the threshold—both of which are costly. Second, on average temporary workers find jobs much *faster* than permanent workers, reducing their need for extended UI benefits. We provide further details in Online Appendix B.

Repeating this analysis for public sector workers (Figure A1 in the Online Appendix), we find no evidence of manipulation.

Figure 5 further describes the frequency of lay-off 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 years) window.[26]

Following our estimation strategy outlined in Section 2.1, we find the manipulation region to consist of all age bins from six weeks before (missing region), up to four weeks after the

[25] 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 of 50 cut-off. However, Figures 4(a), 5 and the tests in Section F.1 of the Online Appendix show no evidence of such a shift, ruling out differential take-up is the driver of the results.

[26] Section 3.6 presents additional robustness checks.

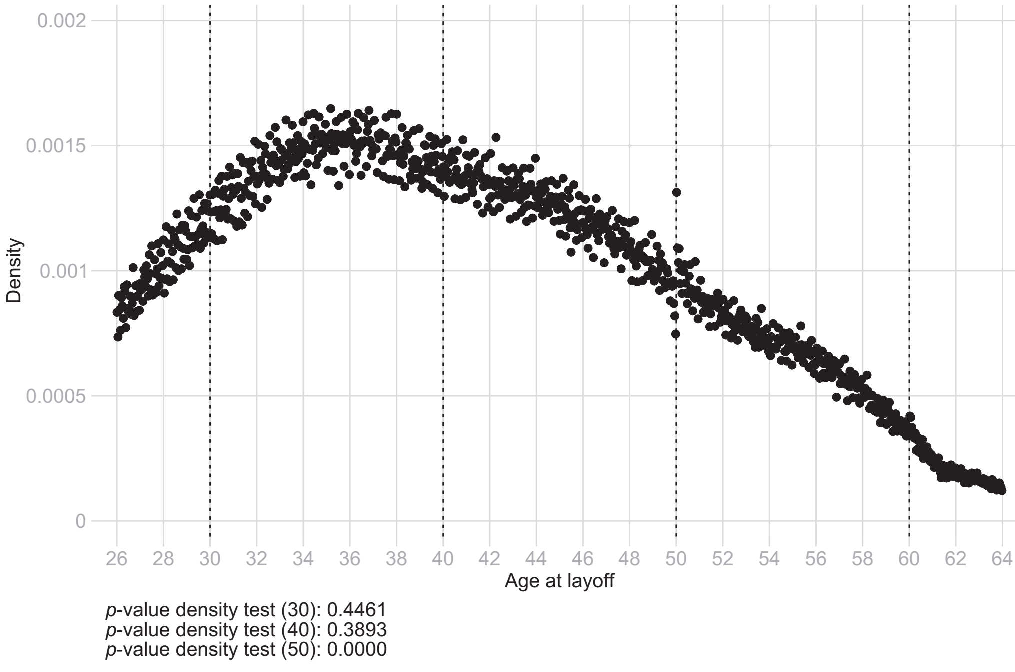


Fig. 5. *Lay-off Frequency in the Private Sector for Permanent Workers with Age at Lay-Off Between 26 and 64 Years.*
Notes: The figure shows the density of lay-offs in the private sector, for individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period from February 2009 to December 2012. The figure plots the density of lay-offs for the entire age range from 26 to 64 years of age. Each dot represents a two-week bin. The *p*-values are from density tests proposed by Cattaneo *et al.* (2020) and implemented through the `rddensity` command in Stata.

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 (458.5; 680.0)	3,038.0 (2,931.0; 3,150.0)	608.6 (496.0; 718.5)	2,390.4 (2,379.4; 2,401.3)	0.158 (0.127; 0.188)	0.203 (0.172; 0.231)

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

threshold (excess region). Table 2 reports our estimates and associated bootstrapped 95% confidence intervals 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 lay-offs are strategically delayed, corresponding to 15.8% of lay-offs 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 lay-offs in the excess region.

The highly localised nature of manipulation suggests that it may be quite costly or offer minimal benefits to firms, which have full discretion over the timing of lay-offs. 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 lay-offs in a four-week window of a tenure threshold in France are strategically delayed. Huang and Yang (2021) shows that a UI rebate policy in Canada leads to manipulation to be limited to two weeks before lay-off. Le Barbanchon *et al.* (2019) show that a 30% PBD increase at the age of fifty leads to an 8% density increase at the threshold. In Schmieder *et al.* (2012), PBD increases in Germany result in small manipulation effects two weeks away from an age threshold. Other studies have found larger responses in tenure at lay-off when incentives are very large. In France, a policy granting *unlimited* PBD after age 60 shifts lay-offs by up to four months—a larger effect due to the extreme incentive (Baguelin and Remillon, 2014).

In Section 3.5, we characterise 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.

3.2. *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 non-employment duration we begin by plotting these outcomes against workers' age at lay-off in Figure 6. For each outcome we see visible changes around the age threshold indicating that both respond to manipulation. As outlined in Section 2.2, 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.[27]

Our results indicate that manipulators would have collected EUR 5,814.20, and spent 27.8 weeks on UI benefits, had they not manipulated (column 1). Through manipulation these numbers increase to EUR 8,053.60 and 41.8 weeks (column 3), resulting in an additional cost of EUR 2,239 per manipulator (column 5). In order to benchmark these estimates, we compute the same numbers for non-manipulators following the strategy outlined in Section 2.3. We find that non-manipulators generate a total cost of EUR 1,636.90 (column 6) when receiving additional coverage.

