Locked In by Generosity: Place-Based Unemployment Insurance and the Spatial Allocation of Labor*

Laura Montenovo† Long Hong‡ Salvatore Lattanzio§

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Abstract

Geographically varying unemployment insurance (UI) generosity may distort where unemployed workers search. We exploit an Italian reform that equalized potential benefit duration (PBD) across regions by cutting it in the South. Using a difference-in-differences design, we find that shorter PBD reduces days on benefits and of non-employment. It also increases internal migration: a 12-month PBD reduction for workers dismissed in the South makes them 5.5 percentage points more likely to be reemployed in the higher-employment Center-North. The migration response is concentrated among high-ability workers and when destination labor demand is strong. Place-based generosity may lock workers into weaker labor markets.

Keywords: unemployment insurance duration; labor mobility; job reallocation; migration

***JEL* codes:** J61; J63; J64; H55; R23

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†Corresponding Author. Agricultural Economics Department, Purdue University, West Lafayette, Indiana 47907. Email: **lmonteno@purdue.edu**. Phone: +1-812-606-5105.

‡W. P. Carey School of Business, Arizona State University. 300 E Lemon St, Tempe, Arizona 85281. Email: **long.hong@asu.edu**.

§Bank of Italy, RFBerlin and Dondena. Via Nazionale 91, 00184 Rome, Italy. Email: **salvatore.lattanzio@bancaditalia.it**

1 Introduction

Regional differences in employment rates within countries are large and persistent (Overman and Puga, 2002; Kline and Moretti, 2013; Amior and Manning, 2018), yet many unemployed workers remain in weaker labor markets rather than relocating to higher-employment regions. Economists have traditionally attributed the limited mobility to moving costs, housing, family ties, and information frictions (Sjaastad, 1962; Kennan and Walker, 2011; Molloy et al., 2011). Because migration is one of the main channels through which economies absorb regional shocks, frictions that discourage movement toward stronger labor markets can slow regional convergence and distort the spatial allocation of labor (Moretti, 2024). This paper studies a further explanation for that limited mobility, one that has received far less attention: geographically differentiated social insurance.

Unemployment insurance (UI) is not always uniform within countries: it can grant longer or higher benefits in economically distressed areas, effectively embedding a place-based policy inside a national insurance program (Kline and Moretti, 2013; Bartik, 2020b; Austin et al., 2018). For example, the U.S. Extended Benefits program lengthens benefit duration in states with high unemployment, and Canada's Employment Insurance varies entitlement with the regional unemployment rate (Day and Winer, 2011). This design creates a tension that the UI literature has largely left unexplored. The canonical trade-off in UI is between insurance and search incentives: more generous benefits improve consumption smoothing but reduce the incentive to find a job quickly (Chetty, 2008). When generosity varies across space, however, the distorted margin is not only how hard unemployed workers search but also where: benefits that are more generous where jobs are scarce raise the value of remaining unemployed in weak labor markets, dampening incentives to relocate toward stronger ones. Place-based social insurance may thereby perpetuate the very regional disparities it is designed to offset (Tatsiramos, 2009; Nunn et al., 2018; Bartik, 2020a). This paper answers two questions: whether this spatial distortion is empirically relevant, and whether removing it pushes workers into worse jobs.

Italy offers a rare opportunity to answer these questions. Its North–South economic divide is among the starkest and most persistent regional disparities in the OECD.[1] Because wages in Italy are set by nationwide contracts with limited local wage adjustments, employment is lower in the South, where productivity is lower, and unemployed workers there queue for jobs and wait (Boeri et al., 2021). For a quarter century, one Italian social insurance scheme explicitly adjusted for this divide: *Mobilità*, the mass-layoff UI program

[1] GDP per capita in the Center-North exceeds that of the South by over €14,700, and the employment rate among workers aged 25–34 is consistently nearly 30 percentage points higher in the North.

we study, granted longer potential benefit durations (PBD) to workers dismissed in the South and to older workers. The Fornero Reform[2] progressively dismantled these regional differences in 2015 and 2016, equalizing PBD across macro-areas for workers under 40 and narrowing geographic PBD gaps for older workers, without changing benefit levels or formulas. For workers under 40, the reform thus removed the place-based component of the program.

