The London School of Economics and Political Science

Essays in Labour and Public Economics

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

As a disclaimer, I declare that the views expressed in this thesis belong solely to the authors and do not necessarily reflect those of the Bank of Italy nor of INPS.

I declare that my thesis consists of approximately 38,000 words.

Statement of conjoint work

I confirm that Chapter 1 was jointly co-authored with Kilian Russ and Vincenzo Scrutinio, and I contributed 33% of this work.

I confirm that Chapter 2 was jointly co-authored with Andrea Linarello, and I contributed 50% of this work.

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Abstract

This thesis consists of three chapters, all of which make extensive use of Italian administrative data. The first chapter studies strategic delays in the timing of layoffs around an age-at-layoff threshold entitling workers to a four month increase in potential unemployment insurance (UI) benefit duration. After having documented sizeable manipulation of age at layoff near the threshold, we show that the ensuing increase in UI benefit receipt is 81% mechanically due to higher coverage and only 19% the result of moral hazard responses. The second chapter documents the effects of increased import competition from China on the Italian labour market. In the first part of the paper, we show that areas that were initially specialized in import-competing industries suffered larger losses in manufacturing employment. However, these effects are modest in size. In the second part of the paper, we show that incumbent manufacturing workers did not suffer long-term economic losses. Although they spent less time at their initial employers, they were able to carry out successful transitions towards other sectors, in areas with better job opportunities. The third chapter studies the labour market outcomes of individuals starting an apprenticeship and compares them with those of similar individuals starting temporary contracts that, at least formally, do not provide training. I show that while apprenticeships increase the probability of conversion to open-ended contracts, especially at the initial firm, they also decrease the probability of obtaining further temporary contracts. Quantitatively, this second effect prevails, generating a negative effect on the probability of obtaining any job.

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Chapter 1

Happy Birthday? Manipulation and Selection in Unemployment Insurance

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Abstract

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

1.1 Introduction

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

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

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

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

While the above numbers are large, it is important to keep in mind that manipulation provides

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

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

Our survival analysis reveals that approximately 81% of the increase in UI benefit receipt is mechanically due to higher coverage, while the remaining 19% are the result of decreases in job search effort. Put differently, for one euro of mechanical UI transfer the government pays an additional 24 cents due to behavioral responses. Interestingly, we find virtually the same result when studying non-manipulators, i.e. individuals who were laid off just before their fiftieth birthday. This implies that manipulators are not adversely selected on their efficiency cost, which may mitigate concerns about anticipated moral hazard being the prime motive for selection into manipulation. Contrary, we document that manipulators are highly selected on long-term nonemployment risk. Even absent manipulation, manipulators would have exhausted eight months of UI benefits with 16.8 p.p. higher probability than non-manipulators.

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

Together our results document widespread manipulation in unemployment insurance and identify long-term nonemployment risk as an important motive to engage in manipulation. These findings highlight the importance of studying the underlying motives for manipulation and might influence how manipulation is perceived. Our analysis also implies that the type of manipulation we

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

consider has only modest effects on economic efficiency, a conclusion that would not hold if anticipated moral hazard were a prime motive for manipulation. This in turn has implications for the design of optimal differentiated UI policies.[3]

Our work relates to several strands of the literature. A large body of work studies the disincentives effect and the effect on post-reemployment outcomes, such as wages, of UI, exploiting similar policy variation, see e.g. Card et al. [2007], Rosolia and Sestito [2012], Schmieder et al. [2012], Landais [2015], Nekoei and Weber [2017], Johnston and Mas [2018] among others. Contrary to our setting, these papers rely on the *absence* of manipulation to identify the treatment effects of interest, whereas we study the effect of manipulation in a setting where it does occur. Furthermore, while most previous studies of UI focus on the distortion of job search efforts of the unemployed, we examine strategic behavior at the point of layoff. Our work closely relates to two recent contributions by Doornik et al. [2018] and Khoury [2018] who exploit manipulation in UI systems around an eligibility and seniority threshold in Brazil and France, respectively. Doornik et al. [2018] provide evidence of strategic collusion between workers and firms who time layoffs to coincide with workers' eligibility for UI benefits in Brazil. Khoury [2018] exploits a discontinuity in benefit levels for workers laid off for economic reasons and estimates an elasticity of employment spell duration with respect to UI benefits of 0.014. Due to the nature of their policy variation neither of these papers studies the selection patterns we analyze in our work. From a methodological perspective our work is most closely related to the work by Diamond and Persson [2017], who study manipulation in Swedish high-stakes exams. The construction of the manipulation region and of the counterfactual density relies on standard bunching techniques, such as Saez [2010], Chetty et al. [2011] and Kleven and Waseem [2013].

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

The fact that we find substantial manipulation and positive selection on long-term nonemployment risk also speaks to a recent literature studying the role of private information and adverse selection

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

in unemployment insurance, see e.g. Hendren [2017] and Landais et al. [2017]. This literature studies the role of private information about job loss risk in shaping the market for UI. Our results indicate that individuals hold information about their expected duration of unemployment at the point of layoff. Understanding to what degree this information is held privately is beyond the scope of this paper.

The remainder of this paper is organized as follows: Section 1.2 introduces the institutional setting and describes the data; Section 1.3 describes our quantities of interest and presents our identification strategy; Section 1.4 explains how we implement the latter in practice; Sections 1.5 and 1.6 report the results of our empirical analysis and robustness checks; Section 1.7 concludes.

1.2 Institutional Setting and Data

1.2.1 Institutional setting

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

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

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

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

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

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

Two other UI benefit schemes were in place in Italy at the same time of our analysis: Reduced Unemployment Benefits (RUB) and Mobility Indemnity (MI). However, neither one is likely to interfere with our analysis due to different eligibility conditions and less generous benefit coverage. For completeness, we present the two other UI schemes in Appendix 1.A.

1.2.2 Data

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

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

For our main sample we restrict attention to individuals who lost their job between February 2009 and December 2012, were between 46 and 54 years of age at the time of layoff, and claimed OUB. Unfortunately, our data does not cover the years prior to February 2009 and the introduction of a new UI scheme in January 2013 prevents us from including later years. We further restrict attention to individuals who separate from an employer in the private sector after a permanent contract. The motivation for this is twofold. First, we show in Section 1.5.5 that manipulation is confined to permanent contract private sector work arrangements. Second, the UNIEMENS database does not contain job information for public sector jobs, which means we have no information about the previous work arrangement, nor would we observe re-employment. At this point, one might be worried that we are missing some re-employment

[7] *Sistema Informativo Percettori* in Italian.

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

events, namely, those into public sector jobs. This in unlikely to affect our results because transitions from private into public sector jobs are rare for workers at such late stage in their careers. We replicated the analysis for a subsample of individuals for whom we have information on the full contribution history and results are qualitatively similar. After the exclusion of a few observations with missing key information we are left with 249,581 separation episodes that lead to UI claims.

Table 1.1 reports summary statistics for our main sample. The average worker receives UI benefits for about 30 weeks (6.9 months) corresponding to roughly one third of the 90 weeks (21 months) average nonemployment duration. An average of 50% and 39% of workers are still nonemployed after eight and twelve months, respectively, implying substantial exhaustion risk. Our sample of workers is predominately male, on full time contracts, and employed in blue collar jobs. Workers have spent about 27.5 years in the labor market since their first job and almost 6 years in their last firm. In terms of geographic distribution, 46% of workers are laid off in the south or the islands.[9] Workers earned about 70 Euro per day which is equivalent to $70 \times 26 = 1820$ Euro per month if working full time. [10] The separating firm is relatively old (14 years) and large (28.16 employees), but this is driven by a few very large firms. Indeed, more than 60% of workers come from firms with less than 15 employees while only 18% come from firms with more than 50 employees. Because our main sample contains workers in their late forties and early fifties, one might be concerned that transitions into retirement play an important role. However, this is not the case with only about 1,500 or 0.6% of workers in our sample claiming retirement benefits before the end of our observation window.[11] We now turn to a description of our objects of interest, which precedes our identification strategy

1.3 Conceptual framework

1.3.1 The moral hazard cost of extended UI coverage

Manipulation provides individuals with additional UI coverage. As in any insurance context the increase in coverage might cause individuals to change their behavior by reducing the incentive to avoid adverse states of the world. This change in behavior, in our context a reduction in job search intensity, constitutes a classical moral hazard response. From an efficiency perspective

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

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

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

it is crucial to understand how much of the increase in total insurance payments is driven by changes in behavior and how much is mechanically due to higher coverage. We consider distinguishing between these two effects as one of this paper's main contributions. Quantifying the relative importance of these effects also leads to potentially different positive views about manipulation and the motivation behind it, which in turn might shape how the phenomenon is perceived both by policy makers and society at large.

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

$$\begin{aligned}\Delta B = B^{12} - B^{8} &= \int_0^{12} b_t \cdot S_t^{12}\, dt - \int_0^{8} b_t \cdot S_t^{8}\, dt \\ &= \underbrace{\int_0^{12} b_t \cdot (S_t^{12} - S_t^{8})\, dt}_{\text{behavioral response } (\Delta B^{MH})} + \underbrace{\int_8^{12} b_t \cdot S_t^{8}\, dt}_{\text{mechanical effect } (\Delta B^{ME})} \quad , \end{aligned} \tag{1.1}$$

where B and S denote the average benefit receipt and the survival probability each under the twelve and eight months PBD scheme, respectively, and b_t is the benefit amount in period t. The behavioral moral hazard response, ΔB^{MH}, captures the part of the benefit receipt increase that is paid due to the outward shift of the survival curve. The mechanical effect, ΔB^{ME}, corresponds to the remaining increase in benefit receipt that occurs even absent any behavioral response and is uniquely due to the additional UI coverage in months eight to twelve. Figure 1.1 illustrates decomposition (1.1) graphically by plotting hypothetical nonemployment survival rates under the eight and twelve months PBD scheme, under the simplifying assumption of a constant benefit level. The total increase in benefit receipt corresponds to the sum of the behavioral/moral hazard effect (dark gray area) and mechanical effect (light gray area).

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

$$\frac{BC}{MC} = \frac{\Delta B^{MH}}{\Delta B^{ME}}. \tag{1.2}$$

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

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

$$\Delta N = N^{12} - N^{8} = \int_0^\infty b_t \cdot S_t^{12} dt - \int_0^\infty b_t \cdot S_t^{8} dt = \underbrace{\int_0^\infty b_t \cdot (S_t^{12} - S_t^{8}) dt}_{\text{behavioral response } (\Delta N^{MH})} \tag{1.3}$$

where, as above, N and S denote the average nonemployment duration and the survival rate each under the twelve and eight months PBD scheme, respectively. Since all of the increase in nonemployment duration constitutes a moral hazard response, we add the resulting cost to the behavioral cost and adjust formula 1.2 as follows:

$$\frac{BC}{MC}^{\tau} = \frac{\Delta B^{MH} + \tau \cdot \Delta N^{MH}}{\Delta B^{ME}}, \tag{1.4}$$

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

1.3.2 Identification strategy

This section provides a self-contained sketch of our estimation strategy and explains the sources of variation in the data that are used to pin down parameters of interest. The main idea is to exploit the local nature of manipulation by extrapolating outcomes from regions that are unaffected by manipulation to learn about what would have happened in the manipulation region in the absence of it. We first assess the range of the manipulation region with standard bunching techniques. We then fit polynomials to the unmanipulated part of the data and interpolate to construct a counterfactual layoff frequency and recover the number and share of manipulators.

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

Similarly, we construct counterfactuals of outcomes that are not directly manipulated, such as subsequent benefit receipt or nonemployment survival probabilities, to learn whether these outcomes respond to manipulation. Intuitively, any unusual change in these outcomes near the cutoff together with an estimate of how many manipulators are causing it, let us recover manipulators' responses. Under minimal additional assumptions, estimates of the response for the average individual combined with the share of individuals who are manipulators let us recover the responses of non-manipulators, whom we use to benchmark manipulators' responses. We also illustrate how we can use part of the procedure just described to study selection into manipulation. Our approach is closely related to that of Diamond and Persson [2017]. In the remainder of this section we lay out our approach in more detail.

Quantifying manipulation: Consider a hypothetical manipulated layoff density as in Figure 1.2a. Absent any manipulation we would expect the frequency of layoffs to be smooth in the neighborhood of the cutoff. Manipulation instead causes a sharp drop in the number or layoffs right before and a spike right after age fifty. As in standard bunching techniques, we recover the counterfactual frequency of layoffs by fitting a polynomial to the unmanipulated parts of the data (on the left and right of the cutoff) and interpolate inwards. We determine the lower bound of the missing region by visual inspection, and then iteratively try different upper bounds of the excess region until we are able to balance the missing and excess mass. The difference between the observed frequency and the fitted counterfactual lets us recover missing and excess shares, as well as the number of manipulators in each bin of the missing and excess regions. This estimation strategy assumes that manipulation takes the form of a pure re-timing of layoffs that would have occurred anyways. One concern is that the increase in PBD at the age threshold leads to extensive margin effects [Jäger et al., 2018]. We provide evidence that this is not the case in our setting in Section 1.6.2.

Effects of manipulation: Equipped with a measure of how many manipulators there are, we then study outcomes which are not directly manipulated but potentially affected by it. Figure 1.2b illustrates the idea for one of our outcomes of interest: nonemployment survival rates. Manipulation provides workers with additional UI coverage from month eight to twelve. Thus, it is likely that nonemployment survival rates respond to the increase in coverage. Consider a hypothetical statistical relationship between nonemployment survival and age at layoff, as in Figure 1.2b. In order to estimate how manipulators' survival rate responds, we take the difference between two quantities: manipulators' actual survival probability and manipulators' counterfactual survival probability had they not been able to manipulate. As illustrated in Figure 1.2b, we obtain these quantities by separately studying the missing and excess region. First, we fit a flexible counterfactual on the right side of the threshold and estimate the difference between the observed and predicted survival rates to assess manipulators' actual survival probability. Intuitively, survival rates in the excess region are higher than predicted by the un-manipulated region to the right only due to manipulation. The extent to which observed and predicted nonemployment survival rates differ, together with an estimate of how many manipulators are causing this difference, let us recover manipulators' actual nonemployment survival probability. We use analogous arguments to back out manipulators' counterfactual nonemployment survival probability on the left side of the threshold. The exact estimation and calculation steps are presented in Section 1.4.[13]

Effects of UI on the average individual and on non-manipulators: Counterfactual outcomes allow us to recover the statistical relationship between such outcomes and age-at-layoff, absent manipulation. Under some assumptions the jump in counterfactual outcomes at the threshold gives us an estimate of the treatment effect of additional UI coverage for the average individual in the population, akin to a Donut-RD design [Barreca et al., 2011]. In Figure 1.2b this would correspond to the difference between the grey dots on the right and on the left of the threshold. Responses obtained in this way are nothing but a weighted average of responses for manipulators' and non-manipulators, absent manipulation. Assuming that manipulators' response after receiving four extra months of UI does not depend on whether they have *chosen* to manipulate or whether they have been randomly assigned to such treatment, then the Donut-RD coefficient, together with previously estimated manipulators' response and shares allow us to recover the implied response for non-manipulators. We use the latter to benchmark the results for manipulators.

Selection into manipulation: The procedure illustrated in Figure 1.2b also lets us study selection into manipulation by comparing manipulators' counterfactual outcomes to non-manipulators realized outcomes. Figure 1.2b highlights this comparison and would suggest that even absent manipulation, manipulators would have had a higher nonemployment survival rate than non-manipulators due to the drop in the outcome variable to the left of the cutoff. This is indeed what we show in Section 1.5. In light of the selection patterns we document, it is worth bearing in mind that we are estimating the effect of manipulation on individuals who endogenously decide to engage in manipulation, akin to a local average treatment effect.

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

1.4 Regression Framework

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

1.4.1 Estimating the number of manipulators

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

$$c_j = \alpha + \sum_{p=0}^{P} \beta_p \cdot a_j^p + \sum_{k=z_L}^{z_U} \gamma_k \cdot \mathbb{I}[a_j = k] + \nu_j, \tag{1.5}$$

where c_j denotes the absolute frequency of layoffs in headcounts in bin j, a_j is the mid-point age in bin j, P denotes the order of the polynomial. Coefficients γ_s control flexibly (bin-by-bin) for differences between the observed data and the counterfactual frequency in the manipulation region $[z_L, z_U]$.[14] The whole counterfactual layoff frequency can be recovered from the fitted values of equation (1.5) omitting the contributions of the missing and excess region dummies, i.e. the counterfactual number of individuals in bin j is given by $\hat{c}_j = \sum_{p=0}^{P} \hat{\beta}_p \cdot a_j^p$. Notice that $\hat{\gamma}_k < 0$ if k belongs to the portion of the manipulation region before age fifty, while $\hat{\gamma}_k > 0$ in the portion of the manipulation region after age fifty. This sign difference will be important below when we compute the shares of manipulators.

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

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

analysis. For practical purposes we will refer to them as the "missing region" and the "excess region", respectively.

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

$$N^{\text{missing}}_{\text{mani}} = \sum_{k \in \text{missing}} |\hat{\gamma}_k| \tag{1.6}$$

$$N^{\text{missing}}_{\text{non-mani}} = \sum_{k \in \text{missing}} c_k. \tag{1.7}$$

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

$$N^{\text{excess}}_{\text{mani}} = \sum_{k \in \text{excess}} \hat{\gamma}_k \tag{1.8}$$

$$N^{\text{excess}}_{\text{w/o mani}} = \sum_{k \in \text{excess}} c_k - \hat{\gamma}_k, \tag{1.9}$$

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

$$s^{\text{missing}} = \frac{N^{\text{missing}}_{\text{mani}}}{N^{\text{missing}}_{\text{mani}} + N^{\text{missing}}_{\text{non-mani}}} \tag{1.10}$$

$$s^{\text{excess}} = \frac{N^{\text{excess}}_{\text{mani}}}{N^{\text{excess}}_{\text{mani}} + N^{\text{excess}}_{\text{w/o mani}}}. \tag{1.11}$$

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

$$s_k^{\text{missing,}} = \frac{|\hat{\gamma}_k|}{|\hat{\gamma}_k| + c_k} \quad \text{for } k \in \text{missing} \tag{1.12}$$

$$s_k^{\text{excess,}} = \frac{\hat{\gamma}_k}{c_k} \quad \text{for } k \in \text{excess.} \tag{1.13}$$

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

1.4.2 Estimating the effects of manipulation

In the previous section we constructed the number of manipulators and the share they represent in the missing and excess region. We now move to the estimation of the effect of manipulation on outcomes, such as benefit receipt or nonemployment survival, that are not directly manipulated but might respond to manipulation. As outlined in Section 1.3.1, we relate differences in observed and predicted outcomes in the missing and excess region to the missing and excess share of manipulators to recover our outcomes of interest.

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

$$\begin{aligned} y_i = {} & \alpha + \sum_{p=1}^{P} \beta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i \leq 50] + \sum_{p=0}^{P} \beta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i > 50] \\ & + \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \xi_i, \end{aligned} \tag{1.14}$$

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

Specification (1.14) allows for a treatment effect of longer PBD on average outcomes, i.e. $\beta_0^{>50}$. We refer to $\beta_0^{>50}$ as the "Donut" RD coefficient. This coefficient captures the average treatment effect of four additional months of PBD for the average individual in the population, as shown in Barreca et al. [2011]. We will use it to benchmark our results for the response of manipulators (more on this below). Intuitively, $\beta_0^{>50}$ recovers the difference between the two grey dots in

Figure 1.2b.

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

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} - \bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\delta_k}{s_k^{\text{missing}}} \tag{1.15}$$

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

$$\bar{Y}^{\text{missing}}_{\text{non-mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k}, \tag{1.16}$$

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

$$\bar{Y}^{\text{missing}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} - \frac{\delta_k}{s_k^{\text{missing}}} \tag{1.17}$$

and manipulators average counterfactual outcome over the entire missing region as

$$\bar{Y}^{\text{missing}}_{\text{mani}} = \frac{1}{N^{\text{missing}}_{\text{mani}}} \sum_k |\hat{\gamma}_k| \cdot \bar{Y}^{\text{missing}}_{\text{mani},k}, \tag{1.18}$$

where the $\hat{\gamma}_k$ are estimated in Section 1.4.1.[15] The logic behind this re-scaling is straightforward: if we found that the absence of 10% of individuals in the missing region resulted in a 100 unit drop starting from a predicted counterfactual of 1000 units, we could infer that the now missing individuals must have had an outcome of $\frac{1000-0.9\times(1000-100)}{0.1} = 1900$ units on average.

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

$$\bar{Y}^{\text{excess}}_{\text{mani},k} - \bar{Y}^{\text{excess}}_{\text{w/o mani},k} = \frac{\delta_k}{s_k^{\text{excess}}}. \tag{1.19}$$

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

[15]Equation 1.18 is nothing but an application of the law of iterated expectations, as average outcomes in the bins are aggregated using the share of manipulators in each bin.

$$\bar{Y}^{\text{excess}}_{\text{observed},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} = \frac{\gamma_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k} + (c_k - \gamma_k) \cdot \bar{Y}^{\text{excess}}_{\text{w/o mani},k}}{c_k}. \tag{1.20}$$

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

$$\bar{Y}^{\text{excess}}_{\text{mani},k} = \frac{\sum_{i=1}^{N} y_i \cdot \mathbb{I}[a_i = k]}{c_k} + (1 - s^{\text{excess}}_k) \cdot \frac{\delta_k}{s^{\text{excess}}_k}, \tag{1.21}$$

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

$$\bar{Y}^{\text{excess}}_{\text{mani}} = \frac{1}{N^{\text{excess}}_{\text{mani}}} \cdot \sum_k \gamma_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k}, \tag{1.22}$$

which, together with equation (1.18) lets us define manipulators' response (or treatment effect) as

$$Y^{TE}_{\text{mani}} \equiv \bar{Y}^{\text{excess}}_{\text{mani}} - \bar{Y}^{\text{missing}}_{\text{mani}}. \tag{1.23}$$

1.4.3 Recovering the implied response of non-manipulators

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

$$s^{\text{missing}} \cdot Y^{TE}_{\text{mani}} + (1 - s^{\text{missing}}) \cdot Y^{TE}_{\text{non-mani}} = \beta_0^{>50}. \tag{1.24}$$

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

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

Rearranging thus gives us an estimate for non-manipulators' response:

$$Y^{TE}_{\text{non-mani}} = \frac{\beta_0^{>50} - s^{\text{missing}} \cdot Y^{TE}_{\text{mani}}}{1 - s^{\text{missing}}}. \tag{1.25}$$

1.5 Results

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

1.5.1 Evidence of manipulation

To provide graphical evidence of manipulation, Figure 1.3 plots the relative frequency of layoffs against workers' age at layoff. Figure 1.3b covers the entire age range from 26 to 64 years of age, while Figure 1.3a zooms into a narrower, four year window around the age-fifty threshold.[17]

[17] By plotting the layoff frequency over the entire age range in Figure 1.3b, we already rule out that manipulation is caused by other mechanisms like (round-) birthday effects. All our estimates for the counterfactual density and counterfactual outcomes are based on the narrower (46-54) window. Section 1.6 presents additional robustness checks.

Both figures show a clear drop in the frequency of layoffs just before, and a pronounced spike after, the age-fifty threshold.

Following our estimation strategy outlined in Section 1.4.1, we find the manipulation region to consist of all bins from six weeks before (missing region), up to four weeks after the threshold (excess region). Table 1.2 reports our estimates for the respective headcounts for the four groups of interest: manipulators in the missing region, non-manipulators in the missing region, manipulators in the excess region and all individuals in the excess region who are not manipulators, as well as share estimates for the missing and excess region (see equations (1.6) - (1.11) above). We estimate that a total of 571 layoffs are strategically delayed corresponding to 15.8% of layoffs in the missing region. The counterfactual relationship appears almost perfectly linear and is robust to the choice of the order of the polynomial. The estimated number of manipulators in the excess region, 609, deviates slightly from that in the missing region due to measurement error and corresponds to approximately 20.3% of layoffs in the excess region.

We consider the evidence presented until here as this papers' first contribution. It documents that incentives generated by the UI system can influence the timing dimension of layoffs and thereby the length of an employment spell. Complementing previous work on the extensive margin response of job separations, we focus on the timing dimension of the layoff decision.[18] Having established sizable manipulation, we now turn to the estimation of its effect on manipulators' benefit receipt.

1.5.2 Effects of manipulation: UI benefit receipt and duration

Successful manipulation provides workers with four more months of potential UI coverage, after the eighth month of nonemployment. In this section we study the effects of such longer coverage on manipulators' actual benefit receipt and benefit duration. We begin by plotting these outcomes against workers' age at layoff in Figure 1.4a and 1.4b, respectively. The observed pattern in the raw data fits with the model of manipulation we laid out in Section 1.3 and constitutes clear non-parametric evidence that UI receipt and actual duration respond to manipulation. As explained in Section 1.4.2 our procedure combines abnormal changes in outcomes near the threshold with the share of manipulators causing them. This allows us to retrieve manipulators' as well as non-manipulators' responses.

We report all relevant estimates with associated 95% confidence intervals in Tables 1.3 and 1.4. Our estimates indicate that manipulators would have collected 5814.2 Euros, and spent 27.8 weeks on benefits, had they not manipulated (column (1)). When manipulation lengthens

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

individual UI coverage, these figures jump up to 8053.6 Euros and 41.8 weeks, generating an increase in fiscal outlays of 2239 Euros per manipulator. In order to benchmark this number, we compute the same increase for non-manipulators, which we report in column (6). We find that this corresponds to 1637 Euros only. From an accounting perspective our results indicate that overall it would be cheaper to finance longer coverage for non-manipulators rather than for manipulators. However, the size of the efficiency cost of financing manipulators' crucially depends on subsequent behavioral changes that purposefully reduce the probability of finding a new job. We therefore ask a more interesting question: what fraction of these additional UI expenditures is actually due to behavioral responses and how much is instead mechanically due to longer coverage? In the next subsection we make use of a survival analysis to shed light on this question.

1.5.3 Distinguishing behavioral responses from mechanical effects

In this section we make use of the methodology presented in Section 1.3 to decompose UI receipt and actual duration response into a mechanical and a behavioral component, so as to shed light on the effective moral hazard cost of manipulation. Nonemployment survival probabilities, together with information on statutory replacement rates, are the crucial pieces of information needed to measure the relative size of these two sources of cost. Intuitively, it is important to understand *when* manipulators respond, in order to distinguish between relatively expensive moral hazard responses during months of benefit receipt from those that happen after benefit exhaustion.

Similarly to what we did for UI receipt and duration, in Figure 1.5 we report the observed relationship between survival in nonemployment and age at layoff for a selected set of months after separation. Qualitatively, we observe bigger jumps around the thresholds precisely during the months with extra coverage. Within the manipulation region we also see outcome changes that are abnormal, compared to what could be predicted by the data outside of it. Similarly to before, we combine these changes with the share of manipulators causing them to trace monthly survival curves for both manipulators and non-manipulators.