As highlighted in Section 2.4, 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.

[27] All confidence intervals in the paper are obtained by simple non-parametric bootstrapping: we operationalise this by resampling UI events and re-estimating the entire procedure, including the share of manipulators, 5,000 times.

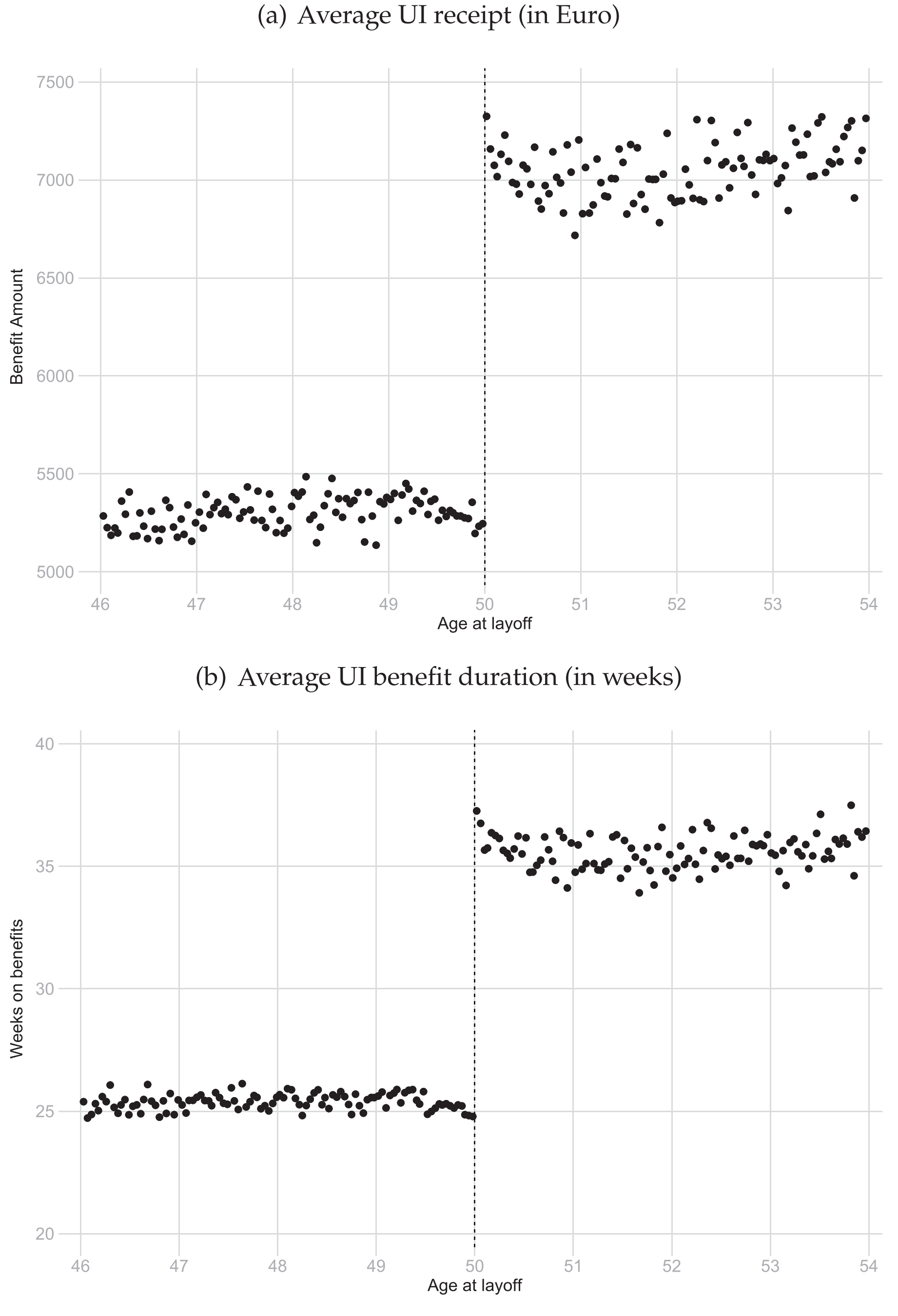


Fig. 6. *Benefit Receipt and Duration.*

Notes: The figure displays the average UI receipt in euro (a) and average UI benefit duration in weeks (b) by age at lay-off. 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 February 2009 to December 2012. The underlying data consists of 249,581 lay-offs.

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
5,814.2	5,223.5	8,053.6	7,044.2	2,239.4	1,636.9
(5,178.5; 6,459.2)	(5,125.0; 5,325.7)	(7,326.9; 8,836.5)	(6,974.5; 7,112.4)	(1,276.7; 3,261.6)	(1,410.9; 1,849.6)

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

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)

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

3.3. *Distinguishing Behavioural Responses from Mechanical Effects*

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

We start by plotting non-employment survival rates against age at lay-off at various months after lay-off in Figure 7. Qualitatively, we observe bigger jumps around the thresholds precisely during the months with extra coverage. Similarly to before, we combine these changes with the estimated share of manipulators causing them, to trace out monthly survival curves for both manipulators and non-manipulators.