The reform generated quasi-experimental variation in UI generosity, which we exploit with a difference-in-differences (DiD) design comparing *Mobilità* recipients dismissed in the South with those dismissed in the Center-North, before and after the reform. Because the reform reduced PBD by different amounts across age groups, we estimate separate models by age group and focus primarily on workers under 40. For this group, the reform provides the cleanest variation: it fully equalized PBD across macro-areas, cutting it in the South while leaving it unchanged in the Center-North. Workers dismissed in the South are therefore the treated group, and those dismissed in the Center-North serve as controls. For older workers, the reform generated dose–response rather than treatment–control variation, and we report their results separately.

We implement this design using linked employer–employee administrative records from the Italian Social Security Institute (INPS) for 2011–2019. We identify *Mobilità* recipients in the program's benefit archive and link them to their pre-layoff jobs, restricting attention to the program's core target population of workers on permanent, non-managerial contracts. For each worker, we track four sets of outcomes over the three years following layoff: (i) actual benefit receipt; (ii) non-employment duration; (iii) geographic mobility, measured as reemployment in a different macro-area or in a region with a higher employment rate than the region of layoff; and (iv) post-layoff job match quality, including wages, firm wage premia, sector switching, and employment stability. We also convert benefit receipt into euros using the program's statutory schedule and measure post-layoff earnings, which allows us to quantify the reform's budgetary savings and its effect on workers' incomes. We exploit within-firm variation in PBD by including firm-of-layoff and layoff-year fixed effects, so that identification comes from differential changes in outcomes across dismissal cohorts of the same firm, rather than from time-invariant differences across firms or macro-areas.

To guide the empirical analysis, we develop a two-region random search model in which regions differ in search frictions and wage distributions. Longer UI duration in the low-employment South raises the value of remaining unemployed and hence reservation wages, dampening both the job-finding hazard and the incentive to relocate to the stronger

[2]Law 92/2012.

labor markets of the Center-North. In the model, place-based generosity thus acts as a spatial wedge: it raises the reservation wage that workers in the South apply to Center-North offers, keeping them in the region where offers are fewer and worse. Equalizing PBD lowers the value of unemployment in the South, yielding three testable predictions: (1) shorter non-employment duration, (2) higher out-migration to the Center-North, and (3) a theoretically ambiguous effect on job match quality, since faster search may worsen matches while access to higher-employment regions may improve them. Two extensions sharpen these predictions: because high-ability workers are more likely to draw attractive Center-North offers, the migration response should concentrate among them; and because workers can direct their search effort across regions, the response should be amplified where Center-North offers are plentiful. Our empirical results map directly onto these predictions and speak to the compensating forces at work on the ambiguous match-quality margin.

We first document the reform's direct effect: shorter PBD substantially lowers actual benefit receipt, by about 0.54 days for each one-day cut in PBD among workers younger than 40. We then show that lower UI generosity speeds reemployment: a 12-month reduction in PBD reduces non-employment duration by approximately 150 days (about 0.4 days per one-day cut in PBD) among workers displaced in the South, consistent with the model's first prediction. At least for the 2015 cohort, the acceleration operates through reemployment into both permanent and temporary jobs, rather than through a shift toward temporary contracts.

Our central finding, in line with the model's second prediction, is that the reform significantly increases geographic mobility toward stronger labor markets.[3] Among workers younger than 40, the 12-month PBD reduction raises the probability of reemployment in a different macro-area by 5.5 percentage points for workers dismissed in the South, relative to those dismissed in the Center-North. That corresponds to about 70 percent of the pre-reform migration rate. The response has two features that tie closely to the model. First, the aggregate effect is mainly driven by South-to-Center-North moves, and estimates are similar when the outcome is defined as reemployment in a region with a higher employment rate than the region of layoff: workers sort toward stronger labor markets rather than simply moving across macro-area boundaries. Second, high-ability workers, measured by worker fixed effects from an AKM wage decomposition, exhibit a large and significant migration response, while low-ability workers show none, although the difference between the two groups is imprecisely estimated. This is consistent with the

[3]We measure migration as reemployment in a different macro-area, i.e., a change in work location. Section 6.5 shows that the response is not simply commuting across macro-area borders.

model's prediction that high-ability workers are most likely to draw Center-North offers that justify the moving cost.