Figure 1.6a shows the estimated nonemployment survival curve of manipulators under the eight and twelve months PBD scheme. Figure 1.6b reports the difference between the two curves at any point, with associated bootstrapped 95% confidence bands. The difference between the two curves reveals the effect of longer PBD along manipulators' survival curve. It shows virtually no difference in survival probabilities in the first six to seven months, after which the two curves start diverging. The shift in manipulators' survival curve is substantial with their nonemployment probability after twelve months increasing by 16.7 p.p. due to the more generous scheme. Perhaps unsurprisingly, the behavioral response is concentrated in the months eight to twelve and coincides with the time of extended UI coverage. However, as pointed out, there is very little evidence of moral hazard in the first eight months of nonemployment. The difference between the two curves then decreases again after month twelve, consistent with the idea that these individuals increase their job search efforts again once the benefits expire. We replicate the same type of analysis for non-manipulators and report it in Figure 1.7a and 1.7b. The qualitative picture is similar. Also in this case we see very limited anticipatory responses of longer coverage during the months zero to eight, and a pronounced divergence after month eight, indicative of a moral hazard response.[19]

Absolute shifts alone are not appropriate to represent efficiency costs because they ignore the fact that not all individuals have the same probability of still being nonemployed during the periods of longer coverage. To solve this issue we follow Schmieder et al. [2012] and compute BC/MC ratios, as detailed in Section 1.3.1. We compute these ratios by numerically integrating the survival curves over the relevant ranges, and appropriately weighting by statutory survival rates. We perform integration by using the midpoint rule and impose that the behavioral cost has to be weakly positive at any given point.[20]

We report BC/MC ratios in Table 1.5. In column (1) we report the simple BC/MC ratio, as in equation 1.2. Manipulators' estimate of 0.24 implies that for one additional Euro used to provide longer UI coverage in the months eight to twelve the government would have to spend an additional 24 cents due to behavioral responses that occurs in months zero to twelve. The corresponding estimate for non-manipulators' is remarkably similar, implying that manipulators are not adversely selected on the basis of their *effective* moral hazard cost. In column (2) we enrich our analysis by following equation 1.4 by also considering the cost of lost tax revenue during the whole nonemployment spell. These numbers are higher because the government is marginally losing money out of the UI system due to long nonemployment durations. In selecting the tax rate we follow Schmieder and Von Wachter [2016] and use a 3% tax rate. Also in this case, numbers across the two groups are virtually identical. Together these results reinforce the idea that manipulators' responses in terms of decreased job search effort do not generate efficiency costs that are higher than those of the average individuals in the same age

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

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

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

1.5.4 Selection on long-term nonemployment risk

While manipulators are not adversely selected on their effective moral hazard cost, it is still true that financing their UI coverage is more expensive from a budgeting perspective. As a matter of fact in Table 1.3 we previously saw that providing four additional months of UI coverage increased the average UI benefit receipt by 2239 Euros for manipulators and only by 1637 Euros for non-manipulators. This seems to suggest that manipulators are instead adversely selected on their long-term nonemployment risk. In this subsection we corroborate this hypothesis by showing that manipulators' have higher UI exhaustion rates even when they have the same PBD as non-manipulators. Figure 1.8a illustrates this point by plotting survival rates for manipulators and non-manipulators under the eight month scheme. Comparing manipulators and non-manipulators when they face the same incentives isolates permanent differences in risk. The figure illustrates that even with shorter PBD, the probability of exhausting such benefits without finding a new job is almost 20 p.p. higher for manipulators. The large exhaustion risk is what makes most of the increases in benefit receipt and duration mechanical and thus lowers the BC/MC ratio, *ceteris paribus.*

1.5.5 Characterizing manipulators

Until now we have quantified manipulation and studied its consequences, but we have abstracted from understanding how it occurs. In this section we present a characterization of the manipulators along observable characteristics, in order to provide some suggestive evidence on the economic mechanisms that generate it. In Figure 1.9 we start by visually inspecting the age distribution of layoffs for different types of contracts (permanent and temporary) and sectors (private and public). Workers in the public sector, either with permanent or temporary contracts, show little ability or interest to delay their layoff and the density of layoff does not exhibit any discontinuous pattern for either of these groups. The density for workers laid off from permanent contracts in the public sector also shows substantial variance, due to a smaller number of individuals. Once we move to the private sector, we can observe that workers on permanent contracts are able to manipulate their date of layoff, while the same is not true for workers on temporary contracts. This is consistent with temporary workers having little ability choose a start date for their contracts that positions them on the right-hand-side of the threshold, once laid off. It is also consistent with lower bargaining power with the employer, due to e.g. shorter tenure.[21]

In what follows we focus on the subset of workers who claimed UI after losing a permanent job in the private sector, which was also our sample of interest in the main analysis. To provide a more precise assessment, we make use of a procedure developed in [Diamond and Persson, 2017, Section 6.2]. The idea is similar in spirit to the rest of our analysis. Let us say that we want to investigate whether manipulators are more likely to have a given characteristic, e.g. being female. If there are disproportionately more (less) women in the excess (missing) region compared to what a fitted counterfactual would predict, then manipulators are more likely to be female. Results are in Table 1.7. Columns (1) and (2) report the estimated mean characteristic for manipulators and non-manipulators, respectively. The difference of the two is reported in column (3), together with bootstrapped 95% confidence intervals. In column (4) we report the estimated mean for yet another group, i.e. all individuals whose unmanipulated age-at-layoff falls in the missing region. We find that manipulators are 18 p.p. more likely than non-manipulators to be female, 17 p.p. more likely to be employed in white collar jobs and 7 p.p. less likely to have full-time contracts. We observe that their wages are 6% lower, although estimates are relatively imprecise. No significant difference emerges in terms of tenure and geographic location. We notice that firm size is an important element: manipulators come from firms that are about 40% smaller with respect to firm of non-manipulators. We only see minor and statistically insignificant differences in terms of age of the firm. We can only speculate as to the reasons behind the firm-size differential in manipulation: the effect may work through personal relationships, workers' (credible) threat to sue the firm for unjust dismissal, or direct bribes paid with part of the extra UI. Our data do not allow us to disentangle these possibilities and leave this question to future research. Overall, these findings suggest that adjustment costs, bargaining power and proximity to managers play a role in workers' ability to engage in manipulation.

1.6 Robustness

1.6.1 Placebo tests

One key identifying assumption of our empirical strategy is that the bunching patterns we observe in the data just reflect the strong incentives given by higher PBD and are not linked to

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

other institutional features of the labor market discretely changing at age fifty. In this subsection we test this assumption by looking at two other UI schemes that were introduced *after* 2012 and that did not feature sharp changes in generosity at age fifty. Intuitively we would expect to see no missing and excess mass to the left and to the right of the threshold, respectively. In Figure 1.10 we report the corresponding layoff densities. In order to be consistent with our original sample definition, we focus on workers who were employed on permanent contracts in the private sector. In both cases we fail to detect any graphical evidence of manipulation and see that the density evolves smoothly around the threshold. This suggests that the discontinuous shape of the density in our main sample is directly related to the PBD extension that characterized the OUB scheme.

1.6.2 Extensive margin responses

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

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

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

1.6.3 Testing for shifts in the density

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

$$d_j = \alpha + \lambda \cdot a_j + \gamma \cdot \mathbb{I}[a_j \geq 50] + \delta \cdot \mathbb{I}[a_j \geq 50] \cdot a_j + \nu_j, \tag{1.26}$$

where d_j is the density of layoffs in bin j, a_j is the mid-point age in the bin and ν_j is an error term. Our coefficient of interest is γ, which represents the possible discontinuity in the density at the age fifty threshold. Ideally the coefficient should be close to zero, indicating no extensive margin responses.[22] We report the results in Table 1.8. In column (1) we run equation 1.26 on the whole sample, that is also including the manipulation region. As expected we detect

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

a significant jump at the threshold, which is consistent with excess layoffs after age fifty. In column (2) we run the same model but exclude observations in the manipulation region. We find that the discontinuity becomes much less relevant quantitatively, and statistically not different from zero. In column (3) we repeat the same exercise but with an alternative and extended definition of the manipulation region. Contrary to the traditional Kleven and Waseem [2013] method here we use as missing (excess) region the one characterized by the longest sequence of negative (positive) coefficients starting from the threshold. The resulting missing region is substantially larger and it goes up to 4 months before the cutoff (9 bins) while the excess region is remarkably similar and it adds only a couple of bins to the one used in our baseline estimates.[23] This involves a simple assumption of continuity and increasing cost of manipulation in the distance from the threshold, and delivers convex missing and excess regions. Also in this case the estimate for γ is quantitatively negligible.

1.6.4 Testing for discontinuities in observable characteristics

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

$$x_i = \alpha + \sum_{p=1}^{P} \lambda_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i < 50] + \sum_{p=0}^{P} \lambda_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i \geq 50] + \xi_i \tag{1.27}$$

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

$$\begin{aligned} x_i = \kappa &+ \sum_{p=1}^{P} \theta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i < 50] + \sum_{p=0}^{P} \theta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i \geq 50] \\ &+ \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \nu_i, \end{aligned} \tag{1.28}$$

If manipulators are selected on observables, we would expect $\lambda_0^{>50}$ to be different from zero, a

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

point also raised in section 1.5.5. If however manipulation is the only reason why selection arises, we would expect $\theta_0^{>50}$ to be equal to zero. We reports tests on these two coefficients in Table 1.6 for a large set of observable characteristics. Columns (1) to (3) report estimates from model 1.27. Observable characteristics are indeed different on the two sides of the threshold, because of manipulation, but potentially also because of extensive margin responses. Columns (3)-(5) rule this last channel out. The fact that, after accounting for manipulation, the distribution of observable characteristics is continuous at the threshold makes the presence of additional layoffs related to changes workers' outside options. This is very reassuring for the validity of our design, as it seems that changes in PBD do not induce extensive margin changes in the number of layoffs.

1.6.5 Testing for the presence of extra excess mass

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

$$c_j = \alpha + \beta a_j + \sum_{k=A}^{50^-} \tilde{\gamma_k} \cdot \mathbb{I}[a_j = k] + \sum_{k=50^+}^{B} \tilde{\delta_k} \cdot \mathbb{I}[a_j = k] + \zeta_j \tag{1.29}$$

Where the set of $\tilde{\gamma_k}$ and $\tilde{\delta_k}$ coefficients are enough to measure the size of the manipulation region. Same as in 1.6.3 we consider an extended manipulation region that includes bins from 18 weeks before workers' fiftieth birthday up to 8 weeks afterwards. The lower and upper bounds are denoted by $A < z_L$ and $B > z_U$, respectively. After having estimated the previous equation we rescale the difference between excess and missing mass by the excess mass itself. This yields the share of the excess mass that can be explained by extensive margin responses. Such share amounts to only 1.3%, which is very reassuring about the validity of our identification strategy.

1.6.6 Why are extensive margin responses so small?

In this subsection we discuss why it might be plausible that our extensive margin responses are smaller compared to those found in the studies of Jäger et al. [2018] and Albanese et al. [2019].

Broadly speaking, the reasons have to do with the fact that benefit changes at the threshold are smaller compared to those in these studies, and also that some institutional features in our setting limit the scope for big behavioral responses at the extensive margin.

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

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

We now turn to comparing our work to Albanese et al. [2019], who find a sizable increase in the separation rate for workers who become eligible to the OUB scheme in Italy for the first time. We present two reasons why we think these responses are unlikely to be present in our sample, although we are studying the same UI scheme. First of all it is worth stressing that the workers in our sample have already experienced a jump their PBD in the past, precisely when they met their eligibility criteria. It follows that the observed matches, which end in a separation in our dataset, have already survived a large increase in their outside option, so they should be less sensitive to further increases in it. Secondly, Albanese et al. [2019] exploit

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

variation in UI eligibility rules, which allow workers with no UI to have access to some. We instead study variation at the *intensive* margin, since our workers obtain four extra months of PBD. Whether these two responses should be the same has not been explored so far but it can be argued that the former should be larger than the latter. To our knowledge there is no explicit analysis of this aspect in existing studies and we leave it to future research. All in all, all these considerations might explain the discrepancy between our results and the higher probability of separation identified by Albanese et al. [2019].

1.7 Concluding Remarks

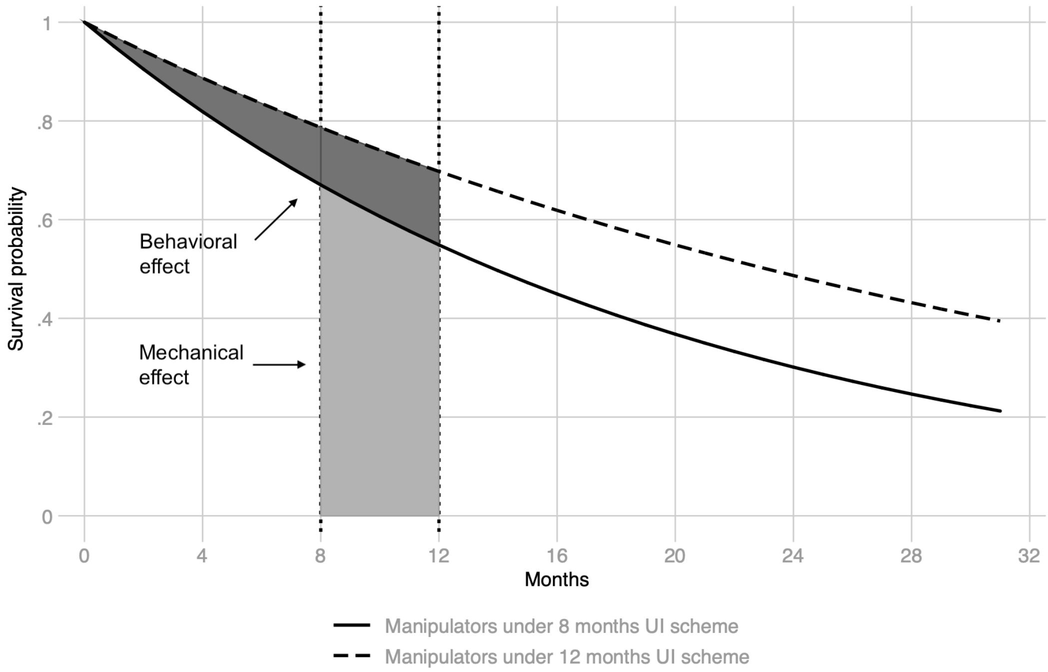
This paper studies manipulation in the context of unemployment insurance. We document substantial manipulation in forms of strategic delays in the timing of layoffs around an age-at-layoff threshold entitling workers to a four months increase in potential UI benefit duration in Italy. Using bunching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

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

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

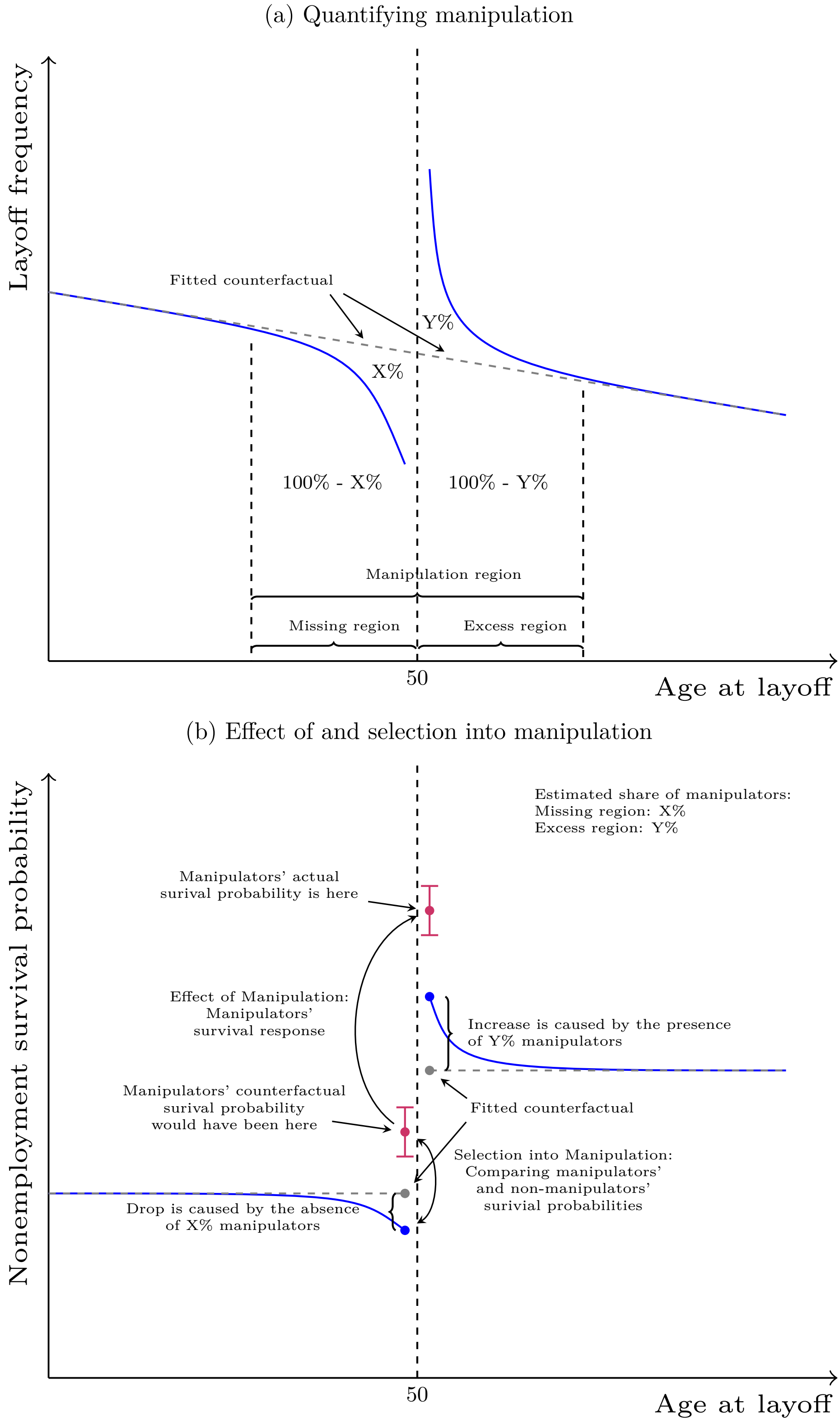
Figures

Figure 1.1: The moral hazard cost of extended UI coverage



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

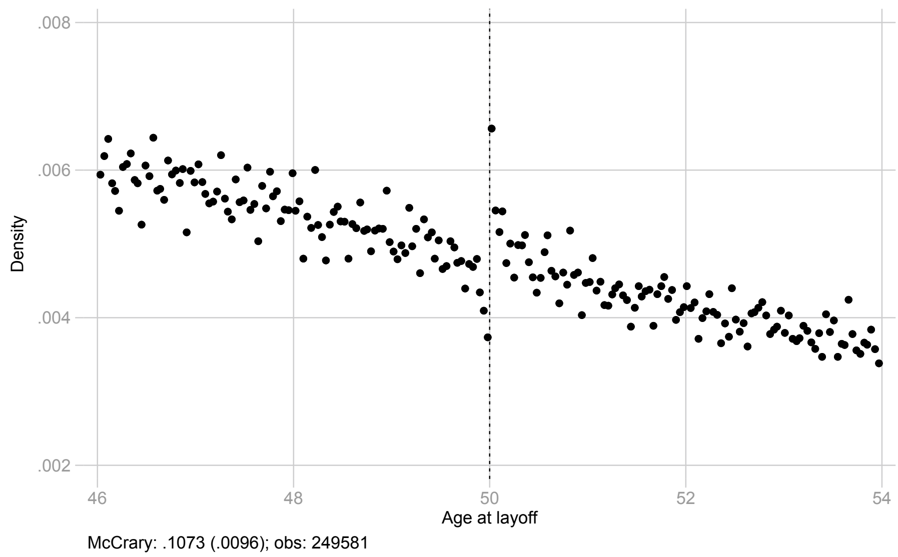
Figure 1.2: Illustration of identification strategy



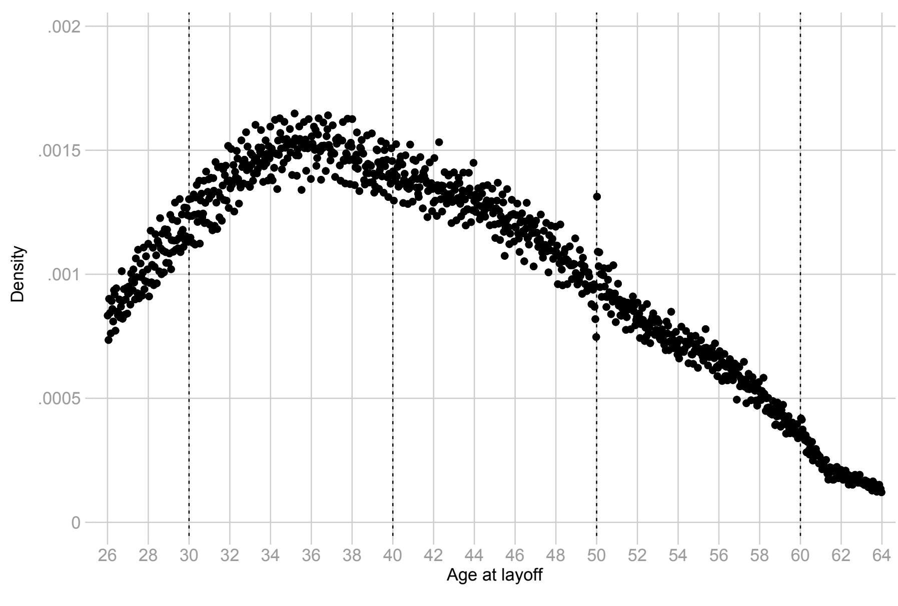
Note: The figure visualizes our identification strategy. Panel (a) illustrates how we estimate the number and respective share of manipulators in both the missing and excess region. Panel (b) constructs manipulators' survival response and illustrates the relevant comparison when studying selection into manipulation. Section 1.4 lays out how we estimate the fitted counterfactuals in practice.

Figure 1.3: Layoff frequency for permanent contract private sector workers

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



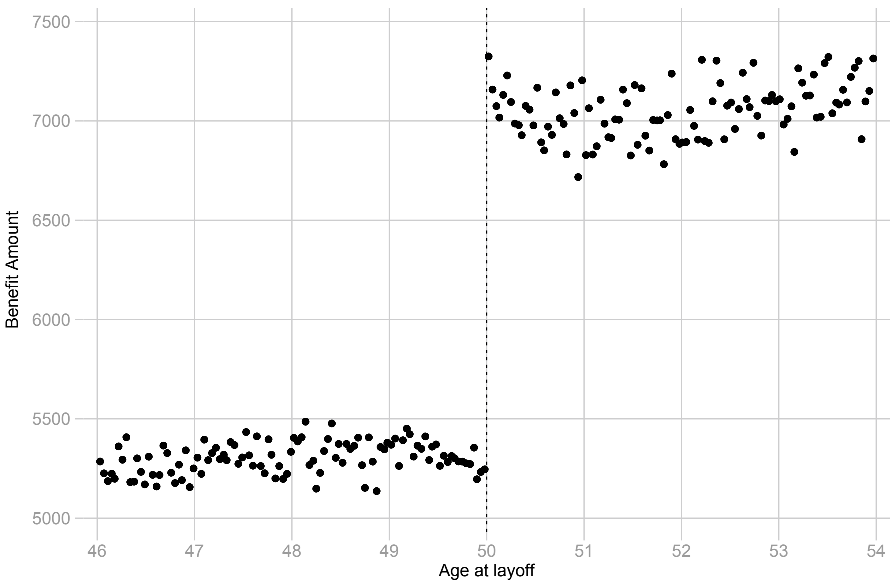
(b) Age-at-layoff between 26 and 64 years



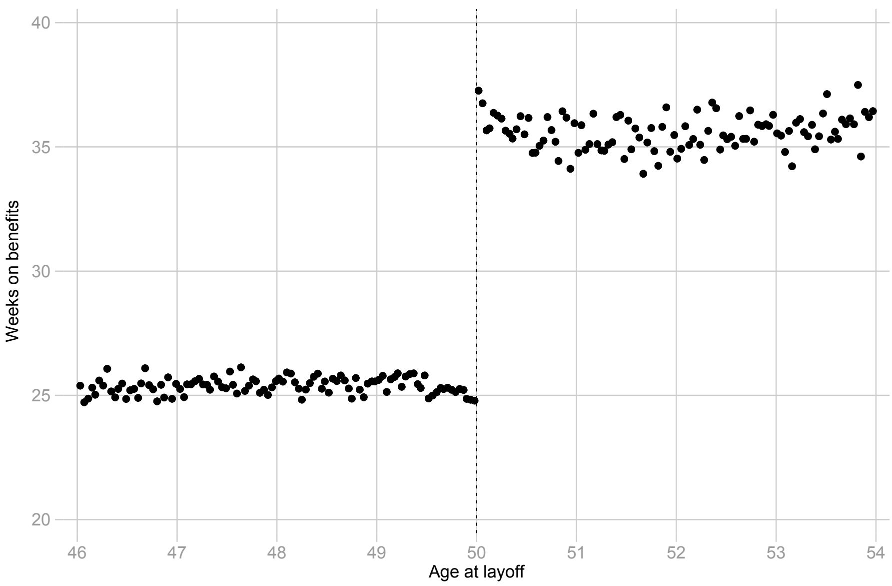
Note: The figure shows the density of layoffs in the private sector, for individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period February 2009 till December 2012. Panel (a) plots the density for the age range from 46 to 54 years, while Panel (b) does so for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

Figure 1.4: Benefit receipt and duration

(a) average UI receipt in euros



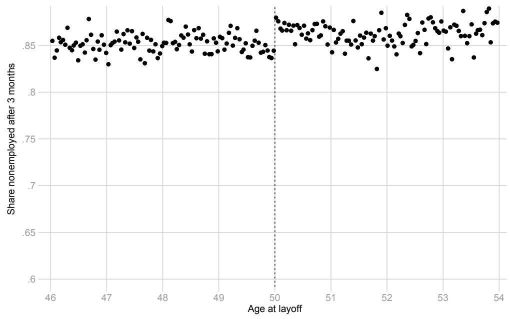
(b) average UI benefit duration in weeks



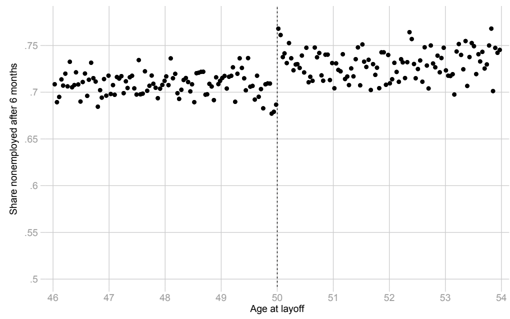
Note: The figure displays the average UI receipt in euros (panel (a)) and average UI benefit duration in weeks (panel (b)) by age-at-layoff. In both panels each dot represents a two week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover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

Figure 1.5: Nonemployment survival probabilities

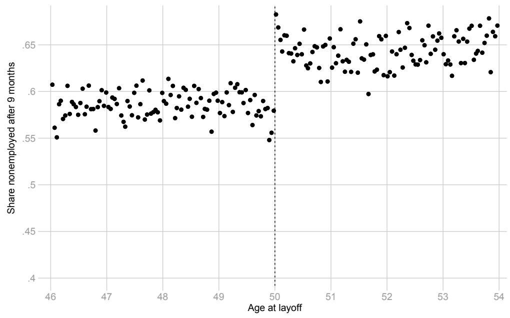
(a) Probability of still being unemployed after 3 months