Figure 8(a) presents our estimated non-employment survival curves of manipulators under the eight and twelve months PBD schemes. Figure 8(b) 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 because the 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 (9). To do so, we rely on numerical integration and weight responses

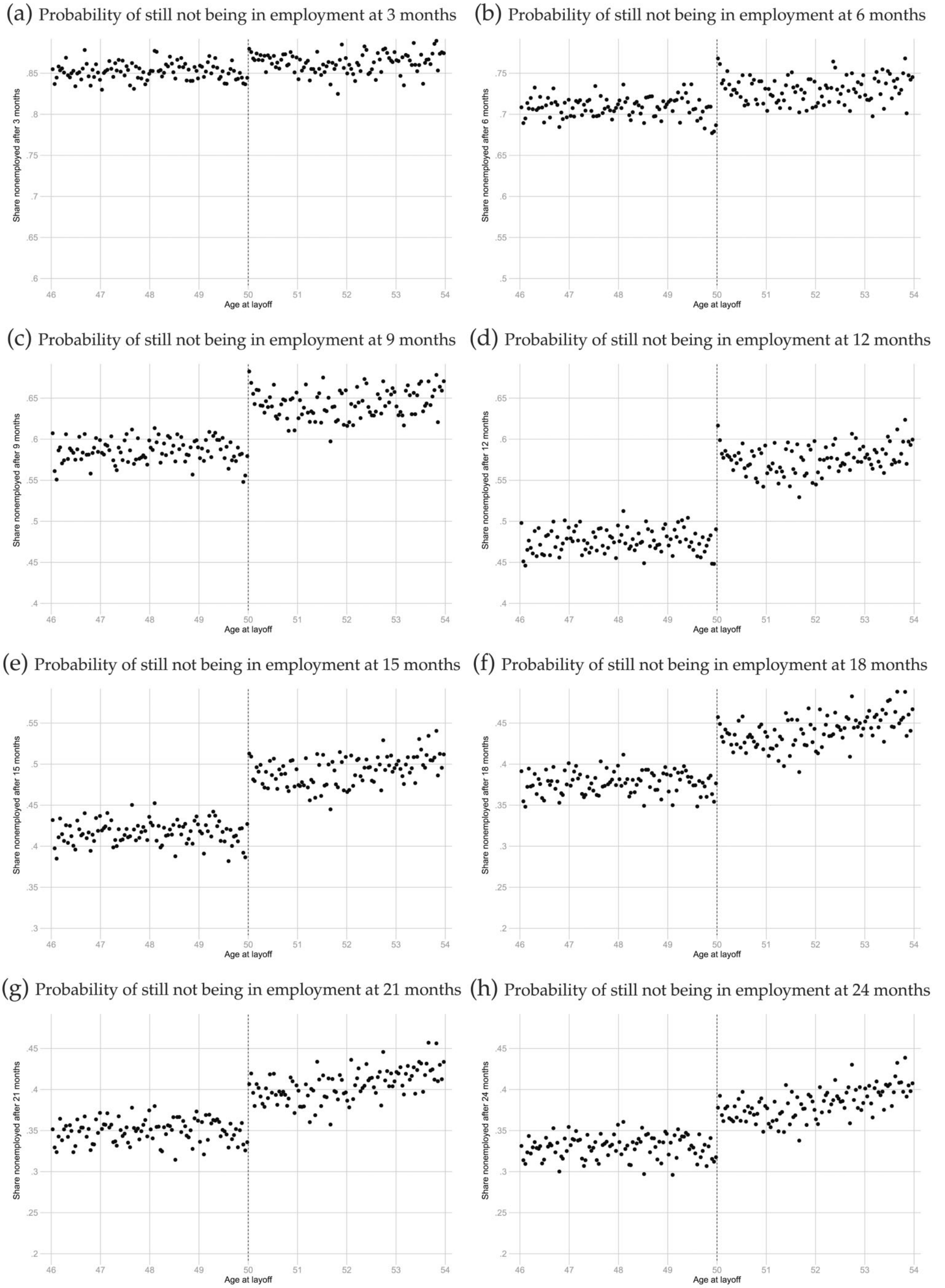


Fig. 7. *Non-Employment Survival Probabilities.*
Notes: The figures show the share of laid-off workers, who are still not in employment after 3, 6, . . . , 24 months. In all panels each dot represents a two-week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period from February 2009 to December 2012. The underlying data consists of 249,581 lay-offs.