The migration response occurs among workers laid off in 2015 and is muted for the 2016 cohort. This attenuation is unlikely to reflect a fading incentive: the geographic PBD gap remained closed, and benefit receipt and non-employment duration continued to fall for the 2016 cohort. Suggestive evidence points instead to a role for destination labor demand. We build measures of workers' *ex ante* exposure to Center-North demand from their pre-layoff region and sector and the pre-reform mobility flows of similar workers. For the 2015 cohort, the response is positive across exposure groups, whereas for the 2016 cohort it remains positive only for workers with high predicted demand. These differences are imprecisely estimated, but the pattern is consistent with the second extension of our model: lower benefits make workers more willing to move, but whether they actually move depends on demand at the destination. Reduced UI generosity thus appears to promote geographic reallocation where destination demand is strong.

Next, we turn to job match quality, the margin on which the model's predictions are ambiguous, and find that the empirical evidence is mixed. Treated workers accumulate about 0.25 additional years of employment over the three years after displacement and are *less* likely to be working in a different sector, consistent with the wider range of opportunities that higher-employment regions offer within a worker's original sector. However, monetary outcomes point to some decline in job match quality. Treated workers experience no significant change in nominal pay, but they are 5.7 percentage points less likely to be at a firm with a higher wage premium than the one that dismissed them, an estimate we interpret with caution given pre-reform differences.

A simple accounting exercise reveals that the reform generated a sizable fiscal saving without a measurable earnings cost to displaced workers. It reduced *Mobilità* payments by about €5,200 per treated worker over three years, equal to two-thirds of what a comparable Center-North worker received, while post-layoff earnings were, if anything, higher. However, because the earnings gain does not fully offset the benefit loss, workers' combined income was lower on net over the three-year window.

We subject the migration result to a battery of validation exercises. First, the reform does not change *who* ends up in the treated and control groups: applying our design to characteristics measured two years *before* the layoff, we find no differences in workers' pre-layoff earnings, employment history, or contract type, nor in the size, age, or wage premium of their firms. Second, a placebo using workers on ordinary unemployment insurance—whose benefit duration was untouched by the reform—shows no analogous migration or employment response once a pre-existing trend in relocation is accounted

for, ruling out confounding shocks common to all UI claimants, including the concurrent 2015 national labor-market reforms. Third, because our mobility measure is based on the location of reemployment, we distinguish genuine relocation from commuting across macro-area borders. The response is largest between bordering macro-areas where some reemployment across the border may reflect commuting rather than moves, yet it remains close to the baseline for non-bordering regions, where commuting is infeasible. Decomposing moves by province further shows that the response is concentrated almost entirely between macro-areas. Fourth, the effect is neither an artifact of the enrollment and firing-cost rules specific to large firms nor sensitive to how the estimation sample is constructed or to whether outcomes are measured cumulatively or year by year.

Finally, we exploit the reform's age-graded structure. For older workers, PBD decreased in both years, and the reform narrowed but did not eliminate the geographic PBD gap. Benefit receipt tracks the geographic gap by age group and by year: for workers aged 40–49 the gap halves in 2015, while for those aged 50 or older it halves only in 2016. The responses to the reform are correspondingly smaller. For workers aged 40–49, the migration response is about half that of younger workers, and for workers aged 50 or older it is absent. This likely reflects both the smaller change in the gap and standard life-cycle migration patterns.