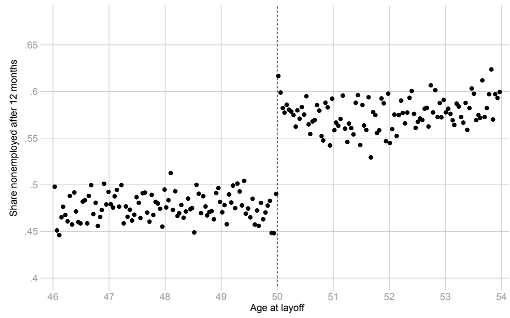
(b) Probability of still being unemployed after 6 months



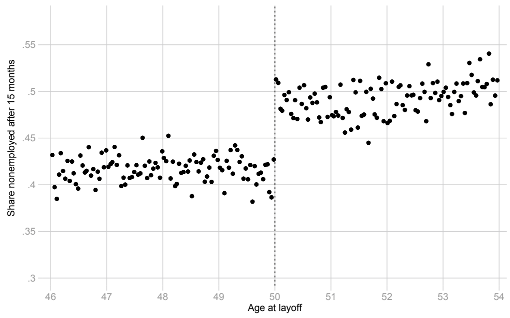
(c) Probability of still being unemployed after 9 months



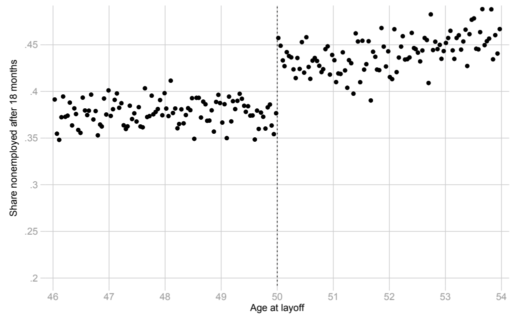
(d) Probability of still being unemployed after 12 months



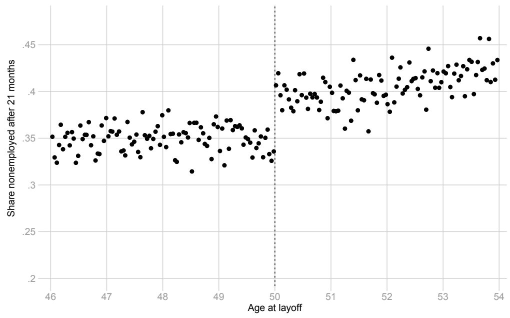
(e) Probability of still being unemployed after 15 months



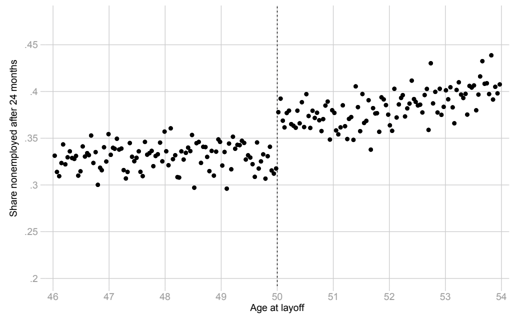
(f) Probability of still being unemployed after 18 months



(g) Probability of still being unemployed after 21 months



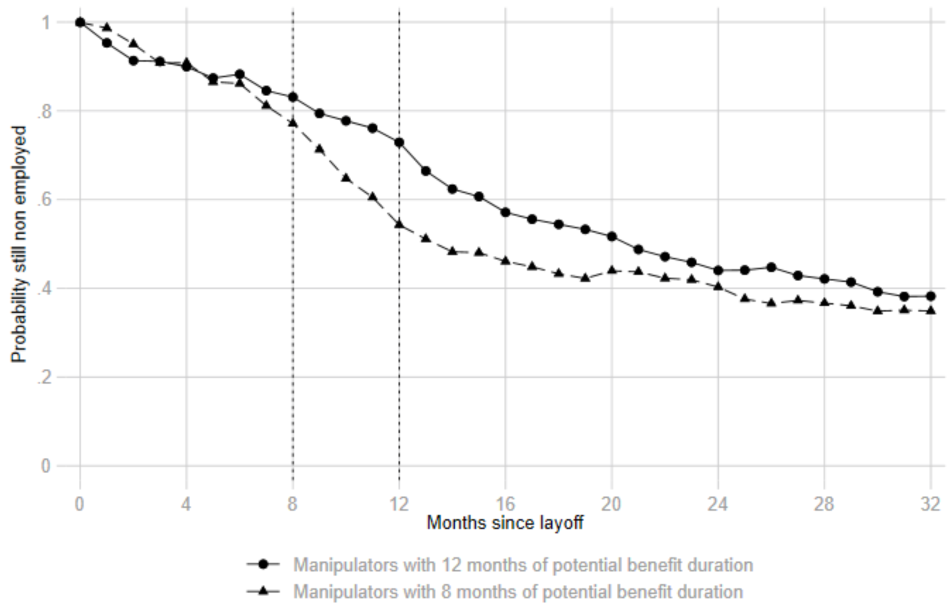
(h) Probability of still being unemployed after 24 months



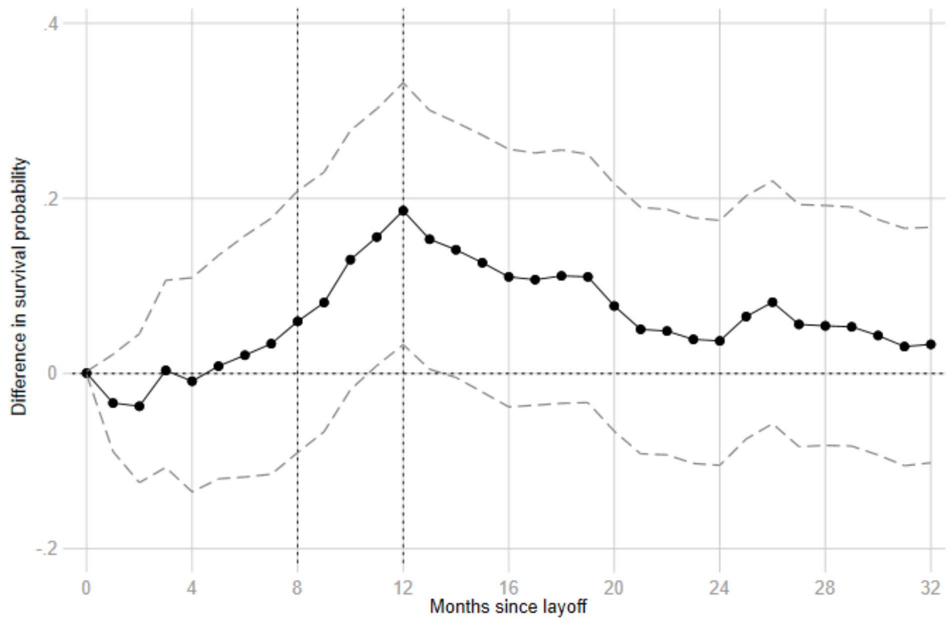
Note: The figures show the share of laid off workers, who are still unemployed after 3, 6, ..., 24 months. In all panels each dot represents a two week bin. The sample includes all individuals working on a permanent contract and claiming regular UI (OUB). The data cover the period February 2009 till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

Figure 1.6: Manipulators with 8 and 12 months of potential benefit duration

(a) Nonemployment survival rates



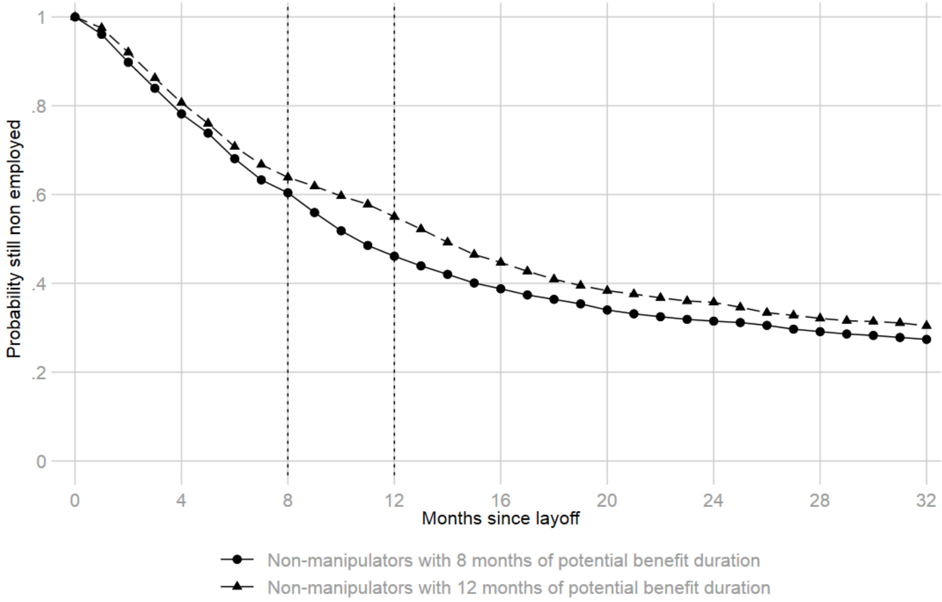
(b) Difference in survival rates



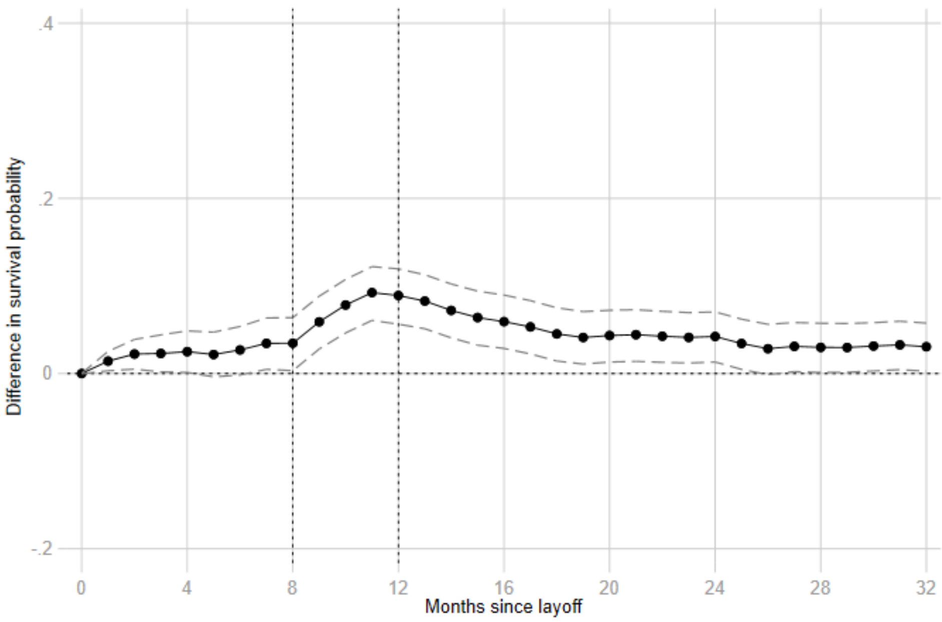
Note: Panel (a) plots point estimates of manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in section 1.4.2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 1.7: Manipulators with 8 and 12 months of potential benefit duration

(a) Nonemployment survival rates



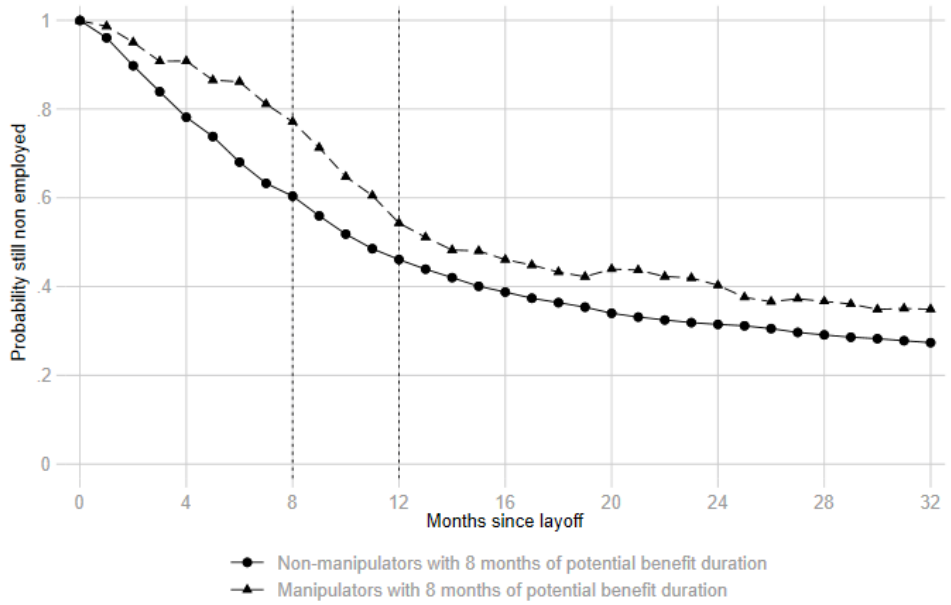
(b) Difference in survival rates



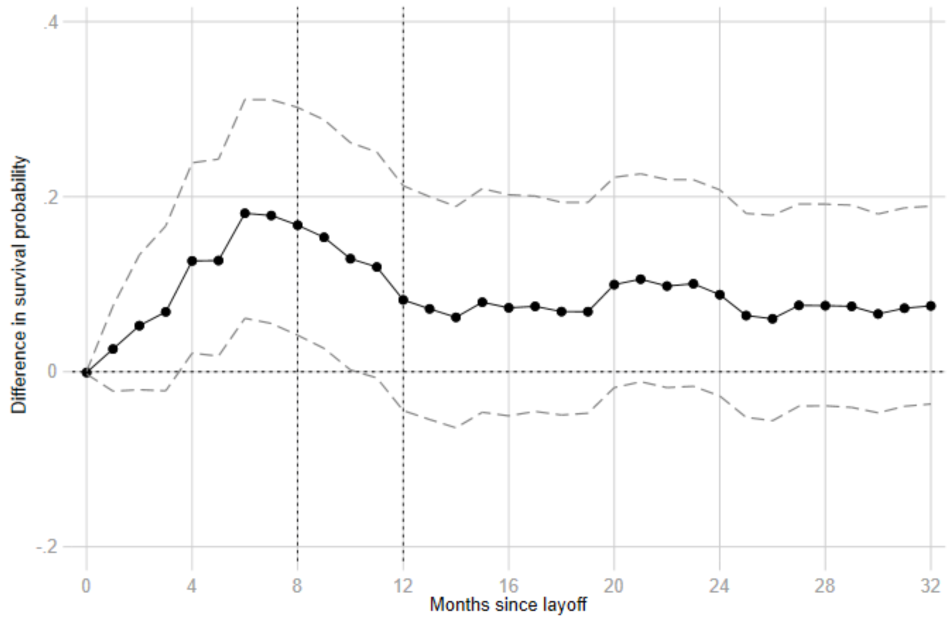
Note: Panel (a) plots point estimates of non-manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in section 1.4.2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 1.8: Manipulators and non-manipulators with 8 months of potential benefit duration

(a) Nonemployment survival rates



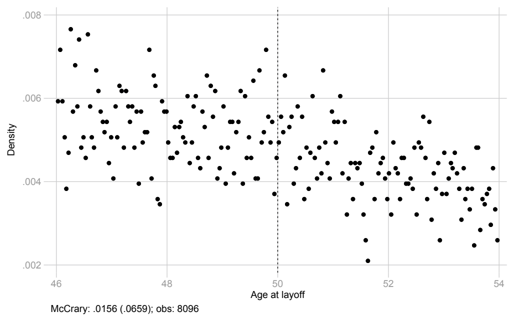
(b) Difference in survival rates



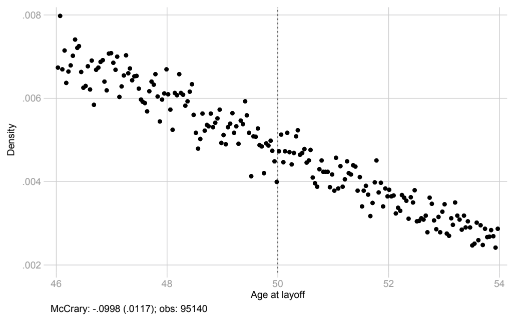
Note: Panel (a) plots point estimates of manipulators' and non-manipulators' nonemployment survival over the first 32 months after layoff under eight months of PBD. The estimation of the former is outlined in section 1.4.2. The latter represents the observed mean survival rate in the missing region. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 1.9: Density of Layoff by Private and Public sector and by Contract Type

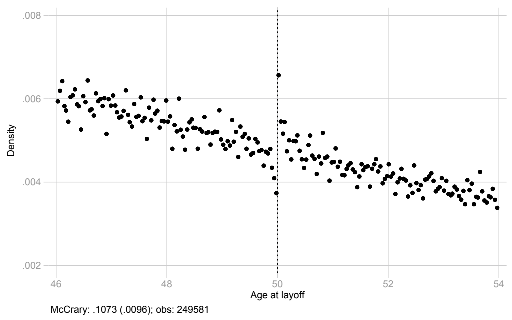
(a) Public Sector: Permanent



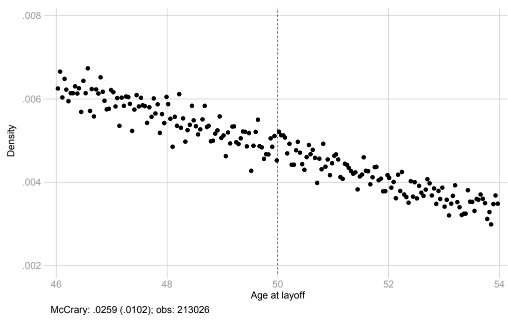
(b) Public Sector: Temporary



(c) Private Sector: Permanent



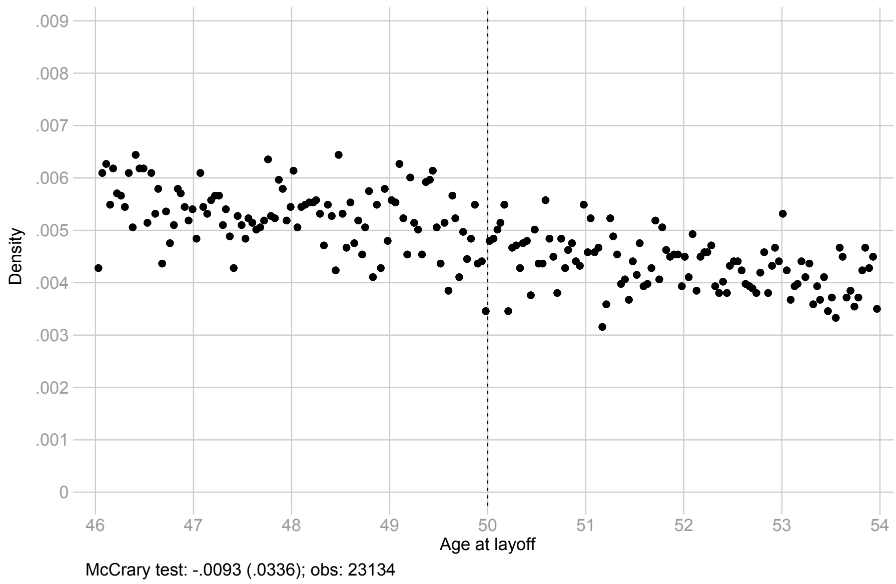
(d) Private Sector: Temporary



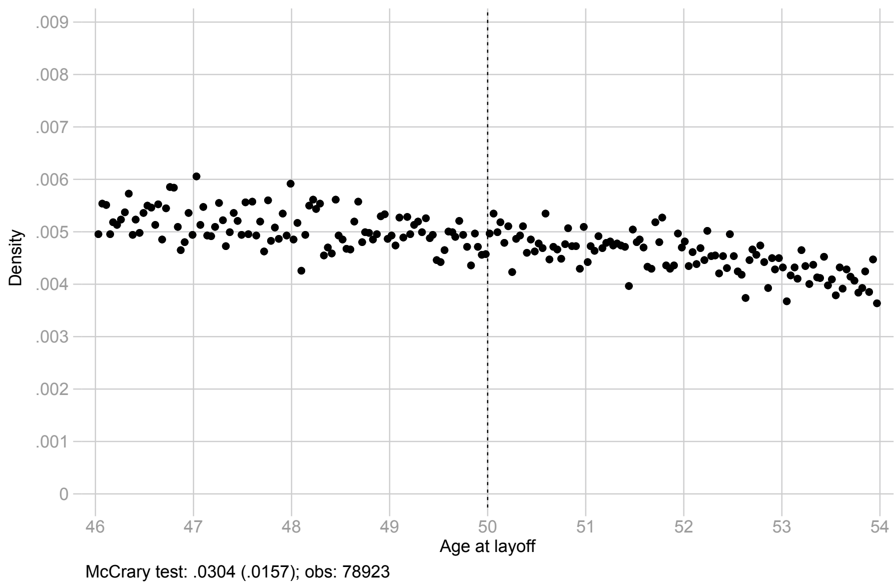
Note: The figure shows the density of layoffs by contract type. The data cover the period February 2009 till December 2012. In all panels each dot represents a two-week bin. Individuals are classified as "public sector" workers if they are present in the SIP database but a corresponding employment spell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

Figure 1.10: Placebo checks: MiniASpI and NASpI and density of recipients at 50 years of age

(a) MiniASpI



(b) NASpI



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

Tables

Table 1.1: Summary statistics

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

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

Table 1.2: Headcount and share estimates

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

Note: The table reports estimates of the total number of individuals in four groups: (1) manipulators in the missing region, (2) non-manipulators in the missing region, (3) manipulators in the excess region and (4) all other individuals in the excess region. Column (5) and (6) contain estimates for the share of manipulators in the missing and excess region, respectively. Bootstrapped 95% confidence intervals are in parentheses. We formally define all quantities in Section 1.4.1. All results are based on our main sample consisting of 249,581 observations.

Table 1.3: UI Benefit receipt estimates (Euros)

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

Note: The table reports estimates of the mean UI benefit receipt (in Euro) of individuals in four groups: (1) manipulators in the missing region, (2) non-manipulators in the missing region, (3) manipulators in the excess region and (4) all other individuals in the excess region. Column (5) and (6) contain estimates of the UI benefit receipt response of manipulators and non-manipulators, respectively. Bootstrapped 95% confidence intervals are in parenthesis. We formally define all quantities in Section 1.4.2. All results are based on our main sample consisting of 249,581 observations.

Table 1.4: Benefit duration estimates (weeks)

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

Note: The table reports estimates of the mean benefit duration (in weeks) of individuals in four groups: (1) manipulators in the missing region, (2) non-manipulators in the missing region, (3) manipulators in the excess region and (4) all other individuals in the excess region. Column (5) and (6) contain estimates of the benefit duration response of manipulators and non-manipulators, respectively. Bootstrapped 95% confidence intervals are in parenthesis. We formally define all quantities in Section 1.4.2. All results are based on our main sample consisting of 249,581 observations.

Table 1.5: BC/MC Ratios

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

Note: The table reports BC/MC ratios for manipulators (a) and non-manipulators (b). BC/MC without taxes are defined in equation 1.2 in Section 1.3.1. BC/MC with taxes are defined in equation 1.4 in the same section. Bootstrapped 95% confidence intervals in parentheses.

Table 1.6: Test for Discontinuity of observables at cutoff

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

Note: The table reports results for the robustness tests described in Section 1.6.4. The analysis based on 249,581 spells of individuals laid off from a permanent contract between 2009 and 2012. $\lambda_0^{>50}$ and $\theta_0^{>50}$ are OLS coefficients from specifications 1.27 and 1.28, respectively. Columns from (1) to (3) report RDD coefficient for dicontinuity of observables at cutoff for whole sample together with standard error and associated t-stat. Columns from (4) to (6) replicates same exercise for sample excluding manipulation region. In both cases, the specification includes a dummy equal to 1 if the worker is fired after turning 50 years of age, a squared polynomial in age in difference from the cutoff and flexible on the two sides. T-stats are bold if coefficients are significantly different from zero at the 5% level. Baseline reports average for the individuals fired between 49 and 50 years of age. Standard Errors clustered at local labour market level.

Table 1.7: Difference in observables between manipulators and other groups

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

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

Table 1.8: Testing for discontinuities in the layoff density at the threshold

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

Note: The table reports a parametric test for the discontinuity in the density of layoff at the cutoff of 50 years of age. Column (1) includes all bins. Column (2) excludes the manipulation region which encompasses the three bins before the cutoff and the two bins after the cutoff. Column (3) also excludes the manipulation region but uses an alternative definition of such region. Details about the alternative definition are provided in Section 1.6.3. Robust standard errors are reported in parentheses.

Appendices

1.A Further details about Italian UI

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

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

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

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

This measure represents a particularly attractive alternative for individuals involved in mass layoffs and could lead to underrepresentation of these types of workers in our sample. What is

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

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

more relevant for our purposes is that selection for this benefit is mostly beyond the control of the worker: indeed, the firm needed to be undergoing significant economic restructuring and have a minimum size, while workers needed to meet some tenure requirements.

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

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

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

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

Chapter 2

The impact of Chinese import competition on Italian manufacturing

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Abstract

This paper documents the effects of increased import competition from China on the Italian labor market. In line with recent studies [Autor et al., 2013, 2014], we take two complementary approaches and study both the effects on local labor markets and on incumbent manufacturing workers. Our analysis shows that the Italian local labor markets which were more exposed to Chinese trade by means of their industry composition ended up suffering larger manufacturing and overall employment losses. Nevertheless, back-of-the-envelope calculations suggest that the aggregate effect on total manufacturing employment is modest. At the individual level, contrary to what has been documented for many developed countries, more exposed incumbent manufacturing workers did not suffer long term losses in terms of lower earnings or more discontinuous careers. While they were less likely than other similar manufacturing workers to continue working at their initial employer, they were also able to carry out successful transitions towards the non-tradable sector, in areas with better job opportunities.

2.1 Introduction

China's economic growth in the last 30 years has been unprecedented. Thanks to a series of market-oriented reforms started in the late 70s, and culminated with the WTO accession in 2001, it came to be the third largest world economy and biggest manufacturing producer. In recent years, a growing literature has quantified the effect that such an economic rise has had on the labour markets of developed economies, mostly via international trade (see Autor et al. [2016] for a review). While a robust finding from this line of work is that the "China shock" has displaced manufacturing jobs and deteriorated the careers of incumbent manufacturing workers, the margins of adjustment and the workers' transitions towards other parts of the economy seem to be country specific.

In this paper we investigate the impact of increased Chinese import competition, during the 1991-2007 period, on the Italian labor market. Our analysis takes two complementary approaches. In the first part of the paper we make use of Italian Census data to look at the effects of Chinese trade from the perspective of local labor markets (LLMs). Here we follow the methodology used by Autor et al. [2013] and investigate whether areas specialized in industries subsequently hit by Chinese competition lost more manufacturing jobs in the 1990s and 2000s. In the second part of the paper we take advantage of administrative matched employer-employee data on individual working histories to examine the careers of incumbent manufacturing workers, similarly to Autor et al. [2014]. We ask whether those individuals who in 1991 were working in industries subsequently hit by Chinese competition were more likely to lose their job in the following years and, if so, whether they were able to carry out successful job transitions towards other firms.