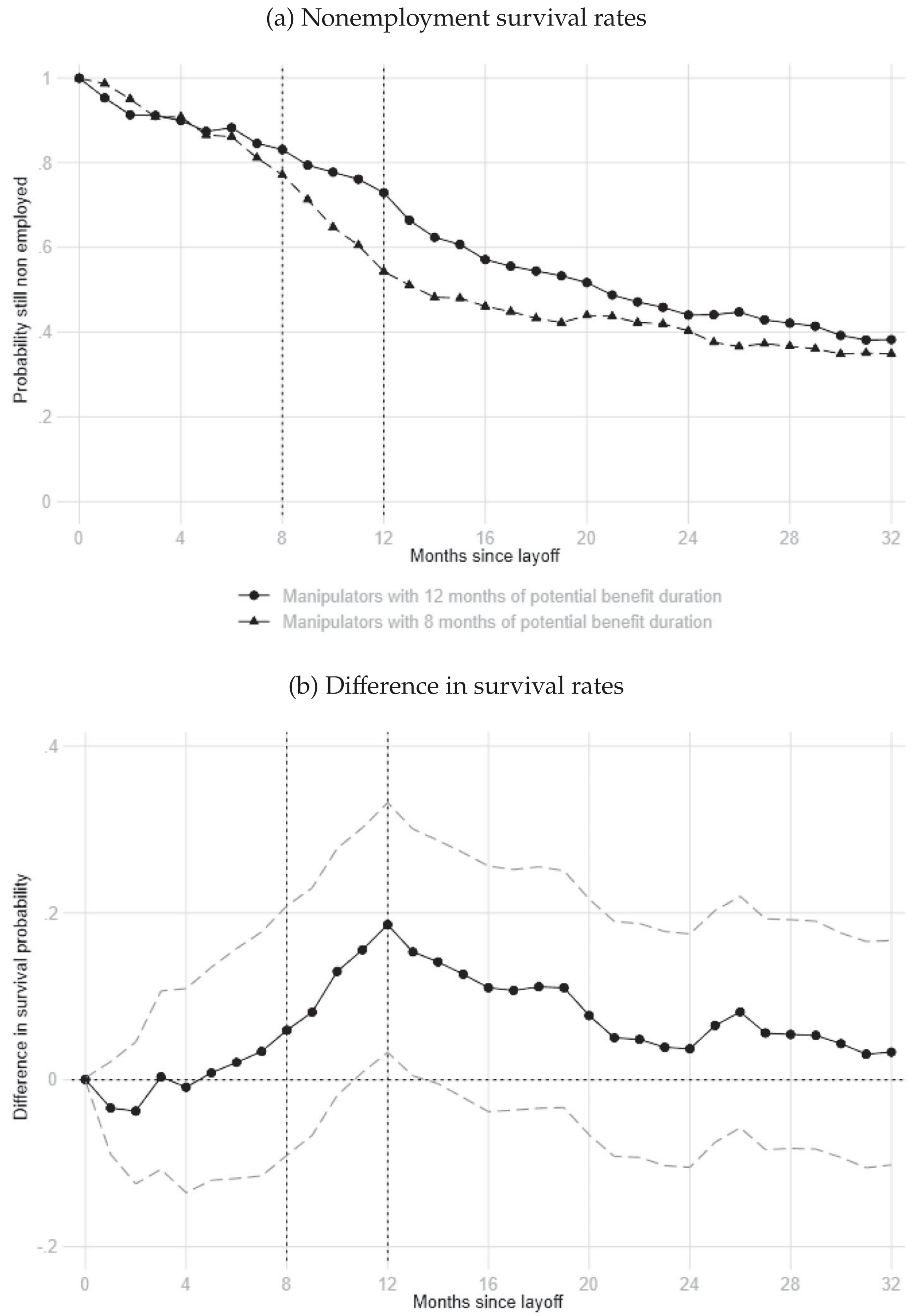
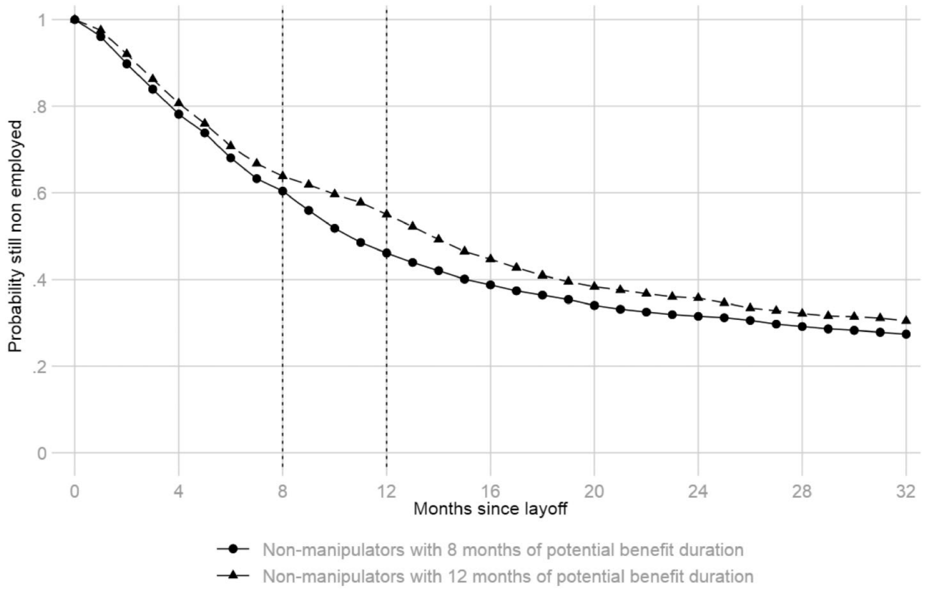