Our paper makes three contributions. First, we provide, to our knowledge, the first quasi-experimental evidence that reducing regional disparities in UI generosity increases migration toward higher-employment areas. A long-standing literature asks whether welfare and unemployment benefits deter migration, but identification has been challenging because jurisdictions typically set their own benefit generosity in response to the same labor market conditions that drive migration, and findings are correspondingly mixed (Enchautegui, 1997; McKinnish, 2005; De Giorgi and Pellizzari, 2009; Tatsiramos, 2009; Day and Winer, 2011; Nunn et al., 2018). The Fornero Reform overcomes this identification problem: regional differences in PBD, though linked to the South's persistently weaker labor market, were set by national law rather than by regions, and the reform removed them on a predetermined schedule for reasons unrelated to contemporaneous regional conditions. Our results also complement Kroft and Notowidigdo (2016), who show that the moral hazard cost of UI is lower when unemployment is high. We document a distinct margin of moral hazard that is spatial rather than cyclical: remaining in a weak labor market rather than searching in a strong one. Their analysis abstracts from this margin, which we show is quantitatively important where labor markets are weak.

Second, we contribute to the literature on UI generosity and labor market outcomes (Meyer, 1990; Gruber, 1997; Card et al., 2007; Krueger and Mueller, 2010; DellaVigna et

al., 2017; Ganong and Noel, 2019; Marinescu and Skandalis, 2021), and especially to the quasi-experimental work on PBD (see Schmieder and von Wachter, 2016, for a review), including Lalive et al. (2006), Lalive (2008), Johnston and Mas (2018), Karahan et al. (2025), and Hartung et al. (2026), and to evaluations of large-scale social insurance programs in Italian administrative data (Giupponi and Landais, 2023). Relative to this work, both the policy and the response are spatial in our setting: benefit duration varies across macro-areas, and workers respond to cuts not only by searching harder but also by searching elsewhere. This changes the trade-off of shorter benefits. In the standard framework, faster reemployment may come at the cost of lower-quality matches (Nekoei and Weber, 2017). In our setting, however, reallocation toward stronger labor markets may offset this cost. We formalize this mechanism in a two-region search model whose predictions organize the empirical analysis.

Third, we contribute to the literature on place-based policies (Kline and Moretti, 2013; Busso et al., 2013; Ehrlich and Seidel, 2018; Lu et al., 2019; Moretti, 2024; Ciani et al., 2025; Incoronato and Lattanzio, 2024). That literature has focused mainly on demand-side interventions, such as subsidies to firms, infrastructure, enterprise zones, and geographically targeted fiscal transfers (Corbi et al., 2019), and on their capitalization and displacement effects. We study a supply-side counterpart: social insurance that is more generous in distressed areas. We show that this form of place-based support generates an unintended distortion, locking unemployed workers into weaker labor markets, and that removing it encourages relocation toward higher-employment areas without reducing post-layoff nominal pay, employment, or sectoral attachment. The withdrawal also generated a sizable fiscal saving. Our results therefore speak directly to whether redistribution toward distressed places should operate through insurance generosity, which distorts location incentives precisely when workers are most mobile, or through instruments that do not discourage workers from moving. Since we do not measure the insurance value of the benefits withdrawn, this is an allocative rather than a welfare conclusion.

The remainder of the paper proceeds as follows. Section 2 describes the *Mobilità* program and the Fornero Reform. Section 3 develops the two-region search model and derives its predictions. Section 4 describes the data and sample construction. Section 5 presents the empirical strategy. Section 6 reports the results on benefit receipt, non-employment, migration, job match quality, the policy accounting, the robustness analysis, and the results for older workers. Section 7 concludes.

2 Institutional Background

This section describes the *Mobilità* program and the reform that generates our identifying variation. The reform had two institutional features that are key in our setting. First, benefit levels and replacement rates were set by national formulas that the reform left unchanged. Second, PBD varied only by age and macro-area of dismissal, precisely the two dimensions along which the reform then narrowed or eliminated those differences.