We find that LLMs traditionally specialized in import-competing sectors see a decrease in manufacturing and overall employment. On aggregate, however, this fall is modest in size. If we compare the evolution of the share of working-age population employed in manufacturing over the period 2001-2007 of two areas respectively at the 75th and at the 25th percentile of our import competition measure, we see that the former experiences a differential decrease of about 0.6 percentage points, a 5.3% fall in relative terms. Under the assumption that relative differences across areas represent absolute changes in employment, a back-of-the-envelope calculation, first developed in Autor et al. [2013], reveals that the "China shock" would have displaced around 24,000 jobs during the 1991-2001 period and 119,000 jobs during the 2001-2007 period. While China can account for about half of the overall decline (280,000 jobs), these figures are very modest if one considers that the number of individuals employed in manufacturing stood at 5.1 million in 1991.[1]

[1] Authors' calculations based on the 1991 Istat Census.

A decomposition of the overall impact into industry-level effects, developed in Goldsmith-Pinkham et al. [2018], reveals that negative employment changes are mainly driven by the textile and clothing sectors, inclusive of footwear. At the same time, Italy remained relatively shielded from the rising import competition in consumer electronics and integrated circuits that characterized the United States over the same period [Goldsmith-Pinkham et al., 2018, Bloom et al., 2019].

Interestingly, we also find that incumbent workers employed in more exposed manufacturing industries did not face more discontinuous careers, nor earned less than other similar individuals when in work. While they were more likely to terminate their work relationship at their initial employer, they were also more likely to carry out successful transitions. Workers predominantly moved towards the non-tradable sector and, in particular, towards unskilled labor intensive industries.[2] In addition, we document that part of these job moves can be explained by increases in geographical mobility. To the best of our knowledge we are the first to find a response along this margin. These effects are almost entirely driven by workers with high wages and employed in bigger firms.

Overall our results suggests that while the rise of China has certainly implied concentrated employment losses in some local labor markets, this was not enough to cause an overall decline in manufacturing employment in Italy. As a matter of fact, even though the manufacturing share of employment has witnessed a steady decline during the last fifteen years, Italy has experienced only a limited fall in the absolute number of people working in manufacturing, compared to other developed countries (see Figure 2.1). Moreover, workers' transitions out of manufacturing were helped by sustained employment growth in the non-tradable sector, which characterized Italy during those years. While the manufacturing employment share of working-age population has decreased by 1.4 p.p. during the 1991-2007 period, the non-tradable share went up by 9.0 p.p., leading to an overall rise of the employment rate of 7.6 p.p..[3] Correspondingly, the unemployment rate has been on a declining path from the late 1990s until the onset of the Great Recession, reaching 6% in 2007.[4] All in all, the "China shock" seems to have hit in a moment of favorable labor market conditions, when it would have been relatively easy for workers to find alternative job opportunities outside of manufacturing.

Our paper contributes to the growing literature on the effects of Chinese import competition on the labor markets of developed economies. At an aggregate level, all existing studies document negative employment effects. However, some important differences emerge in terms of size.

[2] In order to classify non-tradable industries we employ the Eurostat "knowledge-intensive" definition.

[3] Authors' calculations based on IStat Census data and Italian Statistical Register of Active Enterprises.

[4] IStat [2019]

In Spain, Donoso et al. [2015] find employment effects much larger than Autor et al. [2013] found in their seminal paper on the US. They rationalize this with the presence of labor market rigidities that do not allow wages to respond to trade shocks. To the contrary Balsvik et al. [2015] find muted effects of Chinese competition in Norway, with job destruction being limited to few thousands units. For France, Malgouyres [2017] also finds smaller effects compared to the US, although bigger than in Norway. A peculiar case is represented by Germany. Dauth et al. [2014] find that while areas specialized in import-competing industries lost employment, this was more than compensated by gains in areas specialized in export-oriented industries. The latter led to a net gain of approximately 300,000 jobs that would not have otherwise arisen. For the Italian case, our results document that the China shock had only modest aggregate effects on manufacturing jobs. In Portugal Cabral et al. [2018] and Branstetter et al. [2019] find muted effects on the domestic market, but strong effects on export markets. Previous literature on the Italian case has pointed out that industries hit by import competition from low-wage countries lost employment compared to other manufacturing industries and that this is especially true in low-skill and labor intensive industries [Federico, 2014]. In our paper, we are able extend the analysis and to look at the local labor market and the individual level margins of adjustment to trade shocks.

At the individual level, the general consensus so far reached is that the “China shock" has adverse consequences on workers’ careers, mostly due to the partial inability of transferring industry-specific skills to other sectors. For the US Autor et al. [2014] find negative effects on earnings, but not on the number of years with positive earnings. While workers of all skill levels are equally likely to separate from their initial employer, low-skilled workers are the hardest hit, because they keep churning among exposed industries and find it hard to transition to services. Higher-skilled workers, instead, are able to move out of manufacturing, with no apparent earning loss. Qualitatively similar results have also been found for Germany [Dauth et al., 2018] and Denmark [Utar, 2018], where the service sector can account for the majority of the transitions towards new employers. In contrast to the previous papers, we find that displaced workers were able to complete successful job transitions, thanks to the favourable labor market conditions, because new jobs were created in industries whose skill requirements were close enough to those needed in their previous jobs. This has mitigated the otherwise negative impact of increased international competition on the time spent in employment as well as on cumulative earnings.

The paper is organized as follows: in Section 2.2 we describe our data sources. In Section 2.3 we describe how we construct our measure of import exposure and detail our IV strategy. In Sections 2.4 and 2.5 we report our analyses at the local labour market and individual level, respectively. In Section 2.6 we conclude.

2.2 Data and Measurement

For the purpose of this study we combine data from different sources. International trade data comes from UN Comtrade and Eurostat. The former contains import flows at the product level classified at the 6-digit HS level, for over 170 countries, starting from 1991. Since Italian data is not present for 1991 in Comtrade, we integrate it with data from Eurostat. We convert ECU-valued trade flows from Eurostat into dollars using the average nominal ECU/$ exchange rate for 1991. We also deflate all import values so that they are expressed in 2007 dollars at constant prices. We aggregate product-level data to the level of 4-digit ISIC rev. 3 industries, using the concordances provided by Eurostat-RAMON. Domestic production data, needed to construct import penetration measures at the 4-digit level, comes from the Unido-INDSTAT4 database. In the remainder of the paper the term “industry" refers to 4-digit classifications and the term “sector" to 2-digit classifications.

Chinas’s share of world exports in goods soared from 2% in 1990 to about 15% in 2015. As for Italy, real imports from China have also been rising during the whole period. In 1991 Italy imported goods from China for a total value $3.1 billion. The same figure was around $28.1 billion in 2007, a 800% real increase. Over the same period, overall imports grew by a factor of 170%. An important feature of this exceptional growth is the high degree of variation across sectors. Table 2.1 reports 1991-2007 changes in the import penetration ratio and employment shares in total manufacturing employment for 22 2-digit sectors. The greatest incraese in import penetration occurred in sectors linked to textile and furniture, while industries that experienced the lowest increases are in the food and beverage sectors. The three most exposed sectors constituted 19.1% of the total manufacturing employment in 1991, indicating that Italy was relatively specialized in those sectors subsequently hit by Chinese competition.[5] In 2007, the same three 2-digit sectors accounted for 15.8% of total manufacturing employment, which approximately corresponds to a 1/5 decrease.

In the regional analysis our unit of interest is the local labor market (LLM). We obtain information on LLMs from the National Institute of Statistics (Istat). LLMs are groups of municipalities with strong commuting ties, and are similar to commuting zones in the US.[6] In 1991, Istat grouped Italy’s 8,101 municipalities in 784 local labor markets. For each LLM we collect employment data by industry in 1981, 1991 and 2001 from the manufacturing census and in 2007 from the Italian Statistical Register of Active Enterprises (ASIA). In order to match industry employment data to international trade data, we convert all employment-related

[5] If there was no correlation between import exposure and initial specialization we would expect that the first three sectors occupy $(100/22) \times 3 \times 100 = 13.6\%$ of total manufacturing employment.

[6] For more details about the methodology, see ISTAT. [1997] and Coppola and Mazzotta [2005]

variables from the original NACE classification to the ISIC Rev. 3 classification up to the level of 4 digits. In order to construct demographic and socio-economic control variables at the LLM level in 1991 and 2001, we draw information from the Population Census at the municipality level. We report descriptive statistics in Table 2.2, panel (a). Similarly to other developed economies, manufacturing employment as a share of working age population has been declining in the last two decades. However, a strong growth in the non-tradable sector has lead the overall employment rate to rise markedly, more than in other OECD countries.

In the worker-level analysis, our units of interest are the incumbent employees of manufacturing firms in 1991. We draw information on their career before and after 1991, and up to 2007 from the Italian Social Security Institute (INPS). We rely on a matched employer-employee dataset covering the universe of workers from the population of privately employed individuals in Italy. Public sector, farming and self-employment are not present in the dataset. For each job spell in every year we observe worker and firm identifiers, together with gross earnings, number of weeks worked in full time equivalent units, part-time status and a coarse occupational code (apprentice, blue collar, high-skilled blue collar, white collar, middle manager or manager). For each worker we also observe a series of basic demographic characteristics such as gender, year of birth and place of birth. As for their firms, we observe 4-digit industries and municipality for each establishment.[7] We select a sample of approximately 700,000 workers born between 1952 and 1970, who were between 21 and 55 years old during the 1992-2007 period. We exclude individuals born in earlier cohorts because industry specific retirement patterns may act as a confounder. We restrict our attention to workers with high labor market attachment, who had a year-round job in the manufacturing sector in 1991, but were also employed the whole time in the three years before. In Table 2.2, panel (b), we display descriptive statistics. Out of the 192 months between 1991 and 2007, the average worker spent 157 months in employment, cumulatively earned 15 times her initial average annual salary, displaying a wage growth of 14% of her initial average annual salary for every 12 months spent in employment. One-third of our sample is made of females, while 70% is made of blue collar workers. Only 2% of these individuals were born abroad. In the years from 1988 to 1991, the average worker was earning a mean salary of $\exp\{10.6\} \approx 23,000$ euros and experienced a wage growth of around 9%.

[7] Our definition of an establishment is based on the *matricola contributiva* in the INPS dataset, that is the level at which firms pay social security contributions. For a given firm a *matricola* includes a set workers whose activities can be attributed to a unique 4-digit industry, and the set has organizational and managerial autonomy.

2.3 Empirical strategy

Our empirical strategy closely follows recent work by Autor et al. [2016]. We exploit variation in the growth of Italian imports from China across narrowly defined manufacturing industries. For each industry j our measure of the increase in exposure to Chinese competition is the change in the import penetration ratio:

$$\Delta IP_{jt}^{ITA} = \frac{\Delta M_{jt}^{ITA}}{Y_{j,91} + M_{j,91} - X_{j,91}}, \tag{2.1}$$

where ΔM_{jt}^{ITA} is the real change in Italian imports from China in industry j between period t and $t-1$; $Y_{j,91}$ is domestic production in 1991; $M_{j,91}$ is total imports in 1991 and $X_{j,91}$ is total exports in 1991. Import penetration captures the fraction of Italian domestic consumption (for goods produced in j) accounted for by Chinese producers. It can also be seen as the market share in sales that China occupies in the Italian market.

We use this measure in two different ways. In Section 2.4 we apportion industry-level changes as in 2.1 to LLMs, depending on their initial employment shares in such industries. Our aim there is to investigate how local exposure to import competition translates into declines of manufacturing and overall employment at the local level. In Section 2.5, instead, we attribute industry-level changes directly to individual workers, depending on their industry of affiliation in 1991. There we are interested in studying the adverse consequences of international trade on job biographies and explore the margins of adjustment that workers have to recover from an increase in trade exposure.

One could be concerned that the measure in 2.1 is correlated with unobserved industry shocks in Italy, which also explain employment dynamics. This would prevent identification by means of simple OLS.[8] In order to obviate to this issue we employ an instrumental variable strategy aimed at isolating changes in Chinese trade that are due to productivity improvements in China, rather than domestic industry shocks. Consistently with the recent literature [Acemoglu et al., 2016, Autor et al., 2016, 2013, 2014] we instrument 2.1 with an analogous measure that replaces changes in Chinese exports to Italy with changes in Chinese exports to other developed countries (OC). This is equal to:

[8] Say that technological improvements in a given industry allows Italian firms to sell more goods at lower prices. This could independently affect both Italian firms' labor demand and consumer demand for Chinese goods, biasing the OLS coefficient. The sign of the bias would depend on what exactly happens to labor demand (which could increase or decrease following the technological improvement) and to consumer demand for Chinese goods (which could decrease or increase depending on whether the goods are substitute or complements).

$$\Delta IP_{jt}^{OC} = \frac{\Delta M_{jt}^{OC}}{Y_{j,91} + M_{j,91} - X_{j,91}} \tag{2.2}$$

The intuition behind the relevance of this instrument is that a series of structural reforms in China have increased its productive capacity in a specific set of industries where the economy had a comparative advantage. As a consequence China started exporting more in these industries across a wide variety of destinations. In order for this instrument to be valid, it must be that common patterns in Chinese trade across developed economies do not reflect correlated demand or technology shocks across high income countries. Although we cannot rule out this completely we choose our set of high-income countries so that this risk is minimized. We select all countries used in Autor et al. [2013], with the inclusion of the US, but exclude European countries, where Italian exports and trade flows are concentrated. Our countries include therefore: The United States, Australia, Canada, Japan and New Zealand. Import flows that are common between Italy and this set of countries is more likely to capture the common Chinese supply-side component rather than a correlated demand component.

2.4 Local labor market evidence

Our aim in this section is to understand the relationship between changes in import competition from China and changes in manufacturing employment, which we measure as the share of working age population employed in manufacturing, at the local labor market level. Our empirical strategy, first developed in Autor et al. [2013], uses a Bartik-type measure where nation-wide industry changes in import penetration are apportioned to LLMs via initial local employment shares in those industries. The design exploits variation in the initial specialization of LLMs to generate variation in exposure to Chinese competition. Our measure of exposure is:

$$\Delta IP_{it}^{ITA} = \sum_j \frac{L_{ij,1991}}{L_{i,1991}} \Delta IP_{jt}^{ITA}, \tag{2.3}$$

where ΔIP_{jt}^{ITA} is the change in import penetration between period t and $t-1$ for industry j. $L_{ij,1991}$ is employment in industry j in LLM i in 1991, while $L_{i,1991}$ is total private non-agricultural employment in LLM i in 1991. The cross-sectional variation in ΔIP_{it}^{ITA} comes from two sources: (a) differences in the initial manufacturing share of employment[9] and, (b) differences in the industry mix within manufacturing. In our preferred specification we always

[9]Imports from China consist almost exclusively of manufacturing goods. Given this fact, consider a situation where ΔIP_{jt} is constant and equal to k for every industry j in the manufacturing sector. Then $\Delta IP_{it} = k \cdot L_{i,1991}^{m}/L_{i,1991}$, where $L_{i,1991}^{m}$ is total manufacturing employment. It follows that the shock is higher by contruction in those LLMs with higher employment share in manufacturing in 1991.

control for the share of manufacturing employment in 1991, so that the cross-sectional variation only comes from differences in industrial composition across areas with similar manufacturing intensity. By means of their initial specialization, some LLMs experienced marked increases in import penetrations while others remained relatively shielded from it. Two LLMs at the 25th and 75th percentile of import exposure, experienced a differential change in import penetration from China of 0.64 percentage points during the 1991-2001 period, and of 2.7 percentage points during the 2001-2007 period.

In Figure 2.2 we present heatmaps of both changes in the share of working-age population employed in manufacturing and changes in the import penetration ratio, for the 2001-2007 period. Both changes are first residualized against the start-of-period share of manufacturing employment. The hardest-hit areas are concentrated in the North-East (Veneto) and Center (Tuscany and Marche). In the North-West (Piemonte) and vast part of the South (Campania, Molise, Basilicata), competition was lower. We now turn to our estimating equation:

$$\Delta Y_{it} = \alpha_r + \gamma_t + \beta \Delta IP_{it}^{ITA} + X'_{i,'91}\delta + \epsilon_{it}, \tag{2.4}$$

where our main outcome of interest is the change in the share of working-age individuals who work in manufacturing; α_r are 20 "NUTS 2" region fixed effects; $X'_{i,'91}$ is a vector of LLM-level controls measured in 1991, namely the female employment rate and the share of manufacturing employment in private non-farm employment; ϵ_{it} is an error term.[10] We estimate Equation 2.4 in long differences, stacking the two periods 1991-2001 and 2001-2007. We normalize variables to decade-equivalent changes[11], and include a decade dummy (γ_t). All regressions are weighted by initial LLM share of working age population. We cluster standard errors at the LLM level to account for serially correlated shocks over time within areas. The differenced specifications net out unobservable time-invariant characteristics at the LLM level, which explain the level of manufacturing employment. Our specification in long differences measures long-run changes and should not be affected by year-to-year volatility in manufacturing employment or trade flows.

As described in Section 2.3, one possible concern when estimating Equation 2.4 by OLS, is that ΔIP_{it}^{ITA} could be correlated with the error term because of domestic industry-specific shocks. In order to obviate to these problems we instrument our measure in 2.3 with:

[10]Contrary to Autor et al. [2013], we do not have good measures of education and the incidence of routine occupations at the local level. These controls are aimed at capturing changes in technology that may be correlated with import exposure and explain the evolution of manufacturing employment. To obviate to this lack of measurement we try to control for these factors indirectly, by using (twenty) region fixed effects, under the assumption that these characteristics do not vary extensively across local labor markets in the same region.

[11]This involves multiplying both the dependent variable and ΔIP by 10/6 in the second period (2001-2007).

$$\Delta IP_{it}^{OC} = \sum_j \frac{L_{ij,1991}}{L_{i,1991}} \Delta IP_{jt}^{OC}, \tag{2.5}$$

that is an analogous measure that replaces changes in Chinese exports to Italy with changes in Chinese exports to a subset of other developed countries (OC). In the next section we present the results from our analysis.

2.4.1 Chinese trade and manufacturing employment

Table 2.3 presents the main results of the local labor market analysis. In Panel (a) we report 2SLS estimates of the effect of Chinese import competition on the manufacturing share. Corresponding first-stage estimates and K-P F-statistics are displayed in Panel (b).[12] In all specifications we detect a negative and strongly significant effect of increases in import competition on the manufacturing share. The coefficient associated with the ΔIP_{it}^{ITA} variable in column (1) of panel (a) indicates that, over a decade, a percentage-point increase in import penetration from China is associated with a 0.253 percentage points decline in the share of working age individuals working in manufacturing.[13] In column (2) we introduce 20 regional dummies, meant to capture unobserved differential trends in employment dynamics. During this period, the manufacturing share in working age population was growing more in the South of Italy compared to the North, mostly because of increases in labor force participation, traditionally low in the South. The introduction of geographic dummies partially attenuates the size of our effect of interest, which still remains strong and significant. Compared to specification in column (2), column (3) further adds to the analysis demographic and economic controls measured in 1991, which may independently affect the manufacturing share at the LLM level. Both the share of manufacturing employment and the female employment share are strong predictors of the decline in manufacturing. However the coefficient on our variable of interest decreases only by 1/4 compared to column (2) and remains highly significant. Finally, in column (4) we estimate our model with the full set of controls but without weighting for working age population in the LLM at the beginning of the period. The main results are unaffected, suggesting the results are not driven by a few and very large LLMs. First stage estimates suggest a very strong and statistically significant relationship between our endogenous variable and the instrument. First stage estimates are very stable across specifications.

Column (3) is our preferred specification. Our coefficient of interest indicates that, over a decade, a percentage point increase in the share of domestic spending that falls on Chinese goods

[12] Table 2.A.1 in the Appendix reports OLS estimates of the same specifications.

[13] The level of the share in 1991 was 11.66%, so this implies a 1.7% change.

lowers the share of working age individuals employed in manufacturing by 0.146 percentage points. Under the assumption that differences across LLMs mainly reflect absolute changes in the number of jobs, we can use a simple back-of-the-envelope calculation to assess the relative contribution of China in explaining changes in manufacturing employment [Autor et al., 2013].[14] Since the average local labor market saw a real increase in Chinese import penetration of 0.7 percentage points between 1991 and 2001, and of 3.5 percentage points in the six years between 2001 and 2007, we obtain that Chinese import competition has reduced the manufacturing share in working age population by 0.1 (0.146×0.7) percentage points in the first period and 0.51 (0.146×3.5) percentage points in the second period. Since the overall change in such share has been -0.55 percentage points in the first period, and -0.89 percentage points in the second period, we obtain that China can account for 18% (0.1 over 0.55) of such decrease in the first period, and 58% (0.51 over 0.89) in the second period.

As highlighted in Autor et al. [2013], this benchmarking exercise may overstate the share of the decline that is attributable to China. While $\hat{\beta}_{2SLS}$ reflects the causal effect of an increase in China's productive capacity on Italian manufacturing, ΔIP_{it}^{ITA} reflects both supply and demand changes. Insofar increases in import demand by Italian consumers have less negative effects on employment, our calculation would overstate China's contribution to the decline in Italian manufacturing. Same as in their paper, we rescale the effects multiplying them by the share of variance in ΔIP_{it}^{ITA} accounted for by ΔIP_{it}^{OC}.[15] We find this share to be 61% in our sample. This implies that China can account for 11% of the Italian manufacturing decline in the 1991-2001 period and for 35% of the decline in the 2001-2007 period. Multiplying these shares by 1991 working age population would imply a loss of around 23,700 jobs in the first period and a loss of 119,400 jobs in the second period. In Table 2.A.3 we compare these numbers to those constructed for other OECD countries in similar studies. In Italy, France, Germany and Norway, the number of jobs lost represents between 1% and 4% of 1995 manufacturing employment, reflecting a striking similarity in the magnitude of the response. In Spain and the United States the picture looks much different, with declines of almost 14% and 9% respectively.

To check the robustness of our results we perform a series of falsification tests, where we regress 1981-1991 (past) changes in manufacturing employment against 1991-2001 and 2007-2001 (future) changes in import penetration, properly instrumented. This amounts to check whether areas subsequently hit by Chinese competition were already trending differently in the decade before. In Table 2.4 we show the results. While in some instances the absolute value of point estimates is greater than that of our main effects, we fail to find any statistically significant relationship

[14] Migration across areas constitutes one potential threat to the validity of this exercise. In Section 2.4.2 we show that population counts do not respond to the China shock.

[15] The details of this calculation are presented in the Theory Appendix of Autor et al. [2013]

between past employment dynamics and Chinese trade. Areas later hit by Chinese competition were not on a significantly different trend beforehand.

2.4.2 Other labour market outcomes at the local level

Following a shock to labor demand in manufacturing, incumbent workers losing their job may choose to reallocate to the non-manufacturing sector, to move to other local labor markets or to abandon the labor force altogether.

The indirect effects of trade with China on employment in other sectors may be ambiguous in sign. On the one hand incumbent workers exiting manufacturing may turn to the non-tradable sector looking for a job. Similarly, new entrants may face fewer vacancies in manufacturing and search for a job elsewhere. This reallocation channel predicts that bigger decreases in the share of manufacturing employment should cause an increase in the share of non-manufacturing employment, with no net effect on total employment. On the other hand if workers are not able to obtain other jobs in the non-tradable sector, they may decide to leave the labor force (depressing total employment) or migrate to other local labor markets, inducing changes in population. This may happen both because industry specific human capital prevents transitions across sectors, or because the negative demand shock induced by China may dampen the local demand for non-tradables, reducing labor demand.

We use slight modifications of the estimating equation in 2.4 to shed light on these different adjustment mechanisms. In Table 2.5 we study three different outcomes: the number of people employed in the non-tradable sector over working age (15-64) population, the total number of people working over working age population and, finally the log change in working age population. Results in Table 2.5 suggest that in those LLMs that were more exposed to Chinese trade, the decline in manufacturing employent (column 1) was not compensated by an increase in employment in the non-tradable sectors (column 2). Given that working age population did not change in response to increased competition (column 4) total employment in those LLMs fell (column 3).

2.4.3 Why are effects small?

Compared to results found for the United States, our point estimates, combined with aggregate measures of the shock, indicate at most modest effects of Chinese import competition on Italian aggregate employment. Under the assumption that cross-sectional differences reflect absolute changes, China would have caused Italian manufacturing employment to decline by 3% over the

1995-2007 period. The same change, as implied by estimates in Autor et al. [2013] is 8.9% in the United States (Table 2.A.3). In this subsection we try to rationalize this finding and provide some suggestive evidence that may explain the difference.

The first consideration to be made is that the industrial composition of Italy and the United States looked very different already in the mid 1990s. The United States had higher employment shares in high-tech sectors linked to computing and ICT, while Italy was specialized in lower-tech sectors linked to textile and clothing (T&C), together with leather goods. In 1995, Electrical machinery and optical equipment accounted for 14.4% of manufacturing employment in the US, while the same number was only 6.8% in Italy. Conversely, in 1995 20% of Italian manufacturing employment was accounted for by T&C and leather goods, while the same share was around half of that in the United States (9.1%).[16]

The common view is that China exports low-tech goods that are intensive in the use of labor. Given these specialization patterns this would have implied bigger employment losses in Italy, compared to the United States. However, starting from the early 2000s, the structure of Chinese exports changed in favour of consumer electronics and other relatively high-tech goods, in a way that was not expected for a country with that level of development [Rodrik, 2006, Schott, 2008].[17] The relative convenience of Chinese goods in these sectors has likely put competitive pressure on US producers. While such higher-tech goods gained prevalence, it is still true that China was exporting high quantities of T&C goods. However, empirical evidence using European data shows that import competition in T&C has led to technology upgrading within, and reallocation of workers towards, the best firms in the sector [Bloom et al., 2016]. One might argue that such reallocation within T&C may have limited aggregate employment losses in manufacturing. In addition to this, Italian varieties in T&C may have suffered less from Chinese competition as they were already part of a higher-quality and relatively insulated market niche [Truett and Truett, 2014].

In what follows we use techniques developed by Goldsmith-Pinkham et al. [2018] to analyze whether the local labor market effects in the two countries are indeed driven by different industries. The authors show that the 2SLS estimator based on a Bartik instrument (like ours) can be expressed as a weighted average of industry-specific marginal effects, where the weights depend on the relative strength of industry-specific first stages.[18] In our setting, these

[16] We retrieve aggregate data for the US from the County Business Pattern files for 1995, freely available at `https://www.census.gov/data/datasets/1995/econ/cbp/1995-cpb.html`. For T&C (including leather) we consider 2-digit SIC codes 22, 23, 31. For Electrical machinery and optical equipment, we consider 3-digit SIC code 357 and 2-digit codes 36, 38.

[17] One emblematic case in this respect is Lenovo's acquisition of the IBM PC division in december 2004.

[18] These weights are referred to as Rotemberg weights [Rotemberg, 1983]. Although the weights always sum to one, negative weights are possible. This happens when the first stage coefficient associated to one industry and the overall one are opposite in sign. In our sample, as in Autor et al. [2013], negative

industry-specific weights depend on the (relative) strength with which Italian imports from China in an industry can be explained by the Chinese supply shock, as captured by Chinese exports to other countries.