Fig. 8. *Manipulators with Eight and Twelve Months of Potential Benefit Duration.*
Notes: Panel (a) plots point estimates of manipulators' actual and counterfactual non-employment survival for the first thirty-two months after lay-off. Our estimation strategy is outlined in Section 2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

(a) Nonemployment survival rates



(b) Difference in survival rates

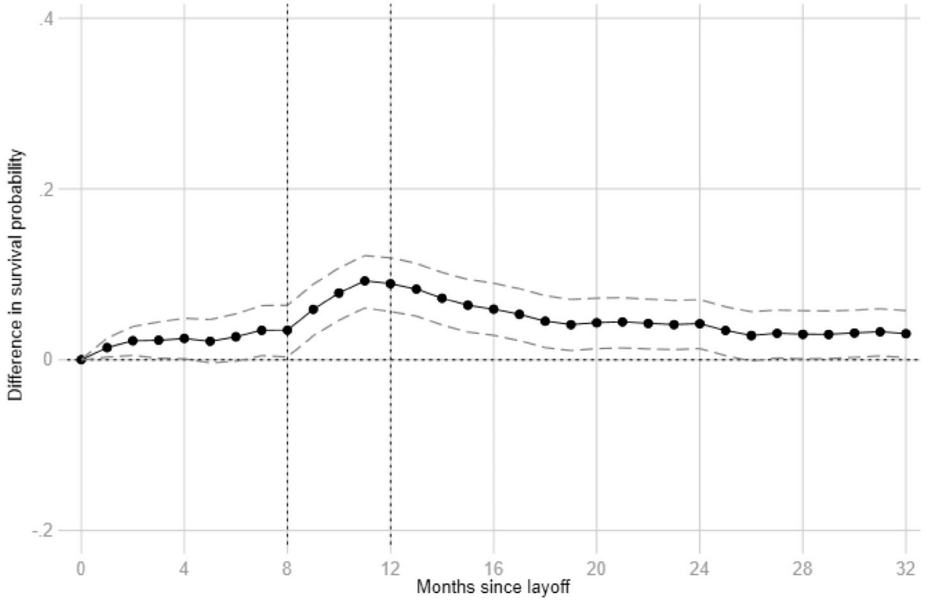


Fig. 9. *Non-Manipulators with Eight and Twelve Months of Potential Benefit Duration.*
Notes: Panel (a) plots point estimates of non-manipulators' actual and counterfactual non-employment survival for the first thirty-two months after lay-off. Our estimation strategy is outlined in Section 2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Table 5. *BC/MC Ratio Estimates.*

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

Notes: The table reports BC/MC ratio estimates for (a) manipulators and (b) non-manipulators. BC/MC ratios are defined in equation (1). Bootstrapped 95% confidence intervals in parentheses.

by statutory benefit rates.[28] 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 in 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 2.4 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 does it alter the conclusions of the paper. The (rescaled) fiscal externality associated with longer UI is still the sufficient statistic for the effective moral hazard cost, regardless of whether this occurs because people stay in non-employment or because people find informal jobs that pay no taxes, as shown in Gerard and Gonzaga (2021).

3.4. *Selection on Long-Term Non-Employment Risk*

The remainder of our analysis explores the underlying drivers of 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 3.3. A clear difference emerges: manipulators face 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 3.3.

[28] We perform integration using the midpoint rule and impose a non-negativity constraint on the behavioural 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.

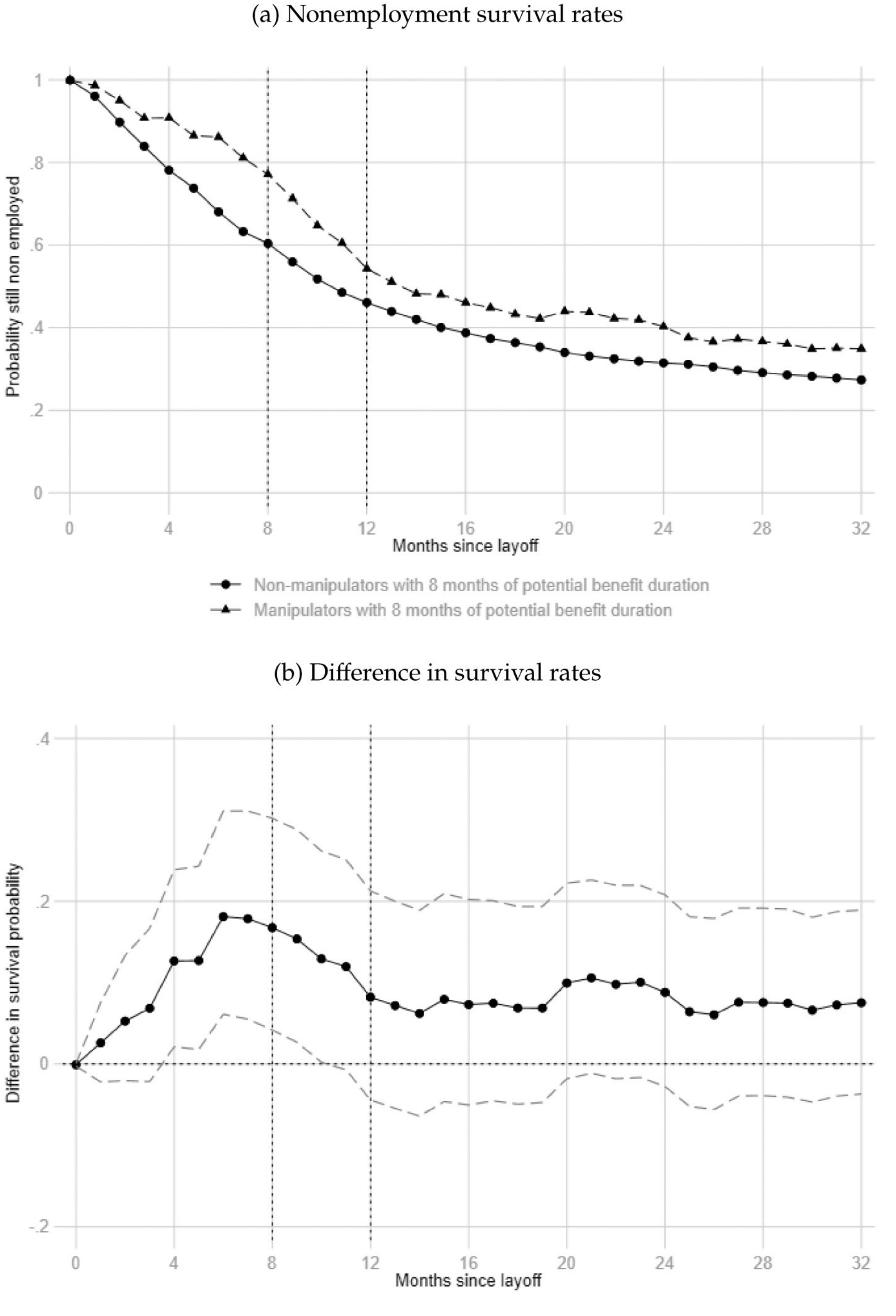


Fig. 10. *Manipulators and Non-Manipulators with Eight Months of Potential Benefit Duration.*
Notes: Panel (a) plots point estimates of manipulators' and non-manipulators' non-employment survival over the first thirty-two months after lay-off under eight months of PBD. The estimation of the former is outlined in Section 2. The latter represents the observed mean survival rate in the missing region. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Our finding that manipulators are selected based on long-term non-employment risk has broader implications. In particular, it relates—albeit indirectly—to potential asymmetric information problems in a hypothetical market where individuals insure against non-employment duration at the moment of lay-off, 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 are we 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.

3.5. *Characterising Manipulators*

In this final section of our empirical analysis, we provide suggestive evidence regarding the mechanisms behind manipulation. First, we identify observable characteristics that correlate with manipulation. Second, we review the literature to explore why manipulation may be more prevalent in some contexts than in others.

3.5.1. *Selection on observables*

We already showed in Section 3.1 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.[29] 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, though this 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.[30] Firm size also plays an important role in manipulation: manipulators come from

[29] The analysis closely follows Diamond and Persson (2017, sect.6.2).

[30] The additional unemployment insurance (UI) collected by manipulators is EUR 2,239 on average. In the most extreme case, a firm would pay additional wages equal to EUR 61 per day over the six weeks in the missing region, totalling around EUR 2,196. This suggests that, in extreme cases, the extra wages could nearly offset the UI payments. However, most manipulators are closer to the cut-off, resulting in smaller payments of about EUR 732 or EUR 1,460 for delays of two or four 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.

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)	135,688.203	136,566.043	−877.84
			(−18,998.984; 16,703.102)
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)

Notes: The table reports differences in observable characteristics between manipulators and non-manipulators in our main sample. Columns 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. 'Manager in firm' is the share of managers at the firm municipality level. 'Sickness', 'Work accidents', 'Disability', 'Disability others' and 'Child leave' represent the amount paid to the worker (normalised 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 lay-off. The total amount received by the worker is normalised by the average wage in the three months prior to lay-off as reported by the SIP. 'Sickness' includes only amounts paid as a consequence of absences longer then seven days, similarly to 'Work accidents' which covers only payments for absences related to work injuries lasting more than seven 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.

firms that are about 40% smaller. Firms laying off manipulators also appear to have a better contract composition. They have a slightly higher, though not significantly different, proportion of permanent contracts—95% compared to 93% in firms laying off non-manipulators—and a markedly higher share of white-collar employees, at 35% as opposed to 21%. The presence of managers within firms does not differ significantly between the two groups. Overall, these findings seem to suggest that adjustment costs, bargaining power and proximity to managers or owners 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 non-employment 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 serve as strong predictors of long-term unemployment risk. If firms were aware of this, they could utilise observable data to make strategic decisions about which workers to support. However, we find no substantial differences in the career trajectories of manipulators and non-manipulators in the ten years before the lay-off: manipulators worked five months longer than non-manipulators (ninety-three months compared to eighty-eight), but still earned slightly less (about EUR 900 less, out of about EUR 136,600 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 consider 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 child in the four years preceding the lay-off (Table 6).[31,32]

Although these events are generally rare at this stage of workers' careers, the evidence suggests that manipulators display greater signs of vulnerability than non-manipulators. They received more transfers related to work accidents, disability, disability of a relative and for child leave. However, these estimates are noisy and the only statistically significant difference between the two groups emerges in the case of child leave, which includes both absences due to childbirth and those taken for a child's illness. Overall, these results suggest that firms might be using some observable characteristics, not accounted for by policymakers, to provide additional benefits to workers who require extra insurance.