In order to perform this exercise for the United States we make use of data from the replication packages of Autor et al. [2013] and Acemoglu et al. [2016].[19] Results are reported in Table 2.6. In Panel (a) we report the top five industries in terms of industry-specific weights (α_k) for the United States, together with the associated marginal effects (β_k). Electronic computers and semiconductors strongly contribute to the overall decline. The importance of such industries is also consistent with recent evidence from Bloom et al. [2019], who find that most of China-related employment changes in the US are driven by large multinationals in high-tech sectors switching from manufacturing activities (probably offshored) to service activities. We also find negative effects in furniture and toys, consistent with fast and marked increases in import penetration.[20] Perhaps surprisingly, communication equipment (radio and TV) did not witness employment changes, despite strong import competition. When turning to Panel (b), we find a very different set of industries driving effects in Italy. We find that import changes in the textile and clothing (T&C) sector are associated with employment declines and none of the high-tech sectors rank among the top five. The industry that carries the highest weight is the cutting and shaping of stone. While in this industry Chinese imports rose substantially, this did not cause a fall in employment. This is likely due to strong foreign demand of certain Italian stone varieties (e.g. marble sold to China) that prevented labor demand from falling.[21] These results confirm the effects are driven by different industries in the two countries, consistent with the evidence from the literature presented in this subsection.

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weights are quantitatively unimportant.

[19] In order to harmonize the import competition measure across the two settings, we substitute the original import per worker measure employed in Autor et al. [2013] with an import penetration one, built thanks to data from Acemoglu et al. [2016]. Acemoglu et al. [2016] uses two time windows, 1991-1999 and 1999-2007 that are slightly different from Autor et al. [2013] and ours. We therefore appropriately rescale these 8-year long differences so that they reflect decade-equivalent changes. Industry employment shares are always fixed at 1988.

[20] Reporters from the *Wall Street Journal* have also been arguing that the rise in import competition from China can account for consistent employment declines in the furniture industry [Davis and Hilsenrath, 2016]

[21] The inclusion of the stone-cutting industry is not the only factor responsible for the difference in effects. When repeating the analysis removing such industry, we find a $\hat{\beta}_{2SLS} = -0.315$. The ensuing back-of-envelope calculation of Section 2.4.1 yields an overall loss of 255,000 manufacturing jobs, amounting to 5.5% of 1995 manufacturing employment, which is still lower than the effect found by Autor et al. [2013] for the US.

2.5 Worker level evidence

Although Chinese import competition has a negative impact on the share of population that works in manufacturing, individual careers of incumbent workers need not to be negatively affected. Worker-level effects may be muted if individuals are able to absorb the initial trade shock by transitioning to different firms, sectors or even local labor markets. Focusing on workers allows us to study such individual margins of adjustment and assess their magnitude.

In this section we thus take a complementary approach to the previous one and analyze the career developments of individuals initially employed in industries which saw increases in Chinese competition over the 1992-2007 period. We take a long-run view and look at cumulative outcomes related to the time spent employed and earnings, as in Autor et al. [2014]. Similarly to them, after assessing the overall impact of Chinese trade on careers we decompose outcomes according to where they are accrued: initial employer, other employers, initial 2-digit manufacturing sector, other 2-digit manufacturing sectors, the non-tradable sector, initial local labor market or other local labor markets. We compare individuals who are observationally similar in 1991, except for their narrow industry affiliation. In doing so, we control not only for observable individual characteristics, but also characteristics of the firm and sector where these workers were employed at the time. For identification we use variation within broad manufacturing sub-sectors and within local labor markets.

We attribute 1991-2007 changes in import penetration to each worker based on the 4-digit industry of their employer in 1991. When a worker has more than one job in 1991, we consider the spell where the worker earns the highest share of income for that year. As highlighted in Section 2.3, we instrument changes in the Chinese import penetration in Italy with changes in Chinese import penetration for a selected set of high income countries. We attribute the value of the instrument to each worker based on their industry affiliation in 1988, instead of 1991, to exclude that our effects can be explained by job transitions in anticipation of Chinese trade.

Our empirical specification is very similar in spirit to Autor et al. [2014]. Our preferred specification takes the form:

$$Y_{ij} = \alpha + \beta_1 \Delta IP_{jt} + \beta_2 IP_{j,91} + X'_{ij}\gamma + X'_j\delta + \theta_k + \eta_s + \epsilon_{ij}, \tag{2.6}$$

where Y_{ij} is the outcome of interest for worker i employed in 1991 in industry$j$, ΔIP is the 1991-2007 change in import penetration, $IP_{j,91}$ is the level of import penetration for that same industry in 1991. X'_{ij} is a vector of individual characteristics, all measured at the beginning of the period. This includes a dummy for being female, year of birth dummies, a dummy for being foreign-born, dummies for the age of entry into the labour market, the log of average annual earnings and log change in earnings between 1988 and 1991, a dummy for being a part-time worker, and six dummies related coarse occupational codes.[22] We also include firm level controls measured at the main job the worker holds in 1991: the dimensional class of the firm and the log of the average wage in the firm. X'_j is a vector of 4-digit industry characteristics. We include the share of white collars workers in 1991, the change in the industry employment share between 1983 and 1991, and the log change in the industry average wage between 1983 and 1991. We also use dummies for 14, broadly defined, manufacturing sub-sectors (θ_k) and local labor market fixed effects (η_s). We cluster standard errors at the level of 1991 4-digit industry, to account for the fact that the long-run outcomes are correlated for individuals initially employed at the same firm, or in the same industry.

2.5.1 Import competition and individual careers

In Table 2.7 we present 2SLS estimates of equation 2.6 for different labor market outcomes at the individual level. Regardless of the measure used, we fail to detect any economically significant impact of Chinese import competition on individual careers. This stands in contrast with previous work, which has systematically detected losses for the average exposed worker [Autor et al., 2014, Utar, 2018, Dauth et al., 2018] Column (1) reports the estimated effect of changes in Chinese import penetration on the cumulative number of months with at least one day of employment. The coefficient is not significantly different from zero, and 95% confidence intervals exclude any economically meaningful effects. The point estimate of 0.013 indicates that a 10 percentage-points increase in import penetration is associated with a 4-days ($0.013 \times 10 \times 365/12 = 3.95$) increase in the time spent in employment over a 16-year period.[23] While this indicates a null effect of Chinese trade along the *extensive* margin of employment, it is not conclusive about the *intensive* margin. After a trade shock, workers could remain employed but see their number of working weeks or hours reduced. In columns (2) and (3) we investigate this channel by looking at the cumulative number of weeks and the number of full-time-equivalent (FTE) weeks worked. Any difference in the effects on these two variables should reflect a change in working hours. We find no negative effect along these margins. If anything, we see a slight increase in the number of weeks worked, although the impact is very small in size. A 10 percentage-point increase in import penetration is at most associated with a

[22]These are apprentice, blue collar, high-skilled blue collar, white collar, middle manager, manager.

[23]A 10 p.p. increase in import penetration is approximately the difference faced two workers employed in industries at the 25th percentile and the 75th percentile of import exposure, respectively (that is 10.7 p.p.)

5 days ($0.088 \times 10 \times 6 = 5.3$) increase in time spent in employment, over a period of 16 years.[24]

In the next two columns we look at earnings-related measures. In column (4) we study cumulative earnings normalized by average 1988-1991 yearly earnings, while in column (5) we look at cumulative earnings per 12 months worked, always normalized by average initial earnings (a proxy for wages).[25] More exposed workers did not face any appreciable income loss compared to observationally similar, but less exposed, individuals. As a consequence they did not face lower wages conditional on working.[26]

The fact that the overall impact is not distinguishable from zero does not imply that more exposed workers did not experience any change in their career. It could be that workers experienced a negative shock at their initial employer but were able to adjust by finding job opportunities at new firms, potentially in other sectors and other localities. In Table 2.8 we unpack the total effects analyzed in Table 2.7 into a component observed at the initial employer and a (complementary) component observed at other employers. For ease of exposition we only report effects on the number of months worked, cumulative earnings and earnings per effective year worked. In panel (a) we find that more exposed workers spend less time at their initial employer (column (2)) but that such loss is entirely compensated by transitions towards other firms (column (3)). This is reflected in cumulative earnings changes at the initial employer vs other employers (panel (b)). Conditional on moving towards other firms, workers obtain slightly higher earnings, compared to observationally similar workers who also move. The coefficient in panel (c), column (3) indicates that a 10 p.p. increase in import penetration leads to an earning growth 0.3% of average 1988-1991 yearly earnings every 12 months worked.

2.5.2 Where do workers find new job opportunities?

We have established that, on average, more exposed workers did not lose in terms of time spent in employment or earnings, because of trade. Losses at the initial employer are compensated by transitions towards other firms. In this subsection we investigate where these gains are accrued. We look separately at sectoral mobility and geographical mobility. Similarly to Section 2.5.1, in Table 2.9 decompose outcomes observed at new employers into a component that is accrued within the initial sector and other ones accrued outside. Our estimates indicate that new job opportunities are to be found in the non-tradable sector. More exposed workers spend less time

[24]Results are robust to the set of control variables included (see Table 2.A.2 in the Appendix).

[25]Compared to a specification with log earnings on the l.h.s. and individual fixed effects, such normalization only uses of information on workers' careers that is unaffected by the subsequent rise of Chinese trade [Autor et al., 2014].

[26]The coefficient in column (4) implies that a 10 p.p. increase in import penetration causes a cumulative earnings difference of 3% of average yearly earnings in 1988-1991. Given that the average (gross) salary is around 23,300 euros, the coefficient implies a gain of 700 euros over 16 years

working in their initial 2-digit sector and equally in other 2-digit sectors within manufacturing. Results in panel (c) indicate modest earning growth (compared to the counterfactual) due to transition towards the non-tradable sector.

The importance of the non-tradable sector sector in smoothing out trade shocks in manufacturing is not new in the literature. However previous studies document either that these transitions do not allow workers to fully counteract their initial shock, or that only a subset of them, the high-skilled, is able change sector in a successful way [Autor et al., 2014, Utar, 2018, Dauth et al., 2018, Dix-Carneiro and Kovak, 2019]. We offer two sets of possible explanations for why transitions to the non-tradable sector have been particularly successful for Italian manufacturing workers. The first is that employment growth in non-tradables was strong, when compared to other developed economies. For example, between 1991 and 2007, its employment share went from 57% to 66% (+15.7%) in Italy and from 72% to 77% (+6.9%)in the US.[27] [ILO, 2019]. Therefore, the sector as a whole could provide a high number of vacancies for workers leaving manufacturing jobs. The second is that the skill content of the average job in non-tradables in Italy was sufficiently low so that manufacturing workers could easily switch. As a consequence manufacturing workers could more easily re-employ themselves in such sector. In Table 2.10 we separate non-tradable industries into "knowledge-intensive" (KIA) and "non-knowledge-intensive", according to the Eurostat definition, and check which ones can account for most of the transitions.[28] As expected, non-KIA industries account for 100% of job transitions outside of manufacturing that occur because of Chinese trade.

In Table 2.11 we investigate differential patterns of geographical mobility. Our results indicate that exposed workers were more likely to spend more time outside of their initial LLM (panel (a), column (3)), earning more as a consequence (panel (c), column (3)). For exposed workers, the number of extra months worked in a different LLM (panel (a), column (3)) is lower in magnitude than the number of extra months worked in the non-tradable sector found in Table 2.9. This suggests that part of the new employment opportunities in the non-tradable sector are found close to home, but a substantial component requires commuting to other local labor markets. In Table 2.12 we further decompose geographical mobility responses according to whether they occur within the same region or outside the initial region. We find that workers find new job opportunities outside their region. These result stand in contrast with all previous worker-level studies on the impact of Chinese trade, where no geographical mobility responses have been found (see e.g. Autor et al. [2014], Dix-Carneiro and Kovak [2019]). This is also at odds with another strand of literature that has highlighted the relatively weak relationship between labour

[27] This difference is exacerbated by the fact that, at the same time, the number of manufacturing jobs was declining in the US and staying constant in Italy.

[28] A 2-digit sector is classified as "knowledge-intensive" if more than 1/3 of its employees have completed tertiary education

demand shocks and population in Italy [Ciani et al., 2019, among others]. The higher degree of geographical mobility in Italy in response to the China shock thus constitutes a puzzle that we aim to investigate in future research.

2.5.3 Heterogeneous responses in mobility patterns

In this section we investigate whether the mobility patterns so far investigated are heterogeneous according to worker and firm characteristcs. We run models very similar to 2.6 but we interact our import exposure measure with categorical variables of interest (and include category-specific dummies).

In Table 2.13 we look at effects of import competition by workers' initial wage level. We divide workers into groups by using terciles of average 1988-1991 earnings, within age cohort. Quite remarkably, we see that most of the effect is felt at the high-end of the wage distribution. While also low-wage workers spend less time at their initial employer and move towards other firms, effects for this category are about 10 times smaller and not significantly different from zero. When hit by a negative shock, high-wage workers find new job opportunities in the non-tradable sector and migrate towards other local labor markets. One possibility behind these effects is that high-wage workers are more likely to be employed in exporting firms, which, during this period, faced big losses in their market shares abroad, as a consequence of Chinese trade [Bugamelli et al., 2017].

Although we do not observe the exporting status of firms directly, we corroborate this evidence by looking at heterogeneous effects by the size of the firm. We divide firms according to their average firm size in 1991. Small firms have between 0 and 19 employees; medium firms have between 20 and 249 employees; big firms have more than 250 employees. We present the results in Table 2.14. Consistently with the results by wage level, among individuals working in big firms, we see that more exposed workers experience moderate gains in terms of employment and earnings. These gains are not accrued at the initial employer, where they lose approximately 2 months of employment. Rather they spend more time out of manufacturing, into the non-tradable sector, and out of their initial local labor market. Although workers in smaller firms do not experience any change in employment outcomes, they earn less overall. A coefficient of -0.023 (column (1), panel (c)) indicates that a 10 p.p. increase in import exposure leads to a decrease in earnings per 12 months worked of 2.3% of average initial annual earnings, which approximately correspond to 44 euros per month.[29]

[29]We looked into heterogeneous effects by other categorical variables such as gender and year of birth, but did not detect any difference across groups. Results are available upon request.

2.6 Conclusions

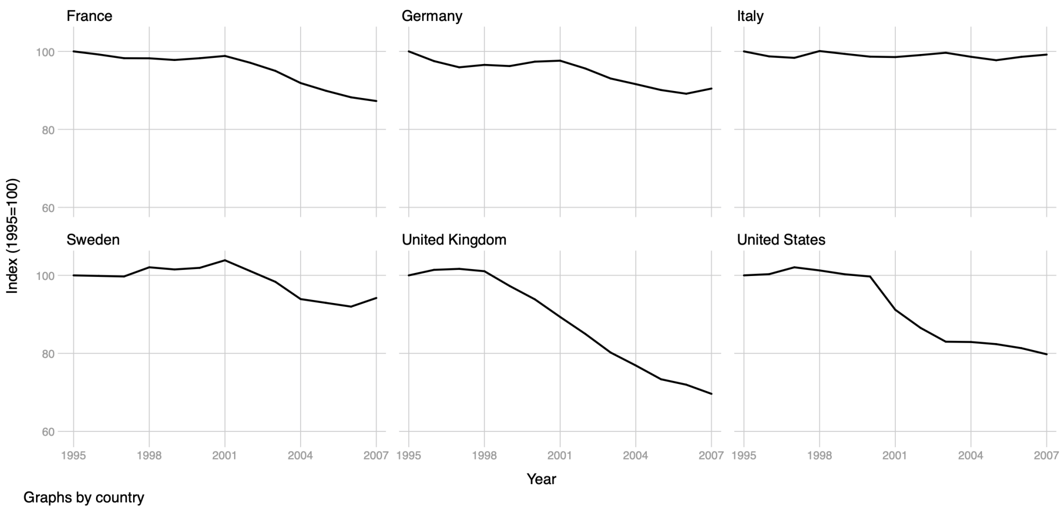
In this paper we studied the effect of the recent rise of China as major worldwide manufacturing producer on local labor markets and individual workers' careers in Italy. While a robust finding from recent works [Autor et al., 2013, Donoso et al., 2015] is that trade with China can account for a substantial fraction of the decline of manufacturing employment, we find that the impact on the Italian labor market has been modest. The lack of an overall change in employment levels does not imply, however, that the manufacturing sector did not experience some important transformations during this period. Opposite to a marked decrease in the share of manufacturing workers employed in more traditional sectors like textile and apparel, in fact, there was a corresponding increase in other sectors like metal manufacturing and machinery [Brandolini and Bugamelli, 2009].

The "China shock" could also have deteriorated the careers of incumbent manufacturing workers, whose industry-specific skills may not have allowed successful transitions towards other parts of the economy [Autor et al., 2016]. Instead, our results suggest that the presence of new job opportunities in low-skill-intensive industries in the non-tradable sector can help workers to perform successful transitions, absorbing the initial shock. We also document that successful transition were associated with an increase in geographical mobility towards areas with better job opportunities.

While the presence of job opportunities in low-skill-intensive industries outside of manufacturing can be peculiar to the Italian case, where non-tradables were gaining employment shares, our results indicate that the ability of an economy to absorb an external shock crucially depends on the macroeconomic context. From this perspective, it should be not surprising that the effects of the China shock vary tremendously across countries, as documented by existing studies.

Tables and Figures

Figure 2.1: Employment in manufacturing across selected OECD countries



Notes: The Figure displays changes in the total number of workers employed in manufacturing (1995=100). Author's elaboration on EU-KLEMS data [O'Mahony and Timmer, 2009, Jäger, 2016].

Table 2.1: Chinese import penetration and industry-level employment shares

	Δ *Import Penetration*$_{07-91}$	*Employment Share (p.p.)* 1991	2007
Tanning and dressing of leather	32.44	4.70	3.53
Furniture and manufacturing n.e.c.	25.84	5.97	6.27
Wearing apparel	19.58	8.46	5.03
Medical, optical and other instruments	13.89	2.27	2.92
Machinery and equipment	13.49	10.45	12.67
Radio, television and communication equip.	12.50	2.70	1.72
Basic metals	11.32	3.33	2.99
Electrical machinery	8.51	4.01	4.20
Textiles	8.16	7.43	4.82
Office, accounting and computing machinery	7.22	0.49	0.32
Fabricated metal products	5.86	11.83	15.93
Rubber and plastic	4.36	3.46	4.39
Other non-metallic mineral products	4.28	5.35	5.37
Other transport equipment	3.85	1.89	2.38
Wood and cork (except furniture)	3.79	3.60	3.66
Chemicals	2.38	4.57	4.17
Motor vehicles, trailers and semi-trailers	1.44	4.16	3.64
Paper	1.33	1.71	1.72
Publishing and printing	0.72	3.78	3.52
Coke, refined petroleum and nuclear fuel	0.61	0.56	0.50
Food and beverages	0.43	8.93	10.22
Tobacco	0.00	0.34	0.03

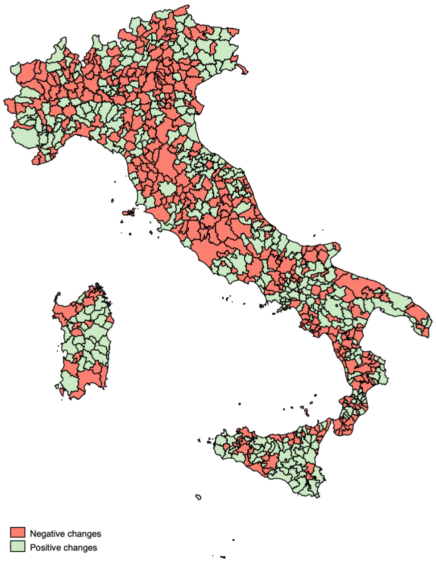
Notes: The second column reports the changes in import penetration from China, between 1991 and 2007, for each 2-digit ISIC3 industry. The change in import penetration is defined as $\Delta IP_{jt}^{ITA} = \Delta M_{jt}^{ITA}/(Y_{j,91} + M_{j,91} - X_{j,91})$. Correspondingly, the last two columns report industry employment shares in total manufacturing employment in 1991 and 2007.

Table 2.2: Summary statistics

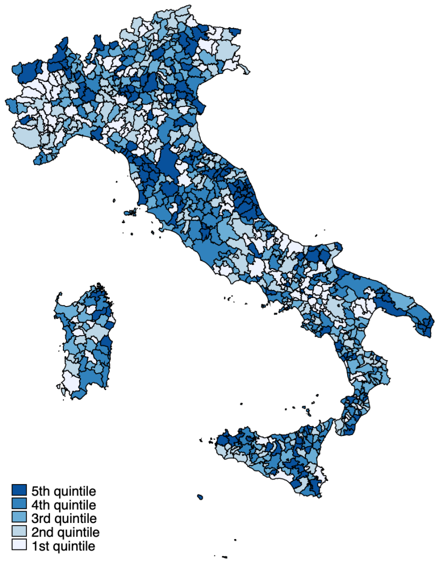
Variable	Mean	Std.Dev.
Panel (a): LLM evidence		
Long-differenced outcomes (1991-2007)		
Δ manufacturing emp/work age pop (p.p.)	-1.43	(2.71)
Δ non-tradables emp/work age pop (p.p.)	9.20	(5.17)
Δ total emp/work age pop (p.p.)	7.77	(5.08)
Import penetration changes (1991-2007)		
Δ Import penetration (1991-2001) (p.p.)	0.68	(0.52)
Δ Import penetration (2001-2007) (p.p.)	3.52	(2.47)
Control variables (1991)		
Female employment rate (p.p.)	27.50	(7.94)
Manufacturing share of empl. in 1991 (p.p.)	33.81	(11.51)
Panel (b): Worker-level evidence		
Cumulative outcomes (1992-2007)		
Months worked	157.26	51.74
Weeks worked	686.75	230.09
FTE weeks worked	674.99	234.59
Cumulative earnings (multiples of 1988-1991 average annual earn.)	15.29	6.52
Cumulative earnings per 12 months worked (multiples of 1988-1991 average annual earn.)	1.14	0.28
Years of positive earnings	13.80	4.10
Control variables (1983-1991)		
Female (share)	0.32	0.47
Apprentice (share)	0.001	0.030
Blue collar (share)	0.72	0.45
White collar (share)	0.27	0.45
Foreign-born (share)	0.021	0.14
Δ log(earnings)$_{1988-1991}$	0.09	0.21
Average log(earnings) in 1988-1991	10.06	0.30
Log average firm earnings in 1991	7.06	0.30
Share of white collars in industry in 1991	0.25	0.14
Δlog(Earnings) 1983-1991 of industry	0.70	0.07

Notes: The table provides summary statistics for variables employed in both the local labour market and worker-level analyses. In panel (a) averages are calculated starting from local labor markets and weighted by start-of-period working-age population. In panel (b) we provide summary measures for the set of all workers who had a year-round job in manufacturing in 1991 and also had a year-round job in all years between 1988 and 1990. Months worked are defined as calendar months with at least one day of positive earnings. Cumulative earnings measures are both expressed in multiples of average 1988-1991 earnings.

Figure 2.2: Changes in manufacturing employment and import penetration across local labor markets



(a) Δ manufacturing share of w.a.p



(b) Δ Import penetration

Notes: The Figure displays 2001-2007 changes for 784 local labor markets. Subfigure (a) displays changes in the share of working-age population that is employed in manufacturing. Subfigure (b) displays changes in the import penetration ratio. Both measures are first residualized against the manufacturing employment share in 2001.

Table 2.3: Imports from China and changes in manufacturing employment (2SLS estimates)

	Δ manuf emp/work age pop (p.p.)			
	(1)	(2)	(3)	(4)
Panel (a) : 1991-2007 stacked differences				
ΔImport penetration^{ITA} (p.p.)	-0.253***	-0.203***	-0.146***	-0.132***
	(0.0436)	(0.0478)	(0.0425)	(0.0471)
Panel (b) : First stage estimates				
ΔImport penetration^{OC} (p.p.)	0.0621***	0.0587***	0.0555***	0.0585***
	(0.00299)	(0.00333)	(0.00359)	(0.00150)
Observations	1568	1568	1568	1568
K-P F-stat.	431.9	309.5	239.5	1525.2
Region FE	NO	YES	YES	YES
LLM controls	NO	NO	YES	YES
Weights	YES	YES	YES	NO

Notes: The table presents 2SLS regressions of the change in manufacturing employment over working age (15-64) population against changes in the import penetration ratio, at the local labor market level ($N = 784$). Region FE include 20 dummies. LLM controls include the female employment rate and the manufacturing share in total employment in 1991. The latter corresponds to the number of people employed in manfuacturing industries over total private non-farm employment. Regressions in columns 1 to 3 are weighted using beginning of period LLM working-age population. Standard errors are clustered at the local labor market level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Future import from China and change of manufacturing employment between 1981 and 1991 (2SLS estimates)

	$\Delta_{'91-'81}$ manuf emp/work age pop (p.p.)			
	(1)	(2)	(3)	(4)
ΔImport penetration$^{ITA}_{1991-2001}$ (p.p.)	0.169	-0.324		
	(0.436)	(1.232)		
ΔImport penetration$^{ITA}_{2001-2007}$ (p.p.)			0.0522	-0.00627
			(0.0665)	(0.211)
Observations	784	784	784	784
K-P F-stat.	620.5	899.3	143.5	617.7
Region FE	YES	YES	YES	YES
LLM controls	YES	YES	YES	YES
Weights	YES	NO	YES	NO

Notes: The table presents 2SLS regressions of the change in manufacturing employment over working age (15-64) population between 1981 and 1991 against changes in future import penetration, at the local labor market level ($N = 784$). In the first two columns the change in future import penetration is computed between 1991 and 2001, in the last two columns the change in import penetration is computed between 2001 and 2007. Region FE include 20 regions dummies. LLM controls include the female employment rate and the manufacturing share in total employment, i.e. the number of people employed in manfuacturing industries over total private non-farm employment, measured at the start of the previous decade, i.e. in 1971. Regressions in columns 1 and 3 are weighted using beginning of period LLM working-age population. Standard errors are clustered at the local labor market level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Import from China and other labor market outcomes (2SLS estimates)

	(1) Mfg. Empl.	(2) Non-trad. Empl.	(3) Total Empl.	(4) Δ log w.a.p.
ΔImport penetration^{ITA}	-0.146***	-0.0412	-0.187**	0.00157
	(0.0425)	(0.0595)	(0.0834)	(0.00106)
Observations	1568	1568	1568	1568
K-P F-stat.	239.5	239.5	239.5	1525.2
Region FE	YES	YES	YES	YES
LLM controls	YES	YES	YES	YES
Weights	YES	YES	YES	NO

Notes: The table presents 2SLS regressions for the stacked difference model between 1991 and 2007. In the first column the dependent variable is the change in manufacturing employment over working age (15-64), as in column 3, panel a of table 2.3. In the second column the dependent variable is the change in the number of people employed in non-tradables over working age (15-64) population. In the third column the dependent variable is the change in the total number of people employed in the private non-farm sector over working age (15-64) population. Finally, in the last column, the dependent variable is the is the (natural) log change in working age (15-64) population. Coefficients in column (1) and column (2) sum up to the coefficient in column (3). Region FE include 20 regions dummies. LLM controls include the female employment rate and the manufacturing share in total employment, i.e. the number of people employed in manfuacturing industries over total private non-farm employment, measured at the start of the period. All regressions are weighted using beginning of period LLM working-age population. Standard errors are clustered at the local labor market level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Rotemberg weights and industry-specific components

Variable	α_k	β_k	95% CI
Panel (a): United States			
Top 5 Rotemberg weights industries (SIC87DD - 392 industries)			
Electronic Computers	0.133	-0.358	[-0.74, 0.15]
Furniture and Fixtures, NEC	0.118	-0.732	[-1.06, -0.48]
Radio and TV Broadc. and Communic. Equipment	0.063	0.037	[-0.50, 0.83]
Semiconductors and Related Devices	0.052	-0.897	[-1.50, -0.49]
Games, Toys, and Children?s Vehicles	0.048	-0.205	[-0.49, 0.08]
Overall $\beta = -0.674$ (0.073)			
Panel (b): Italy			
Top 5 Rotemberg weights industries (ISIC Rev. 3 - 125 industries)			
Cutting, shaping and finishing of stone	0.557	0.023	[-0.06, 0.11]
Footwear	0.232	-0.276	[-0.43, -0.13]
Wearing apparel, except fur	0.054	-0.307	[-0.60, -0.04]
Knitted and crocheted fabrics	0.025	-0.802	[-1.63, -0.36]
Other general purpose machinery	0.023	-0.114	[-0.55, 0.33]
Overall $\beta = -0.146$ (0.043)			

Notes: The table reports Rotemberg weights (α_k) and associated marginal effects (β_k) for industries with the 5 highest Rotemberg weights, for the United States (panel (a)) and Italy (panel (b)). 95% CI is the weak-IV robust confidence interval developed in Chernozhukov and Hansen [2008]. Industries are at the 4-digit level and follow the SIC87DD classification in the United States and the ISIC Rev. 3 classification in Italy. Industry-level effects cannot be compared across panels as the number of industries differs. The overall effect (β) is the IV estimate from using the Bartik instrument.