3.5.2. *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 definitive answers remain elusive, a review of the relevant literature suggests that both the magnitude of economic incentives and the institutional setting play critical roles.

Schmieder *et al.* (2012) reports that extending PBD for 49-year-old workers in Germany leads to a 1.4-month increase in benefit duration, whereas in our setting the same effect is 3.2 months for manipulators and 2.3 months for non-manipulators. These effects combine both mechanical and a behavioural components. Given that BC/MC ratios are similar across the two countries—0.3 on average—we can estimate the mechanical effect using the formula $\frac{MC}{BC+MC} = \frac{1}{1+BC/MC} = \frac{1}{1.3} \approx 0.77$. This implies that the mechanical effect accounts for roughly 1.1 months in Germany

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

[32] 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 lay-off and normalise this amount by the worker's average wage in the three months preceding the lay-off. The resulting measures can thus be interpreted as the number of monthly salaries received by the worker due to these transfers.

(1.4 × 0.77) and 2.5 months in Italy (3.2 × 0.77). The lower mechanical transfer in the German setting likely reduces incentives for workers to manipulate their lay-off date. This discrepancy may stem from differences in PBD levels: in Germany, 49-year-olds receive an extension from twenty-two to twenty-six months, whereas in Italy the increase is from eight to twelve months. *Ceteris paribus* the proportion of workers still unemployed at exhaustion would be lower in Germany, reducing incentives for manipulation.

Age may also play a role. Workers aged 50 and older may face higher risks of not returning to employment quickly, and so have more incentive to manipulate. Nekoei and Weber (2017), Ahammer and Packham (2023) and Guglielminetti *et al.* (2024) all study Austria's UI system, where workers become eligible for thirty-nine weeks of benefits instead of thirty weeks at age 40, and find no evidence of manipulation. In Austria, average non-employment duration is 114 days (16.2 weeks), far below the 30-week PBD threshold, suggesting that workers generally find jobs well before exhausting their UI benefits. As a result, they have little incentive to manipulate. In contrast, Jäger *et al.* (2023) find significant responses when PBD is extended to four years, indicating that large extensions encourage workers to use UI as a bridge to retirement.[33]

Institutional constraints may also limit manipulation. Caliendo *et al.* (2013) note that, in Germany, workers must register with the unemployment office three months before their contract ends, reducing opportunities to negotiate lay-off timing. In the same spirit, other studies using discontinuities in reform timing also found absence of manipulation (Johnston and Mas, 2018; 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.

While these explanations do not exclude the possibility that firms value their reputations or that managers engage in manipulation out of altruism, they highlight the multifaceted nature of UI manipulation. We hope that future research will further explore these important aspects.

3.6. *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 lay-off threshold. Second, the empirical analysis assumes that the discontinuity in PBD around the age threshold affects lay-off decisions in exactly one way, namely, through a delay in an otherwise earlier occurring lay-off.

By plotting lay-offs 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 lay-off 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.[34] Reassuringly, no discontinuity appears in these schemes.[35]

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). Although theoretically possible, we find no empirical evidence of any extensive margin job separation responses in our context through a series of robustness tests

[33] A similar finding is also present in Ahlqvist and Borén (2017), who finds strategic timing of UI claims as a bridge to retirement in Sweden.

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

[35] 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.