Table 2.7: Import competition from China and cumulative labour market outcomes at the individual level over 1991-2007 (2SLS estimates)

	Cumulative Months (1)	Cumulative Weeks (2)	Cumulative FTE weeks (3)	Cumulative Earnings (4)	Cumulative Earnings per year (5)
$\Delta IP^{ITA}_{2007-1991}$	0.013	0.077*	0.088*	0.003	0.009
	(0.011)	(0.045)	(0.045)	(0.002)	(0.009)
Observations	692079	692079	692079	692079	692079
Full controls	YES	YES	YES	YES	YES
K-P F-stat.	458.054	458.054	458.054	458.054	458.054

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration. All outcomes are totals over the 16-year period between 1991 and 2007. In column (1)-(4) the dependent variable is the number of months/weeks/full-time-equivalent weeks with at least one day of positive earnings, respectively. For each spell, full-time equivalent weeks are constructed by multiplying the number of weeks worked by the part-time percentage of that contract. In column (5) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In column (6) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in column (1). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls from specification 2.6. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Import competition from China and labor mobility (2SLS estimates)

	Total (1)	Same firm (2)	Other firm (3)
Panel (a) : Months with positive earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.013	-0.069**	0.082**
	(0.011)	(0.032)	(0.032)
Panel (b) : Cumulative earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.003	-0.009**	0.011***
	(0.002)	(0.003)	(0.003)
Panel (c) :Earnings per effective year			
$\Delta IP^{ITA}_{2007-1991}$	0.009	-0.007	0.033**
	(0.009)	(0.008)	(0.013)
Full controls	YES	YES	YES
K-P F-stat.	458.054	458.054	458.054

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Import competition from China and labor mobility (2SLS estimates)

		Within manuf.		Outside manuf.
	Other firm	Same 2-dig	Other 2-dig	Non-tradables
	(1)	(2)	(3)	(4)
Panel (a) : Months with positive earnings				
$\Delta IP^{ITA}_{2007-1991}$	0.082**	-0.065**	-0.047**	0.195***
	(0.032)	(0.031)	(0.021)	(0.028)
Panel (b) : Cumulative earnings				
$\Delta IP^{ITA}_{2007-1991}$	0.011***	-0.008**	-0.005**	0.024***
	(0.003)	(0.003)	(0.002)	(0.003)
Panel (c) :Earnings per effective year				
$\Delta IP^{ITA}_{2007-1991}$	0.033**	-0.015	0.009	0.091***
	(0.013)	(0.025)	(0.021)	(0.013)
Full controls	YES	YES	YES	YES
K-P F-stat.	458.054	458.054	458.054	458.054

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: Import competition from China and labor mobility (2SLS estimates)

	Non-tradables	Non Knowledge intensive	Knowledge intensive
	(1)	(2)	(3)
Panel (a) : Months with positive earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.195***	0.192***	0.002
	(0.028)	(0.035)	(0.011)
Panel (b) : Cumulative earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.024***	0.024***	0.000
	(0.003)	(0.004)	(0.001)
Panel (c) :Earnings per effective year			
$\Delta IP^{ITA}_{2007-1991}$	0.091***	0.100***	0.050**
	(0.013)	(0.012)	(0.021)
Full controls	YES	YES	YES
K-P F-stat.	458.054	458.054	458.054

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: Import competition from China and labor mobility (2SLS estimates)

	Other firm	Same LLM	Other LLM
	(1)	(2)	(3)
Panel (a) : Months with positive earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.082**	-0.028**	0.110***
	(0.032)	(0.013)	(0.030)
Panel (b) : Cumulative earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.011***	-0.004**	0.015***
	(0.003)	(0.002)	(0.003)
Panel (c) :Earnings per effective year			
$\Delta IP^{ITA}_{2007-1991}$	0.033**	0.006	0.068***
	(0.013)	(0.016)	(0.016)
Full controls	YES	YES	YES
K-P F-stat.	458.054	458.054	458.054

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12: Import competition from China and labor mobility (2SLS estimates)

	Other LLM	Same region	Other region
	(1)	(2)	(3)
Panel (a) : Months with positive earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.110***	-0.017***	0.127***
	(0.030)	(0.006)	(0.031)
Panel (b) : Cumulative earnings			
$\Delta IP^{ITA}_{2007-1991}$	0.015***	-0.002***	0.017***
	(0.003)	(0.001)	(0.004)
Panel (c) :Earnings per effective year			
$\Delta IP^{ITA}_{2007-1991}$	0.068***	0.028	0.101***
	(0.016)	(0.025)	(0.020)
Full controls	YES	YES	YES
K-P F-stat.	458.054	458.054	458.054

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: Import competition effects and initial wage levels

	Overall mobility			Sectoral mobility			Geographical mobility	
	Total	Same firm	Other firm	Same 2-dig	Other 2-dig	Non-tradables	Same LLM	Other LLM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a) : Months with positive earnings								
ΔIP × low wage	0.012	-0.010	0.021	0.052*	-0.065***	0.034**	0.005	0.016
	(0.018)	(0.038)	(0.025)	(0.029)	(0.022)	(0.015)	(0.016)	(0.020)
ΔIP × medium wage	0.001	-0.060*	0.061**	-0.036	-0.026	0.123***	0.002	0.059***
	(0.014)	(0.036)	(0.027)	(0.026)	(0.026)	(0.028)	(0.018)	(0.022)
ΔIP × high wage	0.031**	-0.117*	0.149**	-0.199***	-0.045*	0.393***	-0.081***	0.230***
	(0.013)	(0.069)	(0.068)	(0.059)	(0.027)	(0.043)	(0.022)	(0.066)
Panel (b) : Cumulative earnings								
ΔIP × low wage	-0.002	-0.003	0.001	0.005	-0.007***	0.004**	-0.001	0.002
	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
ΔIP × medium wage	-0.001	-0.007*	0.006**	-0.004*	-0.004	0.014***	-0.001	0.007***
	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
ΔIP × high wage	0.009***	-0.014**	0.023***	-0.023***	-0.005	0.050***	-0.009***	0.032***
	(0.002)	(0.007)	(0.006)	(0.006)	(0.003)	(0.005)	(0.003)	(0.007)
Panel (c) :Earnings per effective year								
ΔIP × low wage	-0.022**	-0.017**	-0.019	-0.043***	-0.014	-0.011	-0.015	-0.006
	(0.009)	(0.008)	(0.013)	(0.015)	(0.027)	(0.012)	(0.020)	(0.020)
ΔIP × medium wage	-0.007	-0.006	0.002	-0.007	-0.042*	0.063***	-0.013	0.048**
	(0.010)	(0.009)	(0.015)	(0.025)	(0.025)	(0.023)	(0.015)	(0.023)
ΔIP × high wage	0.041***	-0.000	0.073***	-0.004	0.060	0.128***	0.025	0.090***
	(0.013)	(0.013)	(0.022)	(0.042)	(0.036)	(0.024)	(0.022)	(0.020)
Full controls	YES	YES	YES	YES	YES	YES	YES	YES
K-P F-stat.	18.261	18.261	18.261	18.261	18.261	18.261	18.261	18.261

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. High wage, medium wage and low wage are dummies for terciles of average 1988-1991 earnings, within age cohort. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14: Import competition effects and firm size

	Overall mobility			Sectoral mobility			Geographical mobility	
	Total	Same firm	Other firm	Same 2-dig	Other 2-dig	Non-tradables	Same LLM	Other LLM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a) : Months with positive earnings								
ΔIMP × small	0.009	0.012	-0.003	0.034	-0.058***	0.021	-0.006	0.003
	(0.012)	(0.029)	(0.025)	(0.027)	(0.020)	(0.017)	(0.015)	(0.019)
ΔIMP × medium	-0.044**	-0.034	-0.010	-0.067	-0.046	0.103***	-0.010	0.001
	(0.021)	(0.067)	(0.056)	(0.063)	(0.035)	(0.018)	(0.052)	(0.028)
ΔIMP × big	0.045**	-0.234**	0.280***	-0.250***	-0.028	0.558***	-0.077*	0.357***
	(0.020)	(0.090)	(0.087)	(0.062)	(0.035)	(0.080)	(0.041)	(0.097)
Panel (a) : cumulative earnings								
ΔIMP × small	-0.003	-0.001	-0.001	0.002	-0.006***	0.003	-0.002	0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
ΔIMP × medium	-0.005	-0.005	-0.000	-0.007	-0.005	0.012***	-0.002	0.001
	(0.004)	(0.007)	(0.005)	(0.006)	(0.004)	(0.002)	(0.004)	(0.003)
ΔIMP × big	0.015***	-0.024***	0.040***	-0.027***	-0.003	0.070***	-0.008*	0.048***
	(0.003)	(0.009)	(0.009)	(0.006)	(0.004)	(0.010)	(0.005)	(0.011)
Panel (c) :Earnings per effective year								
ΔIMP × small	-0.023**	-0.014*	-0.024	-0.044***	-0.033	-0.007	-0.019	-0.019
	(0.011)	(0.007)	(0.015)	(0.015)	(0.028)	(0.012)	(0.017)	(0.021)
ΔIMP × medium	-0.007	-0.007	0.021	0.008	0.014	0.079**	0.018	0.065
	(0.020)	(0.018)	(0.037)	(0.040)	(0.053)	(0.037)	(0.036)	(0.054)
ΔIMP × big	0.074***	0.007	0.103***	0.025	0.080**	0.156***	0.044**	0.117***
	(0.011)	(0.012)	(0.017)	(0.050)	(0.038)	(0.018)	(0.020)	(0.020)
Full controls	YES	YES	YES	YES	YES	YES	YES	YES
K-P F-stat.	18.261	18.261	18.261	18.261	18.261	18.261	18.261	18.261

Notes: The table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration in Italy. In panel (a) the dependent variable is the cumulative number of months with positive earnings in the private non-farm sector over the 1991-2007 period. In panel (b) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (c) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls. Small, medium and big are dummies for firms with 0-19, 20-249, 250 and more employees in 1991, respectively. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendices

2.A Additional Tables and Figures

Table 2.A.1: Import from China and change of manufacturing employment (OLS estimates)

	Δ manuf emp/work age pop (p.p.)			
	(1)	(2)	(3)	(4)
Panel (a) : 1991-2007 stacked differences				
ΔImport penetration^{ITA}	-0.264***	-0.240***	-0.208***	-0.140***
	(0.0396)	(0.0433)	(0.0429)	(0.0403)
Observations	1568	1568	1568	1568
Region FE	NO	YES	YES	YES
LLM controls	NO	NO	YES	YES
Weights	YES	YES	YES	NO

Notes: The table presents OLS regressions of the change in manufacturing employment over working age (15-64) population against changes in the import penetration ratio. Region FE include 20 dummies. LLM controls include the female employment rate and the manufacturing share in total employment in 1991. The latter corresponds to the number of people employed in manfuacturing industries over total private non-farm employment. Regressions in columns 1 to 3 are weighted using beginning of period LLM working age population. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.A.2: Chinese import competition and individual labour market outcomes

	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS
Panel (a): Months worked							
ΔImport penetration^{ITA}	-0.035	-0.028	-0.024	-0.009	-0.010	-0.002	0.013
	(0.033)	(0.034)	(0.038)	(0.017)	(0.015)	(0.014)	(0.011)
Panel (b): Weeks worked							
ΔImport penetration^{ITA}	-0.140	-0.095	-0.069	-0.009	-0.016	0.011	0.077*
	(0.149)	(0.150)	(0.161)	(0.074)	(0.062)	(0.059)	(0.045)
Panel (c): FTE Weeks worked							
ΔImport penetration^{ITA}	-0.159	-0.088	-0.054	0.016	-0.009	0.021	0.088*
	(0.167)	(0.162)	(0.176)	(0.082)	(0.065)	(0.061)	(0.045)
Panel (d): cumulative earnings							
ΔImport penetration^{ITA}	-0.001	0.003	0.002	0.003	0.001	0.000	0.003
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Panel (e): earnings per effective year							
ΔImport penetration^{ITA}	0.016	0.036**	0.029	0.030**	0.016	0.000	0.009
	(0.012)	(0.016)	(0.018)	(0.014)	(0.014)	(0.012)	(0.009)
Year of birth FE	YES	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES	YES
Industry Char.	NO	NO	YES	YES	YES	YES	YES
Industry PreTrend	NO	NO	NO	YES	YES	YES	YES
Individual Char.	NO	NO	NO	NO	YES	YES	YES
Firm Char.	NO	NO	NO	NO	NO	YES	YES
LLM FE	NO	NO	NO	NO	NO	NO	YES
K-P F-stat.		110.980	341.532	416.147	418.732	424.936	458.054

Notes: This table presents 2SLS regressions of individual labour market outcomes against changes in Chinese import penetration. All outcomes are totals over the 16-year period between 1991 and 2007. In panels (a)-(e) the dependent variable is the number of months/weeks/full-time-equivalent weeks with at least one day of positive earnings, respectively. For each spell, full-time equivalent weeks are constructed by multiplying the number of weeks worked by the part-time percentage of that contract. In panel (d) the dependent variable is the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings. In panel (e) the dependent variable is 100× the total of earnings accrued over the 1991-2007 period, in multiples of average yearly 1988-1991 earnings, divided by $(m_i/12)$, where m_i is the dependent variable in panel (a). The latter measure can be interpreted as cumulative earnings per 12 months worked, normalized by average initial earnings. All regressions include a constant, and the full set of controls from specification 2.6. Standard errors are clustered at the 4-digit sector level and reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.A.3: International comparison of the effects of Chinese import competition

	Jobs lost		**Manuf. Empl$_{1995}$**	**Perc. drop**
	(1)	(2)	(3)	(4)
Country	1990s	2000s		
France	16,000	88,000	3,497,000	2.97%
Germany		312,000*	8,040,000	3.88%
Italy	24,000	119,000	4,637,000	3.08%
Norway	750	3,400	395,000	1.05%
Spain	51,000	280,000	2,385,000	13.87%
United States	548,000	980,000	17,231,000	8.87%

Notes: The table reports the number of manufacturing jobs that were lost due to the rise of China (columns 1-2), the number of manufacturing jobs in 1995 (column 3), and the corresponding percentage drop (column 4), by country. Figures in columns 1-2 are obtained via a variance decomposition first presented in Autor et al. [2013] and only uses the supply-side component of trade with China. Results for France come from [Malgouyres, 2017, p.422] and authors' calculations based on descriptive statistics in Table 1 of the same paper. Results for Germany come from [Dauth et al., 2014, p.1656], and results are only available for the whole 1988-2008 period, indicated with (*). Effects also include Eastern-European exposure. Results for Spain come from [Donoso et al., 2015, p. 1756] and authors' calculations based on footnote 14 of the same paper. Results from Norway come from [Balsvik et al., 2015, pp. 142-143]. Results from the US come from [Autor et al., 2013, p.2140]. Aggregate manufacturing figures in column 3 are obtained from EU-KLEMS [O'Mahony and Timmer, 2009, Jäger, 2016] for European countries and authors' calculations on figures in Balsvik et al. [2015], OECD [2019] and Eurostat [2019] for Norway. Numbers in column (4) are obtained by summing numbers in columns 1-2 and dividing by the corresponding figure in column (3). Time windows are slightly different across studies: Autor et al. [2013] uses 1991-2000 and 2000-2007. Malgouyres [2017] uses 1995-2001 and 2001-2007. Donoso et al. [2015] use 1999-2003 and 2003-2007. Balsvik et al. [2015] uses 1996-2001 and 2002-2007. Dauth et al. [2014] uses 1988-2008.

Chapter 3

What are the returns to apprenticeships? Evidence from Italy

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Abstract

What are the returns to apprenticeships? This paper tries to answer this question by leveraging novel administrative data from Italy on individual careers. We adopt a difference-in-difference methodology to compare the labor market outcomes of individuals starting an apprenticeship with those of similar individuals starting temporary contracts that, at least formally, do not provide training. We find apprenticeships to be a "double-edged sword". While they do guarantee a stronger labor market attachment during the first three years after the start of the contract, they produce ambiguous effects afterwards. Apprenticeships increase the probability of conversion to open-ended contracts, especially at the initial firm, but decrease the probability of obtaining further temporary jobs, especially at other firms. Quantitatively, this second effect prevails, generating a negative effect of the probability of having any job. These findings are consistent with a model where retention rates after the end of an apprenticeship convey stronger signals about workers' ability compared to retention after the end of a temporary contract.

3.1 Introduction

Apprenticeships are diffused in many European countries and constitute a middle-ground between high school and university education. Although there are differences across countries, they usually consist of job contracts where labour services are exchanged for certified training in an occupation and a salary [Snell, 1996, Ryan, 2012]. In recent years apprenticeships acquired a prominent place in the policy discourse about youth unemployment and the NEET problem, with many governments offering reduced social security contributions or favorable taxation regimes to incentivize their use [Kuczera, 2017]. Although in policy circles apprenticeships are often seen as a *panacea*, providing young people with good jobs and valuable skills, the economic reality may not be that simple. While it is true that apprentices ought to receive training by virtue of a contractual obligation, it is not a given that on-the-job training provided through apprenticeships has any real content. Firms may have scarce incentives to train if the human capital they need for production is general [Becker, 1962] and even more so if the labor market where they operate does not feature any frictions [Acemoglu and Pischke, 1999]. In such cases, given the low enforcement level of apprenticeship contracts, firms may renege on the promise to provide training and the returns to apprenticeships would be close to zero [Dustmann and Schönberg, 2012]. Conversely, firms will be more incentivized to provide training to young workers if the human capital they need is firm-specific, or if labor market frictions are substantial.

In this paper we empirically quantify the returns to apprenticeships by leveraging novel administrative matched employer employee data from the Italian Social Security Institute (INPS). We have access to the full working history for the universe of individuals born in Italy in 1980 and 1981, regardless of whether they have been employees in the private sector, dependent self-employed (*parasubordinati*) or self-employed. We define returns to apprenticeships as the extra gain coming from starting an apprenticeship compared to a temporary contract. Similarly to the former, temporary contracts also involve an employer-employee relationship but, at least formally, they don't require the firm to provide training. We perform this comparison in a difference-in-difference framework at the job spell level.

The comparison of apprenticeships with other temporary contracts is not completely new in the literature [Berton et al., 2011, Picchio and Staffolani, 2013] and is particularly relevant in the Italian setting. On the one hand the vast majority of apprenticeships happen when individuals have already left technical and vocational schools, and are not formally linked to the education system.[1] Also, apprentices' training can take place entirely within the firm premises and trainees

[1] During the years 2007-2013 INPS data provide information on the type of apprenticeship contract.

do not need to sit a formal examination at the end of the contract.[2] These characteristics make such contracts more similar to temporary training contracts than to a course of study. On the other hand the question of whether apprenticeships are really any different from other types of temporary contracts is recurrent in the Italian debate. Some scholars in other disciplines go as far as saying that "Although a number of legal provisions establish compulsory training during apprenticeship, reality is often very distant from the ideal apprenticeship model, and this tool becomes a mere instrument of exploitation of a flexible and cheaper labour force" [Tiraboschi, 2012, p.20]. For these reasons we think that our focus on temporary contracts is indeed justified to evaluate the returns to apprenticeships in our setting. Given that temporary contracts are known to receive little or no firm-sponsored training [Booth et al., 2002, Albert et al., 2005], they are suited to gauge the magnitude of the returns to training at the *extensive margin*.

To preview our results, we find that apprenticeships are a "double-edged sword". They lead to higher conversion rates towards open-ended contracts, but have a negative effect on the probability of transitioning to other temporary contracts. Quantitatively, the second effect is stronger and produces a negative average treatment effect on the probability of having a job of any kind. We find that most conversions happen at the training firm, while the lack of job opportunities in other temporary contracts is explained by what happens in other firms. On the one hand, this indicates that training provided through apprenticeships is valuable and that training firms are able to appropriate some rents from it.[3] On the other hand it seems that "recalls" do not explain why apprentices spend less time churning between other temporary contracts.

Our findings can be rationalized in an asymmetric information model with adverse selection, where the absence of conversion to a permanent position for an apprentice conveys a stronger signal about ability than for a temporary contract. This can be the case if temporary contracts can fail to be converted because of reasons that are exogenous to the worker's ability with higher probability than apprentices (e.g. the task is temporary in nature ...). In this sense apprenticeships constitute a riskier investment compared to a temporary contract, and its convenience may depend on a worker's ability level and preferences. Alternatively, apprentices may be acquiring firm-specific human capital that is not necessarily useful outside the training firm, and leads to a penalty in terms of future job offers from other firms.

When looking at earnings, we find that, conditional on working, apprenticeships pay off in the first three years after the start of the contract. However we fail to detect any long-run

The share of apprenticeships linked to upper-secondary education was 19.8% in 2007 and steadily declined throughout the time window, reaching a low of 4.6% in 2013.

[2] *Cassazione, sent. 845/1988.*

[3] We leave the question open as to whether such training is firm-specific or if rent extraction is allowed by labour market imperfections [Acemoglu and Pischke, 1999]

effects. Earnings effects are not significantly different from zero six years after having started the contract.

Our paper contributes to the literature on the returns to apprenticeships. Various other studies have measured the extent to which apprenticeships constitute a valid opportunity for the young, when compared to different alternative opportunities. The general consensus so far reached is that apprentices are better off in terms of wages if compared to low-skilled workers with no apprenticeship training, but not if compared to individuals completing full-time vocational education in the classroom (for a review see Samek Lodovici et al. [2013]). Also, while apprenticeships facilitate the school-to-work transition and pay off at an early stage of the working life, their effects may be more muted in the longer run [Samek Lodovici et al., 2013, Hanushek et al., 2017, Parey, 2016].

More in detail, Parey [2016] compares firm-sponsored training with school-based vocational education. He finds that the two tracks do not offer different returns, but that in the very short run firm-based apprenticeships provide stronger labor market attachment. He also finds no effects on wages. Similarly Albanese et al. [2017] compares two apprenticeship tracks that co-existed in Italy in the early 2000s, one of which emphasized firm-sponsored training rather than school-based vocational education. In line with Parey [2016], they find that firm-sponsored training improved the prospects of young workers, increasing their probability of transitioning to open-ended contracts but it also raised their wage levels, especially in bigger firms. Cavaglia et al. [2018] also find positive effects in the UK context. They find that apprentices yield substantial earning premia, especially for men. Fersterer et al. [2008] compare longer and shorter apprenticeships. For identification they exploit the unexpected closure of firms that employ apprentices at different tenure horizons. At such *intensive margin*, they find that an extra year into apprenticeship yields a 3.8% return in terms of higher earnings.

Due to a similar choice of a control group, the studies closest in spirit to ours are Picchio and Staffolani [2013] and Berton et al. [2011]. The first paper exploits age limits in the Italian apprenticeship system and use a regression discontinuity design to compare individuals who manage to get an apprenticeship just before age 30 and those who do not manage to do so. The authors find that, around age 30, individuals who start an apprenticeship are more likely to transition towards open-ended contracts, especially at the initial firm. The second paper uses a Multinomial Logit with individual fixed effects to study the transition matrices between different types of temporary contracts (including apprenticeships) and open-ended contracts. We extend these analyses in different ways: first we characterize the full time profile of returns to apprenticeships at the quarterly frequency and are able to look into the long run, up to six years after the start of the contract. Second, thanks to the matched employer-employee nature of the data we can look at how much of the conversion rate to open-ended contracts can be explained by the training firm or the other firms. Third we look at heterogeneous effects depending on firm size and are able to look at new outcomes that were unstudied before due to data limitations, such as the probability of entering self-employment.

The paper is structured as follows. In Section 3.2 we describe how apprenticeships are regulated in Italy and the data we employ for our analysis. In Section 3.3 we present our identification strategy and regression framework. In Section 3.4 and we present our main findings. In Section 3.5 we present some heterogeneity analysis along the firm size dimension. In Section 3.6 we present other results on the self-employment margin and on earnings. In Section 3.7 we discuss our results and in Section 3.8 we conclude.

3.2 Institutional Framework and Data

3.2.1 Apprenticeships in Italy

The Italian apprenticeship system is made of three separate programmes, with different rules: (1) "right and duty" (*Apprendistato per l'espletamento del diritto/dovere di istruzione*), performed during upper secondary education for individuals aged 15-18 (2) "occupational" (*Apprendistato professionalizzante*), usually performed after the completion of secondary education, for individuals aged 18-29 and (3) "higher" (*Apprendistato di alta formazione e ricerca*), still oriented to individuals between 18 and 29, but who are enrolled in or have already earned a university degree and would like to carry out a thesis or a research project within a firm. In our analysis we require individuals to be at least 22 when doing their apprenticeships, so this excludes type (1) apprenticeships by construction.[4] On the other hand in the data we do not have information needed to distinguish apprenticeships of type (2) from those of type (3) before 2007 or after 2013, so in what follows type (2) and type (3) are pooled together. Again, we stress the fact that the vast majority of apprenticeships in Italy are of type (2).

In terms of contractual obligations, apprenticeships are job contracts, limited to the private sector, in which worker and firm regularly pay social security contributions and work accidents insurance. The formal training content of apprenticeships is quite low. The minimum number of training hours that the firm must provide is 120 per year, split in the following way: 65% are dedicated to occupation-specific training and 35% are dedicated to general training (job safety, psychology of labor and team working). In exchange for training, firms obtain a reduction in

[4]Our analysis excludes individuals younger than 22 at start of the contract in order to have sufficient information on the pre-event working history. This allows us to test whether individuals displayed parallel trends in the outcome variable before the onset of the contract.

social security contributions. The latter amounts to 10% of apprentices' gross earnings, compared to 27% for open-ended and temporary contracts. Also, firms can pay apprentices a lower wage, up to two levels below what a qualified worker would get, according to the corresponding collective bargaining agreement (CBA). At the end of the programme the workers receive a certification which is recognized by firms applying the same CBA. This implies a worker cannot be trained twice for the same occupation in the same CBA. Eligibility on the side of firms is linked to the presence of a *mentor*. The mentor must attend preparatory training and cannot train more than 5 individuals at each point in time. The law sets ceilings in apprenticeship use: they can never be more than the number of qualified workers in the firm (however if firm size is less than 3 the firm can hire up to 3 apprentices). Eligibility on the side of workers is exclusively age-dependent. Recent reforms raised the age limits (measured on the day of hiring).[5] A more complete description of the Italian apprenticeship contract and its recent reforms can be found in Albanese et al. [2017].

3.2.2 Data sources

We use administrative data on careers at the individual level made available by the Italian Social Security Institute (INPS) through the VisitINPS initiative. Below we present each source in detail:

Matched employer-employee data: our primary source is a matched employer-employee dataset covering all job spells in non-agricultural firms with at least one employee. The dataset spans the whole time period 1983-2017. The public sector and firms with no employees are not included. The data records the presence of job spells at the monthly frequency, which gives us the advantage tracing career dynamics at a very fine level. In each month we observe at which firm(s) the worker is employed, the type of contract(s) the worker has (open-ended, temporary), the type of work-time arrangement (full time or part time) and a coarse occupation code (apprentice, blue collar, white collar, supervisor or manager). Absent any change in the aforementioned characteristics, we observe one earning record per year for each worker. In case a worker has a contractual change during the year (e.g. becomes a white collar worker or changes firm) we see two separate earning records. This allows us to precisely separate earning records which belong to different contract characteristics, different firms and different years. For each individual we also observe a series of basic socio-demographic characteristics such as gender, year of birth and place of birth. Given the nature of the dataset, we are also able to build the total firm size in every year, and therefore check whether individuals starting apprenticeships in

[5] The 1997 (*Treu*) reform: from age 20 to age 24 (but 27 in regions entitled to EU structural funds - i.e. the South - and age 29 in artisan firms). The 2003 (*Biagi*) reform: from age 24 to age 29 in all firms in all regions.

bigger firms obtain higher returns.

Dependent self-employment spells: starting from 1996, we also have information on dependent self-employment. The latter is a form of work where workers are formally self-employed but *de facto* employees [Williams and Lapeyre, 2017]. This dataset also has a matched employer-employee structure. For each job spell we observe unique worker and firm identifiers, the beginning and end date of the spell, the type of contract and the overall compensation received for the job in every year. Given that firm and worker identifiers are the same across datasets we are able to merge this information with the matched employer-employee dataset.

Contribution Histories: for a subset of individuals in the matched employer-employee dataset we were able to obtain further information on their full contribution history, including spells as self-employed. This allows us to build more precise measures of labour market outcomes and investigate whether apprenticeships have an impact on the probability of entering self-employment. This dataset does not contain a firm identifier. We obtained such information for the universe of individuals born in Italy between 1980 and 1981, that is our main sample of interest.

3.2.3 Sample selection and variable construction

Our initial sample is made of all individuals born in Italy in 1980 and 1981. We focus on these two cohorts because information on whether an individual works in an open-ended or temporary contract is only available from 1998 onwards (approximately when our individuals leave upper-secondary education). On the other hand we don't choose cohorts younger than 1981 to have a long enough period to observe the evolution of the outcome variables. We restrict the sample only to those individuals who ever started an apprenticeship or a temporary contract between age 22 and age 29. We do not consider contracts starting before age 22 in order to have enough information on past working history, which is useful to check for the presence of underlying pre-trends.

In what follows, we refer to the start of either a temporary contract or an apprenticeship as an *event*. Apprenticeships are treatment events, while temporary contracts are control events. In our empirical strategy, we will look at the differential evolution of outcomes of interest around the event date, between these two types of events.

Among events we only consider first-time temporary contracts and first-time apprenticeships. Further apprenticeships or temporary contracts are not considered, although they contribute to the construction of the outcome variables. An individual may appear more than once (twice at most) in the sample if she starts a temporary contract and then starts an apprenticeship at a later age. In this case we include both events in the regression and study them separately.[6] To the contrary, if an individual starts an apprenticeship and then starts a temporary contract at a later age, only the apprenticeship is included as an event - the temporary contract is used for the construction of outcome variables. If apprenticeships indeed have dynamic effects, then including the latter type of temporary contracts in the set of events risks contaminating the control group and invalidating our design. For similar reasons we drop all individuals who do an apprenticeship and a temporary contract at exactly the same age. Our final sample consists of 285,422 events, either apprenticeships contracts (103,878) or a temporary contracts (181,544).

Although our data would allow us to construct employment outcomes at the monthly frequency, we collapse our dataset at the quarterly level for computational convenience. All employment outcomes are coded as dummy variables, taking value one when the condition is true for at least one month during the quarter. Due to workers changing jobs or holding multiple jobs within a quarter, employment outcomes are never mutually exclusive.

3.2.4 Summary statistics

A description of our final sample can be found in Table 3.1. Apprentices and temporary contracts are not very dissimilar during the quarters leading to the start of the contract. While apprentices have slightly more work experience, they do not seem to have had higher probabilities to hold open-ended contracts before. Their previous wage levels (conditional on working) are also remarkably similar, indicating that apprentices are not particularly selected compared to workers obtaining temporary contracts. It is nonetheless true that apprentices start their contract approximately one year before. In our main specification we control for age fixed effects to account for these differences, although this makes little difference in the estimated coefficients.

3.3 Estimating returns to apprenticeships

In this paper we define returns to apprenticeship as the extra gain in labor market outcomes an individual obtains from starting an apprenticeship relative to another type of temporary contract that does not oblige the firm to provide training. We employ a dynamic difference-in-differences (DiD) strategy to compare the differential evolution of several labour market outcomes across individuals who start either type of contract. Our identification strategy is valid under a standard parallel-trend assumption i.e. individuals in apprenticeships would have followed the same *trend* as individuals in temporary contracts, had they started one. To corroborate the validity of

[6] In order to treat this case we always include individual×event fixed effects, but cluster standard errors at the individual level.

this assumption, we check whether individuals starting apprenticeships were on different *trends* compared to individuals starting temporary contracts, in the quarters leading up to the start of the job. We find no evidence of underlying pre-trends, which reassures about the validity of our design.

Our unit of analysis is an individual i, whom we follow in the quarters k leading up to, and after an event j. Since the same individual may be present more than once in our data, we cluster standard errors at the individual level, but analyze each event separately and therefore include event-specific fixed effects. We run regressions of the form:

$$\begin{aligned} Y_{ijt} \; &= \alpha_j \; + \; \eta_t \; + \; \theta_a \; + \; \sum_{k=-4}^{23} \beta_k \; \times \; \mathbf{1}(\text{distance}_j = k) \\ &+ \sum_{k=-4}^{23} \beta_k^T \; \times \; \mathbf{1}(\text{distance}_j = k) \; \times \; \text{Apprentice}_j \; + \; \epsilon_{ijt}. \end{aligned} \tag{3.1}$$

where Y_{ijt} is a labor market outcome for individual i, around event j, measured in calendar year $\times$ quarter t; α_j are event fixed effects, which control for any time-invariant unobserved heterogeneity at the worker level when starting either her first apprenticeship or first temporary contract, and η_t are year $\times$ quarter fixed effects, which control for time-varying unobservables that are common across the two groups. We also include age fixed effects (θ_a), in quarters, to control for life-cycle patterns that are common across the two groups. Given that both our treatment and control group are assigned to a job contract at distance time $k = 0$, we include both a set of distance-to-event dummies that are common to both groups i.e. $\mathbf{1}(\text{distance}_j = k)$, and a set of distance-to-event dummies interacted with treatment i.e. $\mathbf{1}(\text{distance}_j = k) \times \text{Apprentice}_j$. This specification is very similar to Jaravel et al. [2018] and addresses the presence of dynamic effects around the start of the contract for both treatment and control group. The resulting coefficients may be interpreted as a tenure profile that is specific to each group.[7]

The coefficients of interest are the β_k^T, for $k \neq -1$. Due to multicollinearity issues we omit both $\mathbf{1}(\text{distance}_j = -1) \times \text{Apprentice}_j$ and $\mathbf{1}(\text{distance}_j = -1)$. All coefficients β_k^T must thus be interpreted as changes in the difference across the two groups relative to any pre-existing difference at distance $k = -1$ (one quarter before event). It follows that $\beta_k^T = \beta_k = 0 \, \forall k < 0$ implies the absence of differential trends in outcome variables before the start of the treatment.

[7]We are not including any other control that is time invariant such as firm characteristics in quarter $k = 0$, as these would be absorbed by the event fixed effects. On the other hand we do not condition on the covariates which vary after the start of the contract because these would constitute a bad control.

3.4 Main Results

3.4.1 Graphical evidence on the returns to apprenticeships

As a first step in describing the kind of variation we exploit in the data, we turn our attention to Figure 3.1. The hollow markers represent the share of individuals who have an open-ended contract, as a function of event time k, for individuals who will start either an apprenticeship (circles) or a temporary contract (diamonds) in event time $k = 0$. The outcome can thus be interpreted as the probability of having an open-ended contract. The two curves evolve parallel in the quarters before the start of the contract, suggesting that our research design is valid. The solid circles instead are corresponding difference-in-differences estimates ($\hat{\beta}_k^T$) from specification 3.1. Associated 95% confidence intervals are also displayed. The graph displays an increase in labor market prospects following the start of either type of contract, as reflected in the higher probability of obtaining an open-ended contract in the quarters after $k = 0$. However, the dynamic evolution of the two paths clearly differs. Compared to temporary contracts, apprenticeships yield a negative short term effect, most likely due to the fact that individuals are locked-in their initial training contract (an "incapacitation effect"), but recover afterwards. The recovery from the negative effects follows a step function with more pronounced jumps at quarters 8, 12 and 16 after the start of the contract. This is reasonable because apprenticeships that are brought to completion have (in the majority of cases) fixed durations that are multiples of one year. We still see departures from the step function because apprenticeships may terminate before due to either of the two parties' willingness to stop.[8] After quarter 16 we see that apprenticeships have 8.5 p.p. higher probability of being converted to open-ended, an effect that remains stable up to six years after the start of the contract.

Given this framework, we now turn to the study of different outcomes. Together with the probability of being converted to open-ended contracts, in Figure 3.2 we overlay estimates for two other outcomes: the probability of having a temporary contract, and the probability of having either of the two, that is the probability of having any job that is not an apprenticeship.[9] When looking at the two other outcomes we see that starting an apprenticeship instead of a temporary contract mechanically causes a sharp drop in both the probability of holding a

[8] By the law, apprenticeships have the same EPL coverage as open-ended contracts. They can only be dismissed under a "just cause" or "justified motive", because of economic or disciplinary reasons respectively. Temporary contracts can only be terminated under a "just cause". However firms can roll the latter over, generating more moments at which firms can terminate the working relationship.

[9] Individuals who have more than one job at the same time or transition from a job type to another within the same quarter will be recorded in the data as having *both* an open-ended and a temporary contract in the same quarter. For this reason the coefficient associated to "employee but not apprentice" is not necessarily equal to the sum of coefficients associated to "open-ended contracts" and "temporary contracts".

temporary contract or having any job that is not an apprenticeship. Over time this effect is gradually reduced for both outcomes, as workers start new spells and transition towards different contractual forms. We see that by the end of the period, apprenticeships induce a decrease in the probability of having temporary contracts of around 13.1 p.p.. Quantitatively this effect is stronger than the positive effect on open-ended contracts first analyzed in Figure 3.1, which is reflected in coefficient associated with the probability of being in any job contract that is not an apprenticeship.

In sum, apprenticeships are indeed associated with higher probability of having an open-ended contract on average, but this comes at the expense of a much lower probability of having a temporary contract, with the second effect dominating. The combination of these forces implies a negative treatment effect of around 4 p.p on the probability of having any job that is not an apprenticeship after a six year period.

3.4.2 Decomposition according to firm mobility patterns

In the previous subsection we highlighted that apprenticeships confer to workers a higher probability of obtaining open-ended contracts and lower probabilities to have temporary ones. In this subsection we investigate where these gains or losses are accrued. It could be that apprenticeships lead to higher conversion rates to open-ended jobs at the training firms but lower probability of obtaining an open-ended contract elsewhere. Similarly, the lower probability of churning among other temporary jobs may be due to the fact that temporary contracts give workers the possibility to be periodically recalled by the same firm, a fact documented in Scrutinio [2019]. In what follows we decompose both the probability of having an open-ended contract and the probability of having a temporary contract in spells at the initial firm and at other firms. Similarly to before, Figures 3.3 and 3.4 plot β_k^T coefficients and associated 95% confidence intervals.

Let us consider Figure 3.3 first. We see that apprenticeships have a positive impact on the probability of being employed under an open-ended contract at the initial firm but a negative effect on the same outcome in other firms. Although the overall effect is positive, the entirety of gains in terms of conversion to open-ended contracts are accrued at the training firm while the probability of obtaining open-ended contracts at other firms contributes negatively to the overall effect. This is consistent both with the accumulation of firm-specific human capital and a high degree of wage compression which limits poaching by competing firms in the post-training period. An apprenticeship increases on average the probability of conversion at the initial firm by 10.6 p.p.

Figure 3.4 has a similar structure and displays DiD estimates for the probability of having a temporary job (solid dots) and a decomposition thereof in the the probability of having it at the initial firm or in other firms. We see that apprenticeships do not miss out on the opportunity of obtaining other temporary contracts at the initial firm. However we see that the majority of the effect is explained by what happens in other firms. This goes against an explanation based on higher recall rates for temporary contracts. Rather, it seems that individuals in temporary contracts become more able to move across different firms with the same contractual form.

3.5 Heterogeneous effects

3.5.1 Effects by firm size

In this subsection we look at whether main results are different depending on the size of the firm where the individual starts the contract.[10] In order to do this we carry out the same analysis as before, separately for big and small firms. We classify a firm as being "big" if its average size is strictly greater than 15 in the solar year when the contract starts, and "small" otherwise.

To summarize results, we report β_{23}^{T} coefficients in bar charts and present the corresponding event study graphs in the Appendix. In Figure 3.5 we look at the probability of being employed under an open-ended contract, a temporary contract or either of the two 23 quarters (including 0) after the start of the contract. We see that the overall probability of having an open-ended contract is not different across the two groups. What differs is the probability of being employed in other temporary contracts. Big firms give a substantial disadvantage in this respect. As a consequence, the overall probability of having a job is negative only in big firms, but not in small firms.

When decomposing the rate of conversion to open-ended contracts in Figure 3.6 we notice two interesting facts. First, big firms convert apprenticeships to permanent positions at a much higher rate than small firms. The effect in small firms is 9 p.p. while the one in big firms is 15 p.p, a 66% increase. Secondly, small firms produce higher rates of conversions to open-ended contracts in other firms. The same is not true for big firms, as they have a negative impact on the probability of obtaining permanent position in other firms. When looking at the overall effect, these two mechanism compensate each other: apprenticeships in both types of firms are associated an increase in the probability of having an open-ended contract by 12 p.p..

We perform a very similar exercise for the probability of being employed under temporary

[10]We performed an heterogeneity analysis also based on gender and found identical results for men and women. Results are available upon request.

contracts. Results are displayed in Figure 3.7. We see that the qualitative pattern this time is very similar in both small and big firms. Apprenticeships unambiguously decrease the probability of churning in other temporary contracts, especially in firms other than the initial one. In small firms, the lack of other temporary contracts outside the initial firms accounts for about 90% of the overall impact, while the same figure is 94% for big firms.

3.6 Other results

3.6.1 Self-employment effects

Self-employment is very diffused in Italy and constitutes around 20% of the workforce, way above the European average [Istat, 2017].[11] It is therefore interesting to check whether apprenticeships contribute positively or negatively towards the individual choice of entering self-employment. From an economic standpoint, the direction of the effect is ambiguous. On the one hand apprenticeships increase the conversion rates at the initial firm, as firms train workers to keep them and extract rents from their accumulated human capital. On the other hand apprenticeships may want to learn a trade to establish their own entrepreneurial activity.

In Figure 3.8 we study three outcomes: the probability of working, the probability of being an employee and the probability of being self-employed. As described in previous sections, apprenticeships have a negative impact on the probability of being employees. Here we found that this is not compensated by the self-employment margin. To the contrary, apprenticeships have a negative impact on the probability of being self-employed. Despite being statistically significant, this effect is quantitatively small, in the order of magnitude of 1 p.p..

3.6.2 Wage effects

In Figure 3.9 we study the impact of apprenticeships on wages. Our dependent variable is now the log of quarterly earnings, conditional on working status. Our data does not record earnings at the quarterly frequency, but we still have information on the total amount of earnings received in a given year, separately by job characteristics and employer, in addition to detailed information on which exact months of the year these income flows refer to. In order to construct our measure of quarterly earnings we therefore apportion job-spell earnings to quarters based on the proportion of months accounted for by any given spell.[12]

[11] Our definition of self-employed includes both freelancers (*libero professionista*), entrepreneurs (*titolare d'impresa*) and their collaborators (*coadiutore d'impresa*).

[12] Notice that our measure is imprecise only insofar a worker can receive a pay rise that is not also reflected in a job-title change. If instead a worker receives a pay rise but is also promoted from blue

Given that we established that apprenticeships have an impact on the overall probability of employment, our wage results ought to be interpreted with care. Conditional on having a job, we see that apprenticeships are associated with substantial wage gains. However the effects fade over time and are not statistically distinguishable from zero at the very last quarter of our observation period.

3.7 Discussion

The main result in this paper is that on average apprenticeships can ease workers' transition towards open-ended contracts, but to the expense of fewer positions in other temporary contracts. The two effects do not mechanically cancel out: quantitatively, the second effect dominates, generating a negative impact on the probability of having any job. In this sense, apprenticeships seem to constitute a double-edged sword, because they allow workers to climb higher rungs on the job ladder but lead to higher penalties when conversion to open-ended does not happen.

There are different theoretical mechanisms that can rationalize these findings. The first possibility is that apprenticeships are more accurate screening devices for individual ability than are temporary contracts. Within the training firm, employers may learn workers' types precisely, thanks to higher monitoring and more frequent interactions. Other firms in the markets will then also have access to part of this private information, by observing apprentices' retention choice (or lack thereof). An apprenticeship that is not converted to an open-ended contract reveals the presence of a lower productivity type. To the contrary, temporary contracts are not as precise screening devices as apprenticeships. While the initial firm may still learn a lot about worker types during this period, temporary contract may fail to be renewed because of exogenous reasons with higher probability, and therefore should lead to a weaker updating by the other firms in the market.

The second possibility is that apprentices acquire firm-specific skills that are not easily re-usable at other employers. To the contrary tasks performed in temporary contracts may be more standardized. Even here, dismissals after apprenticeships should lead to a penalty in the labor market, as time was “wasted" learning things not valued elsewhere. This would be consistent with recent evidence showing that apprenticeships may generate specific skills and scarce adaptability to new environments [Hanushek et al., 2017]. The two stories are not necessarily mutually exclusive, and disentangling the two is left for future research.

collar to white collar, we would observe two earning records

3.8 Conclusions

In this paper we have analyzed the returns to apprenticeships by looking at a variety of labor market outcomes. In terms of conversion to open-ended contracts, apprenticeships are dominated by temporary contracts in the first three years after the start of the contract, but guarantee higher conversion rates afterwards, by about 8.5 p.p.. All of these extra conversions happen at the initial firm, while conversions to open-ended in other firms negatively contribute to the overall effect. While they increase the probability of accessing better jobs, they decrease the probability of obtaining further temporary contracts. This second effect is bigger (-13.1 p.p.) and negatively impacts the probability of having any job. We find transitions to self-employment not to be an important margin of adjustment in this context. Taken together, our results highlight a trade-off between the quality and the quantity of job offers that could result after starting an apprenticeship.

Tables

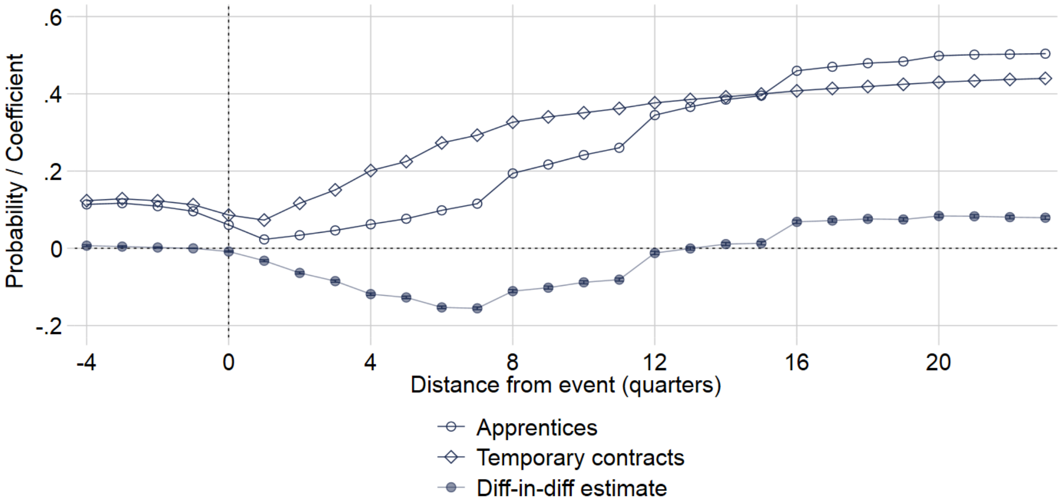
Table 3.1: Summary statistics

	Apprentices		Temporary contracts	
Variable (pre-event average)	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Prob. of having any job	0.230	0.421	0.147	0.354
Prob. of being an employee	0.202	0.402	0.118	0.323
Prob. of being a blue collar	0.121	0.327	0.080	0.271
Prob. of being a white collar	0.084	0.278	0.039	0.193
Prob. having open-ended contract	0.096	0.295	0.114	0.318
Prob. having temporary contract	0.109	0.311	0	0
Age at start of spell (years)	24.098	1.978	25.01	2.139
Average monthly earnings (euros)	1250.82	610.614	1232.238	654.895
Number of spells	100,547		179,528	

Notes: This table provides descriptive statistics for our main sample. All variables are measured as an average of the four quarters before the start of the contract. All employment outcomes are dummy variables that take value one if the condition is true for at least one month during the quarter. As a consequence outcomes are never mutually exclusive. The probability of having any job includes both employment, dependent self-employment and self-employment. Average quarterly earnings is expressed in 2017 euros and winsorized at the 1st and 99th percentile. It includes all earnings from either employment and dependent self-employment. Earnings from self-employment are not included.

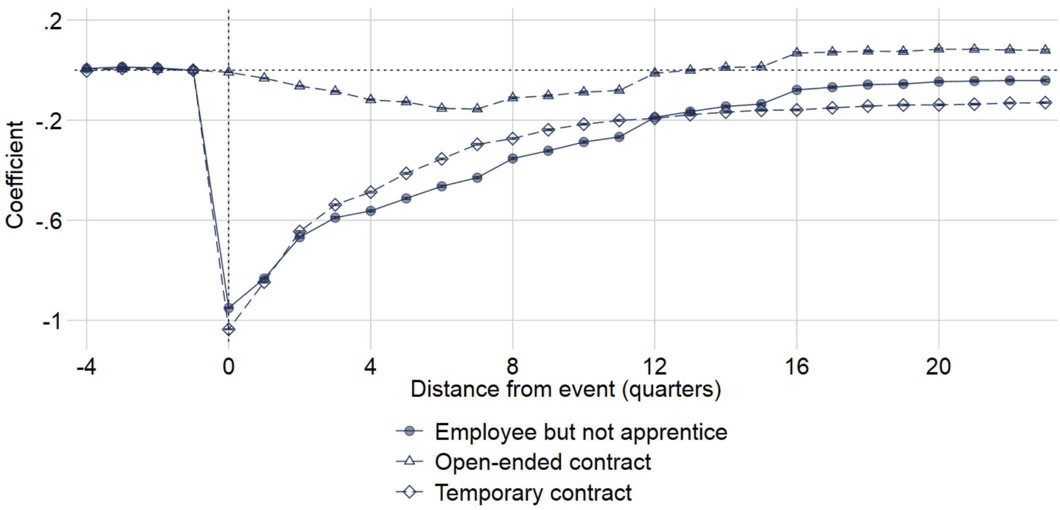
Figures

Figure 3.1: Probability of being an open-ended contract



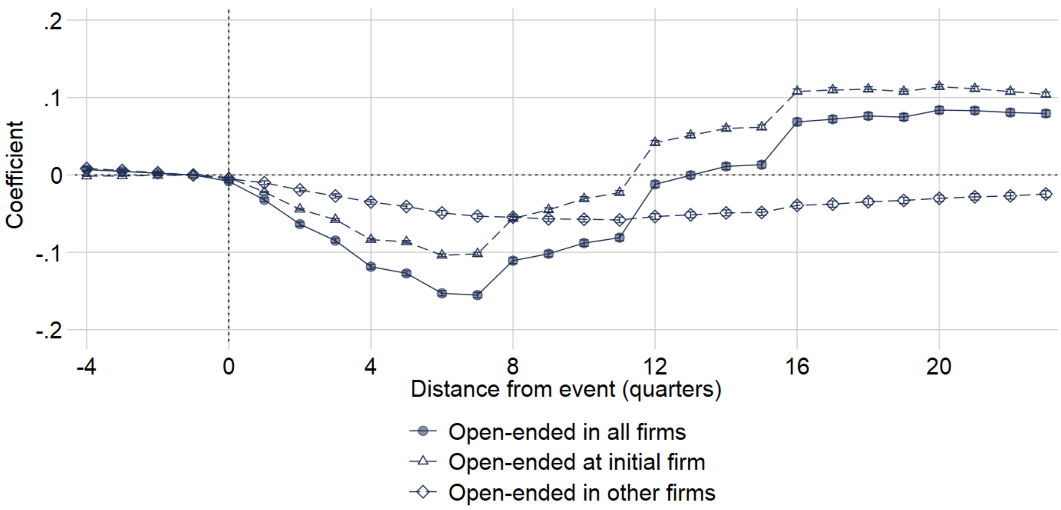
Note: The figure plots the dynamic evolution of the mean probability of being in an open-ended contract, for apprentices and individuals in temporary contracts (hollow circles and diamonds respectively). Solid blue circles indicate difference-in-differences estimates (β_k^T) from specification 3.1. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.2: Probability of being in temporary or open-ended contracts



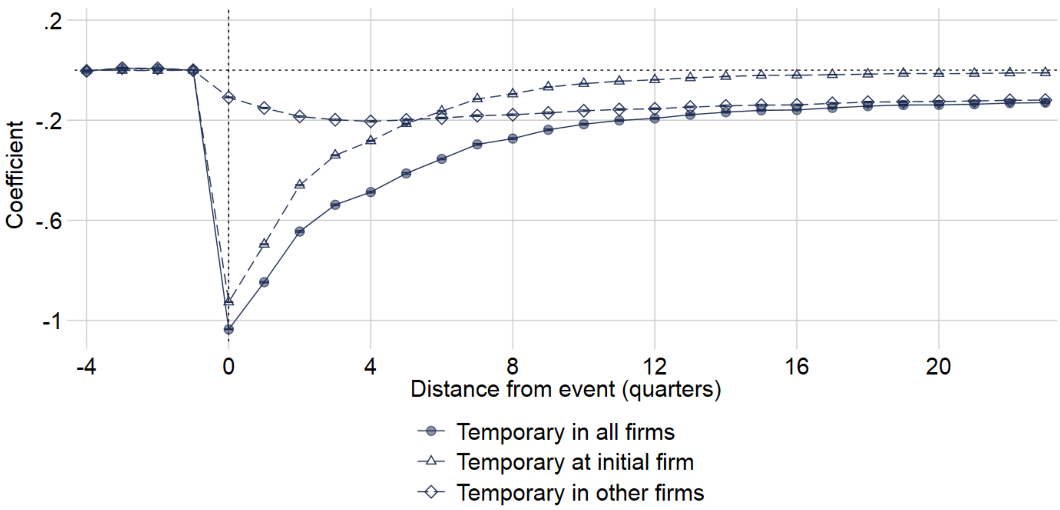
Note: The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under an open-ended contract, the probability of being employed under a temporary contract and the probability of being employed except for apprenticeship contracts. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.3: Probability of being in open-ended contracts at initial or other firms