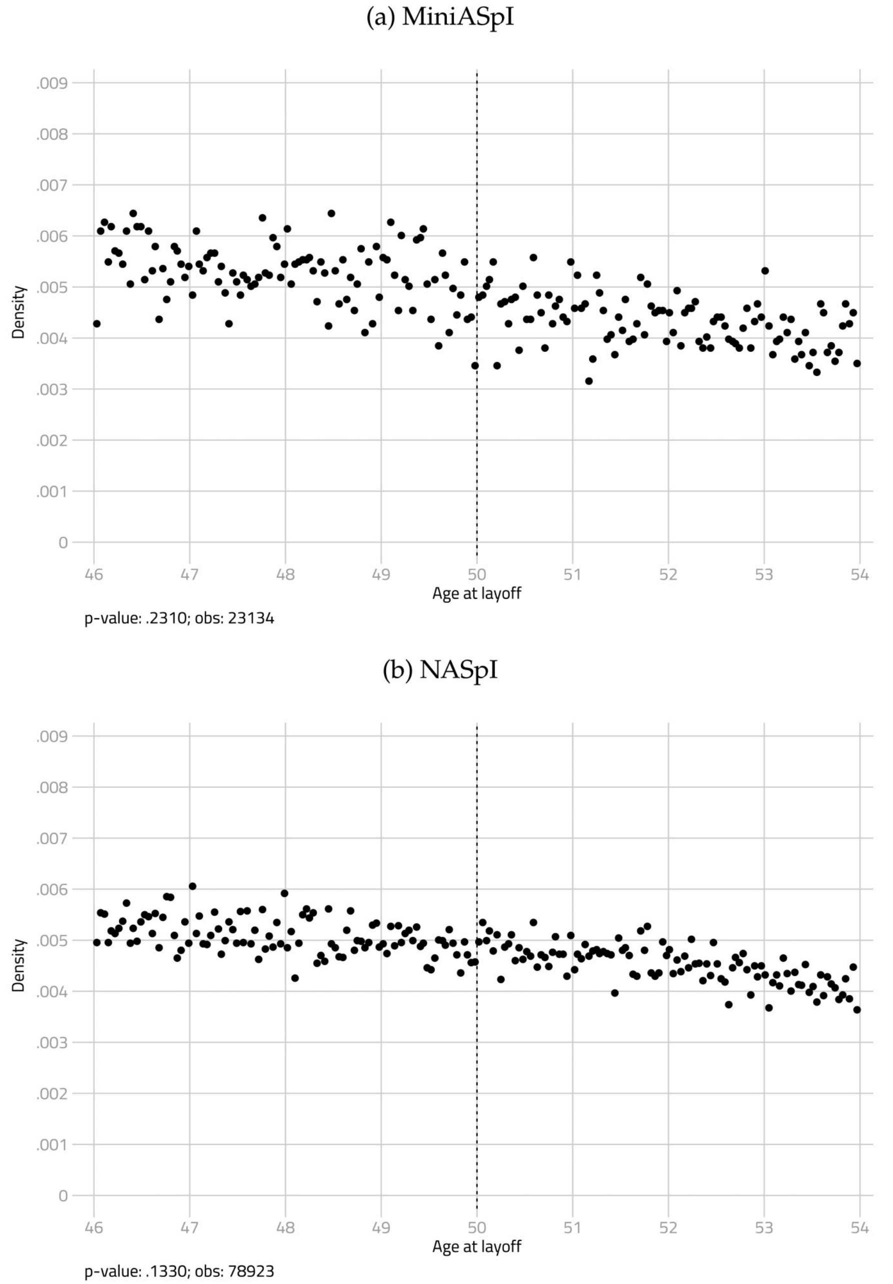


Fig. 11. *Placebo Checks: Lay-off Frequency for Workers Claiming MiniASpI and NASpI Between 46 and 54 Years of Age.*
Notes: The figure shows the density of lay-offs for workers laid off in the private sector and receiving MiniASpI (March 2013 to April 2015) or NASpI (from January 2016). In both panels each dot represents a two-week bin. The sample has been restricted to workers coming from permanent contracts in the private sector. The *p*-values are from density tests proposed by Cattaneo *et al*. (2020) and implemented through the `rddensity` command in Stata.

presented in detail in Online Appendix F. Intuitively, the lay-off density shown in Figure 3, shows no indication of any additional lay-offs to the right of the cut-off that are not explained by missing lay-offs in the missing region. We discuss this point, as well as a series of other robustness tests exhaustively in Online Appendix F and find no evidence for a violation of our identification assumption.

4. 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 Online Appendix C for the full model set-up 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 they 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 with respect to a *given* tag—inclusive of manipulation—has any potential benefit or if the optimal policy is in fact undifferentiated.

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.

4.1. *The Model*

4.1.1. *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 characterised 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.

4.2. *Characterising Optimal Policies*

In Online Appendix C we set up the formal job search problem for the old and the young and then solve the planner's maximisation problem. We derive the following proposition characterising 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:*

$$\frac{\tilde{u}'_y - \bar{u}'}{\bar{u}'} - \frac{BC^y}{MC^y} = \underbrace{\epsilon_{1-M,\Delta P}}_{\substack{\text{manipulation externality}\\ \text{of extra coverage}}} - \underbrace{\epsilon_{1-M,P}}_{\substack{\text{manipulation externality}\\ \text{of baseline coverage}}} + M \cdot \underbrace{\left(\frac{S^m_{P_y}}{S^y_{P_y}}\right)}_{\substack{\text{selection on risk}\\ \text{scale factor}}} \cdot \left\{ \underbrace{\left(\frac{\tilde{u}'_m - \tilde{u}'_n}{\bar{u}'}\right)}_{\substack{\text{selection on consumption}\\ \text{smoothing value}}} - \underbrace{\left(\frac{BC^m}{MC^m} - \frac{BC^n}{MC^n}\right)}_{\substack{\text{selection on moral}\\ \text{hazard cost}}} \right\}, \quad (10)$$

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 behavioural 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 (10) 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 behavioural 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 Online Appendix C.[36] 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

[36] 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. Although our setting does not

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 the 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 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 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 socio-demographic characteristics like gender or *age at lay-off*, 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 behaviour in anticipation of qualifying for longer UI coverage. The selection into these behavioural 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.[37] All in all, finding out which heterogeneities allow welfare-improving targeting in different policies is a fruitful avenue for future research, although policymakers might ultimately refrain from exploiting some of them, because of, e.g., transparency, administrative costs, horizontal equity and fairness concerns.

5. 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 lay-offs around an age at lay-off 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

feature a direct financial transfer from the individual to the government—like a price—Proposition 1 reveals analogous selection effects as in their setting.

[37] Our empirical strategy is only suited to study cut-off rules in some cardinal individual characteristic, as it would be the case with patterns in job histories.

manipulators. We argue that changes in subsequent job search intensities are informative about the underlying motives for manipulation and we identify long-term non-employment 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.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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Advance Access Publication Date: 25 October 2025