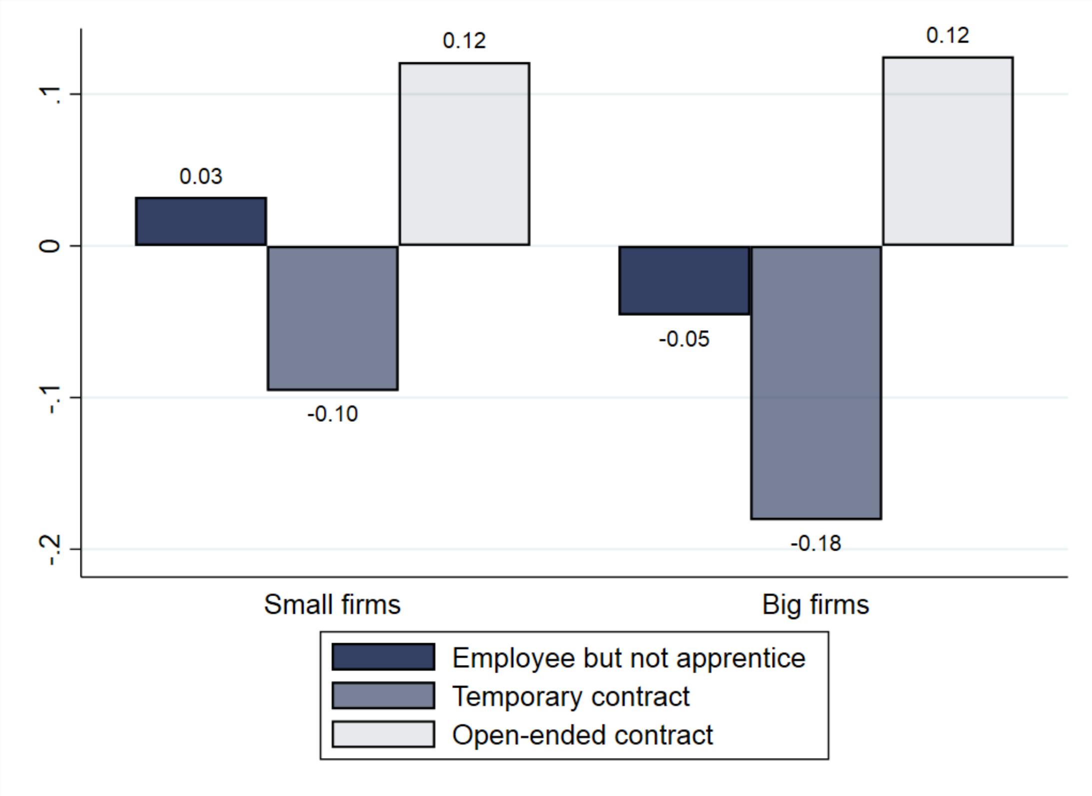
Note: The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under an open-ended contract, the probability of being employed under an open-ended contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under an open-ended contract in firms other than the firm where the contract started. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.4: Probability of being in temporary contracts at initial or other firms



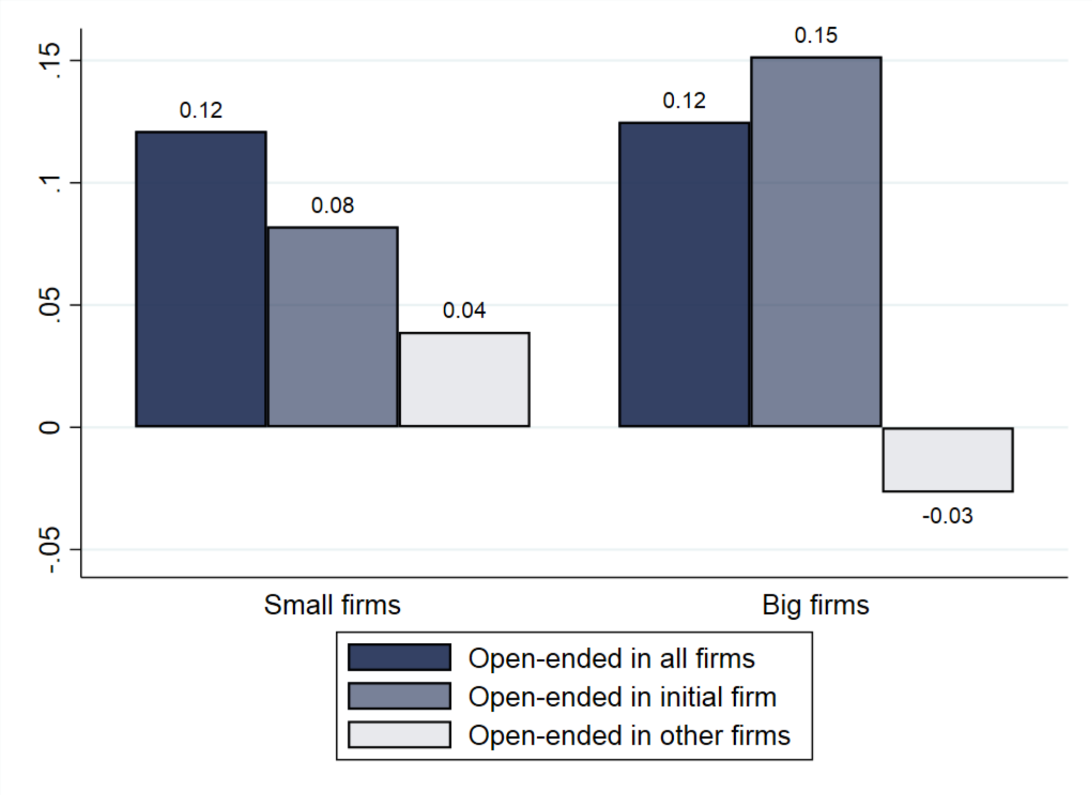
Note: The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under a temporary contract, the probability of being employed under a temporary contract contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under a temporary contract in firms other than the firm where the contract started. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.5: Probability of being in temporary or open-ended contracts by firm size



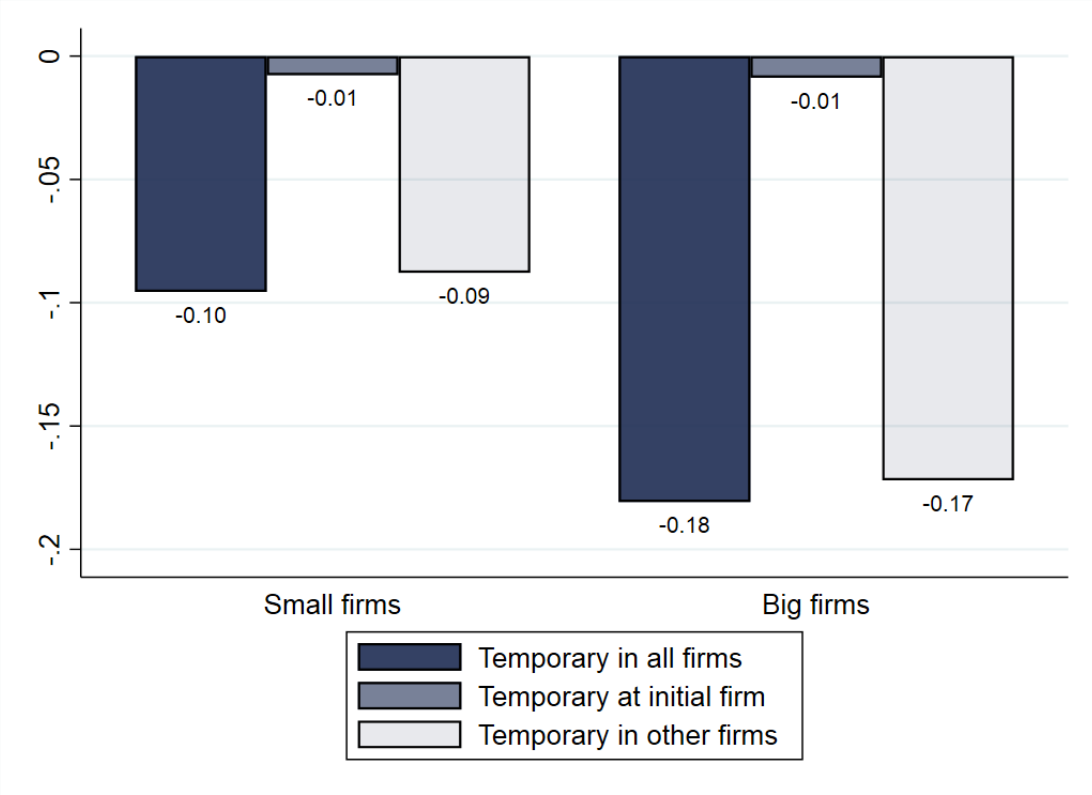
Note: The figure plots β_k^T coefficients from specification 3.1 for $k = 23$ only, run separately for contracts started in small firms and big firms. A firm is defined as big if its average size in the solar year when the contract starts is strictly greater than 15 and small otherwise. Three outcomes are displayed: the probability of being employed under an open-ended contract, the probability of being employed under a temporary contract and the probability of being employed except for apprenticeship contracts. The latter constitutes the union of the former two events. Standard errors for the difference-in-differences estimates are clustered at the individual level. Confidence intervals are not displayed, but estimates are always significant at the 1% level.

Figure 3.6: Probability of being in open-ended contracts at initial or other firms by firm size



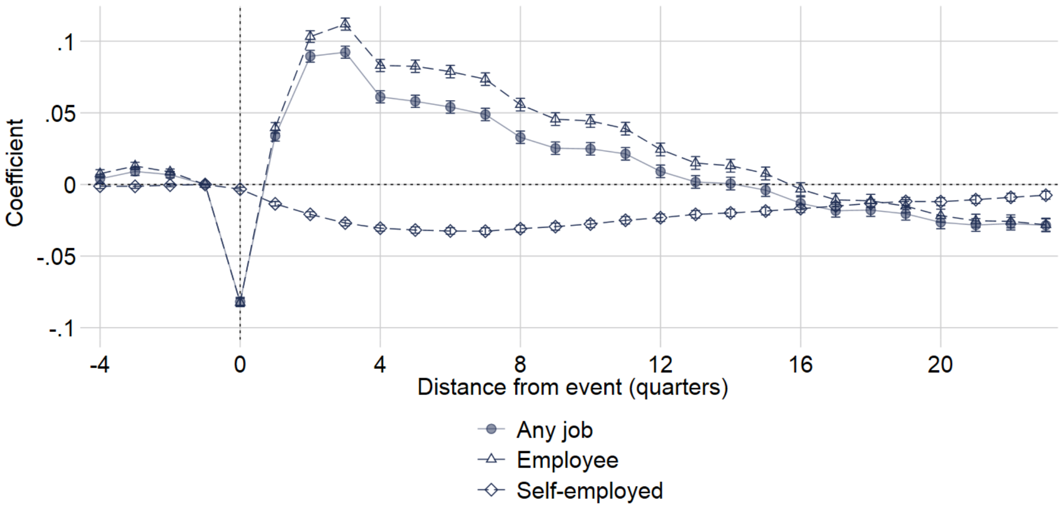
Note: The figure plots β_k^T coefficients from specification 3.1 for $k = 23$ only, run separately for contracts started in small firms and big firms. A firm is defined as big if its average size in the solar year when the contract starts is strictly greater than 15 and small otherwise. Three outcomes are displayed: the probability of being employed under an open-ended contract, the probability of being employed under an open-ended contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under an open-ended contract in firms other than the firm where the contract started. Standard errors for the difference-in-differences estimates are clustered at the individual level. Confidence intervals are not displayed, but estimates are always significant at the 1% level.

Figure 3.7: Probability of being in temporary contracts at initial or other firms by firm size



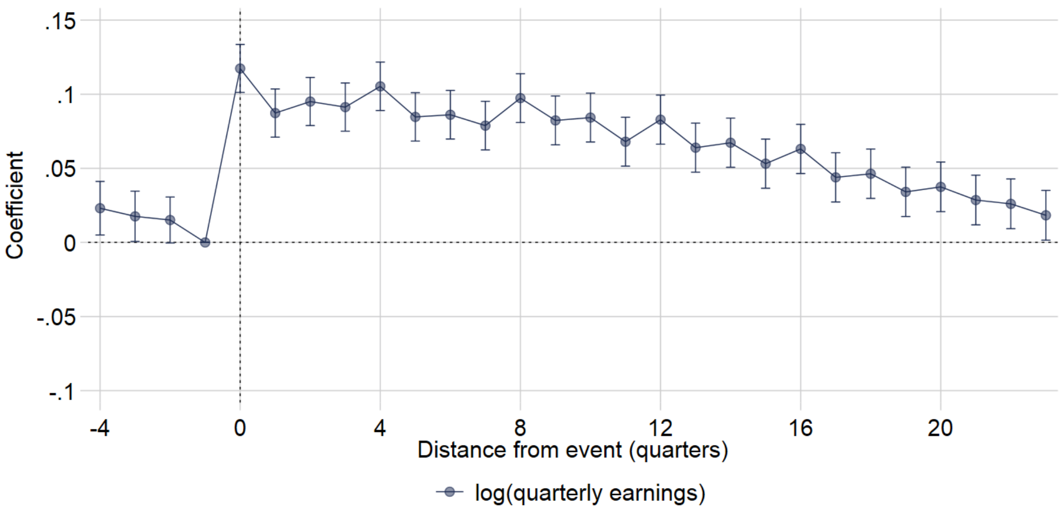
Note: The figure plots β_k^T coefficients from specification 3.1 for $k = 23$ only, run separately for contracts started in small firms and big firms. A firm is defined as big if its average size in the solar year when the contract starts is strictly greater than 15 and small otherwise. Three outcomes are displayed: the probability of being employed under a temporary contract, the probability of being employed under a temporary contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under a temporary contract in firms other than the firm where the contract started. Standard errors for the difference-in-differences estimates are clustered at the individual level. Confidence intervals are not displayed, but estimates are always significant at the 1% level.

Figure 3.8: Employment and self-employment



Note: The figure plots β_k^T coefficients from specification 3.1 for different outcomes. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Given that mechanically both groups have Pr(any job$= 1|k = 0) = 1$, the point estimate at $k = 0$ equals the level difference that exists between the two groups at $k = -1$. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.9: Log(quarterly earnings)

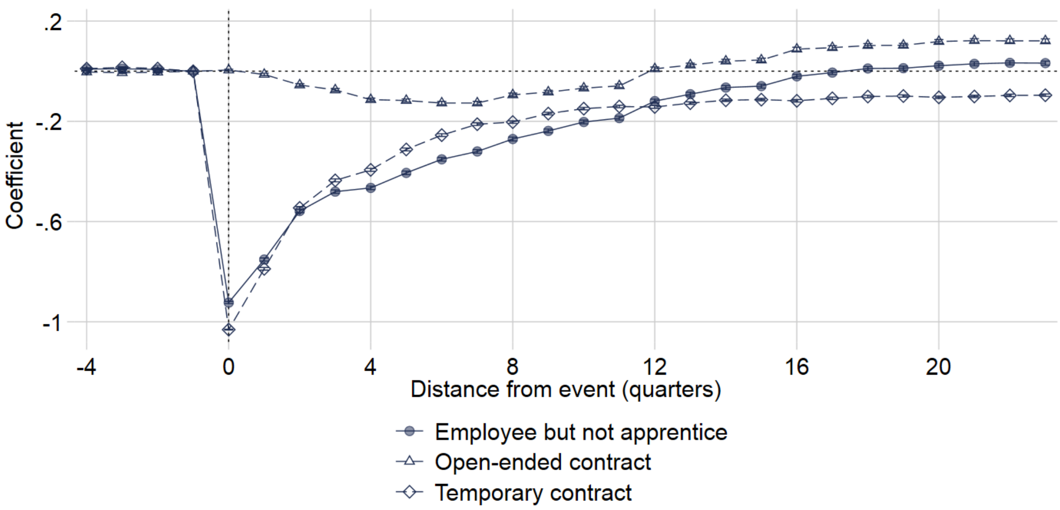


Note: The figure plots β_k^T coefficients from specification 3.1. The dependent variable is the natural logarithm of quarterly earnings, conditional on working status. Earnings include both labor income from employment and dependent self-employment. We have no reliable information on earnings as self-employed. Quarterly earnings are constructed by apportioning yearly earning amounts to quarters in proportion to the number of months spent in a given spell. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Appendices

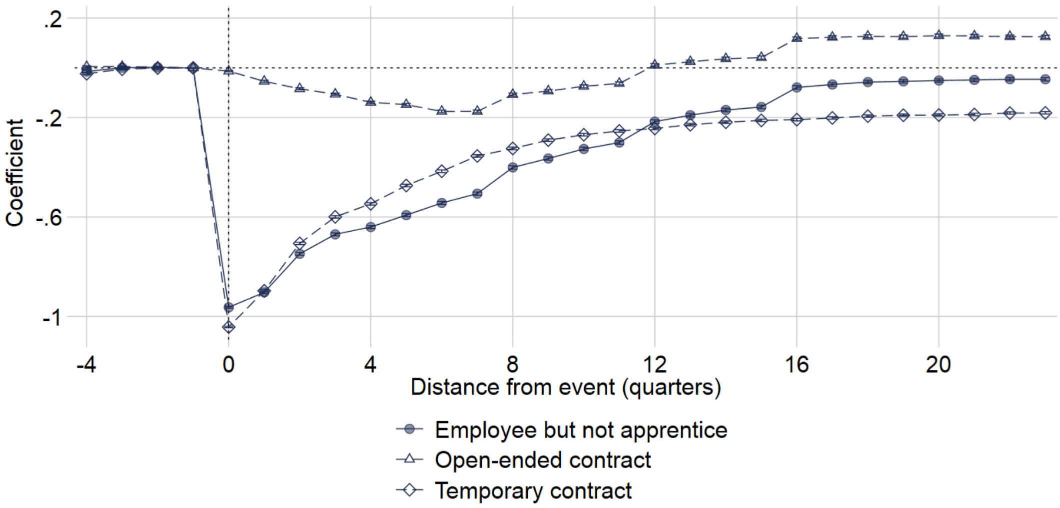
3.A Additional Tables and Figures

Figure 3.A.1: Probability of being in temporary or open-ended contracts (small firms)



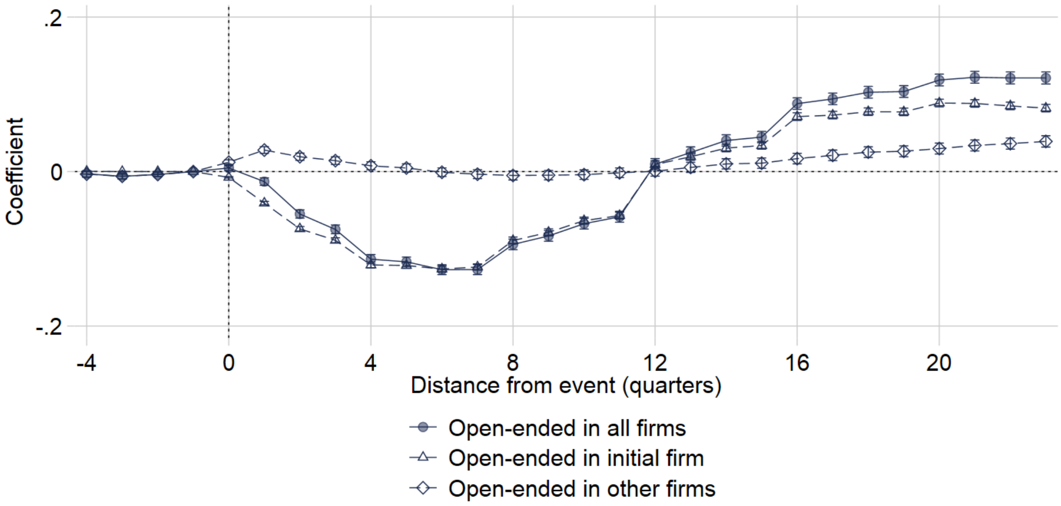
The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under an open-ended contract, the probability of being employed under a temporary contract and the probability of being employed except for apprenticeship contracts. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.A.2: Probability of being in temporary or open-ended contracts (big firms)



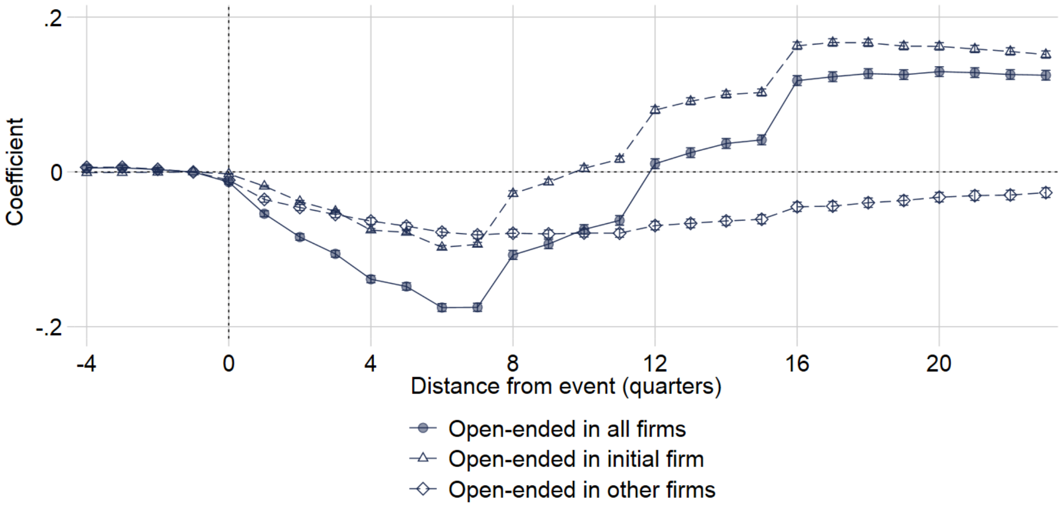
Note: The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under an open-ended contract, the probability of being employed under a temporary contract and the probability of being employed except for apprenticeship contracts. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.A.3: Probability of being in open-ended contracts at initial or other firms (small firms)



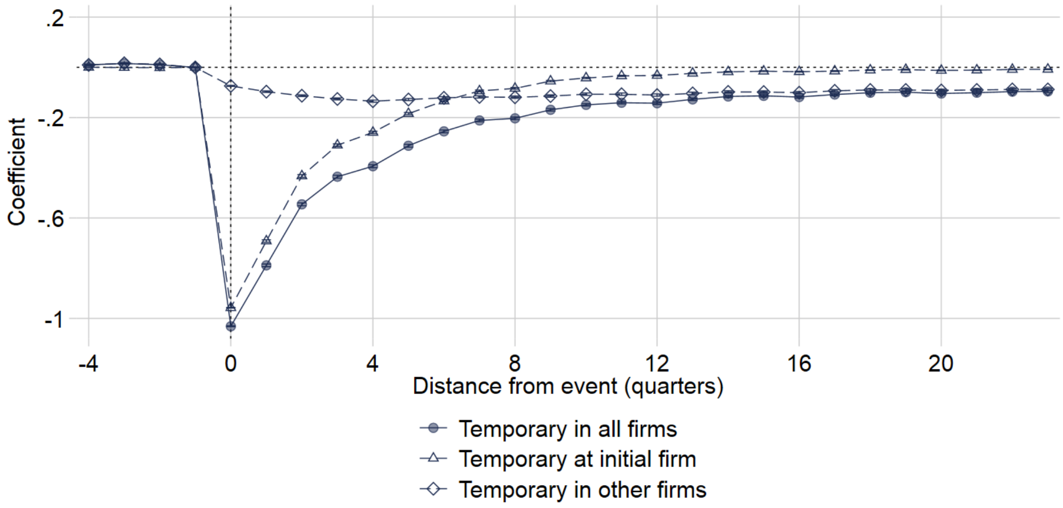
The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under an open-ended contract, the probability of being employed under an open-ended contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under an open-ended contract in firms other than the firm where the contract started. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.A.4: Probability of being in open-ended contracts at initial or other firms (big firms)



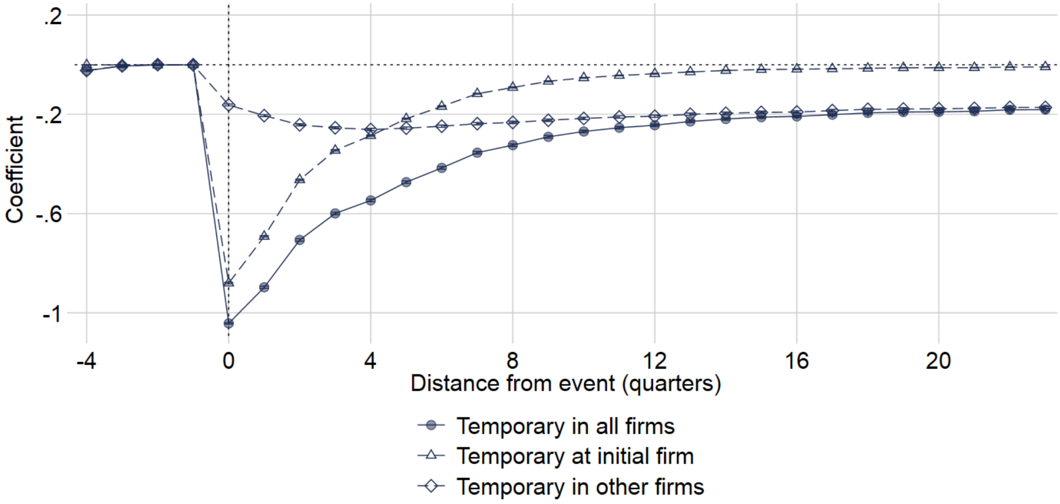
The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under an open-ended contract, the probability of being employed under an open-ended contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under an open-ended contract in firms other than the firm where the contract started. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.A.5: Probability of being in temporary contracts at initial or other firms (small firms)



The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under a temporary contract, the probability of being employed under a temporary contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under a temporary contract in firms other than the firm where the contract started. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

Figure 3.A.6: Probability of being in open-ended contracts at initial or other firms (big firms)



The figure plots β_k^T coefficients from specification 3.1 for three outcomes: the probability of being employed under a temporary contract, the probability of being employed under a temporary contract at the same firm where the contract is started ($k = 0$) and the probability of being employed under a temporary contract in firms other than the firm where the contract started. The latter constitutes the union of the former two events. The difference at event time $k = -1$ is normalized at zero. Event time $k = 0$ corresponds to the quarter when both the apprenticeship and the temporary contract start. Standard errors for the difference-in-differences estimates are clustered at the individual level and corresponding 95% confidence intervals are displayed.

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