2.1 Unemployment Insurance in Italy and the *Mobilità* Program

Between 1991 and 2016, Italy operated two parallel unemployment insurance systems, distinguished by the type of job loss. Individual dismissals were covered by ordinary UI, whereas collective layoffs could qualify workers for the *Indennità di Mobilità* (hereafter *Mobilità*), the program we study. Collective dismissals were defined by statute: a firm with more than fifteen employees that intended to dismiss at least five workers within 120 days, following a reduction or transformation of its activity, was required to open a collective dismissal procedure.[4] These were therefore layoffs arising from firm restructuring, plant closures, or broader economic shocks. Firms could not freely choose which workers to dismiss: selection followed criteria agreed with the unions or, absent an agreement, family responsibilities, seniority, and the firm's organizational needs.[5] These rules were set nationally and were not changed by the Fornero Reform. Eligibility for *Mobilità* was restricted to workers on open-ended contracts (*contratto a tempo indeterminato*) with at least twelve months of tenure at the dismissing firm, of which at least six months of effective work.[6]

The *Mobilità* program comprised three components: unemployment benefits for displaced workers, reemployment services, and hiring incentives for firms recruiting from *Mobilità* lists, which were largest when firms converted temporary contracts into permanent ones.[7] A distinctive feature of the program was its compatibility with temporary and part-time employment: benefits were suspended during these jobs, but these spells did not count toward the worker's remaining entitlement. As a result, workers could extend their eligibility period up to twice the original entitlement duration (Paggiaro and

[4] Law 223/1991, Art. 24. The threshold applies within a single production unit or across production units in the same province. The provision does not cover the expiry of fixed-term contracts, the end of work in construction, or seasonal activity.

[5] Law 223/1991, Art. 5.

[6] Law 223/1991, Art. 16. Effective work includes paid holidays, work-related injury, and compulsory maternity leave (Constitutional Court ruling no. 423/1995).

[7] Firms hiring temporary or part-time workers from *Mobilità* lists also benefited from hiring subsidies; converting these contracts to permanent employment generated the largest incentive.

Trivellato, 2002).[8] Benefit amounts were set by a national formula that replaced about 80% of the previous wage, subject to statutory caps that were uniform across regions and demographic groups.[9] Enrollment in the *Mobilità* lists was automatic for workers dismissed from firms with more than fifteen employees; workers from smaller firms could enroll voluntarily, and generally did (Paggiaro et al., 2009).

2.2 Potential Benefit Duration and the Fornero Reform

From its introduction in 1991, *Mobilità* PBD varied systematically by age and macro-area of dismissal, granting longer entitlements to workers facing lower expected exit rates from unemployment. PBD was defined by three age groups (under 40, 40–49, and 50 or older)[10] and two macro-areas: Center-North and South.[11] Older workers dismissed in the South were entitled to the longest *Mobilità* spells, reaching a maximum of 48 months.

The Fornero Reform (Law 92/2012) progressively phased out *Mobilità* and reduced these macro-area- and age-based differences in PBD, leaving both benefit levels and replacement rates unchanged.[12] The magnitude and timing of the reductions varied across age–region groups, as shown in Figure 1. For workers under 40 dismissed in the Center-North, PBD remained at 12 months. By contrast, comparable workers in the South saw their PBD fall from 24 to 12 months, fully converging to the Center-North level by 2015. For older workers, both regions saw reductions in PBD. In 2015, workers aged 40–49 had their PBD fall from 36 to 24 months in the South and from 24 to 18 months in the Center-North, narrowing the geographic gap from 12 to 6 months. Their PBD fell further in 2016, to 18 months in the South and 12 months in the Center-North, leaving the geographic gap unchanged at 6 months. Workers aged 50 or older had their PBD fall in 2015 from 48 to 36 months in the South and from 36 to 24 months in the Center-North, leaving the geographic gap unchanged at 12 months. In 2016, their PBD fell further to 24 months in the South and 18 months in the Center-North, narrowing the gap to 6 months. *Mobilità* was abolished entirely in 2017, outside our estimation window. From then on, collectively dismissed

[8] Brunello and Miniaci (1997), Paggiaro and Trivellato (2002), and Paggiaro et al. (2009) provide detailed institutional descriptions and empirical evaluations of the program.

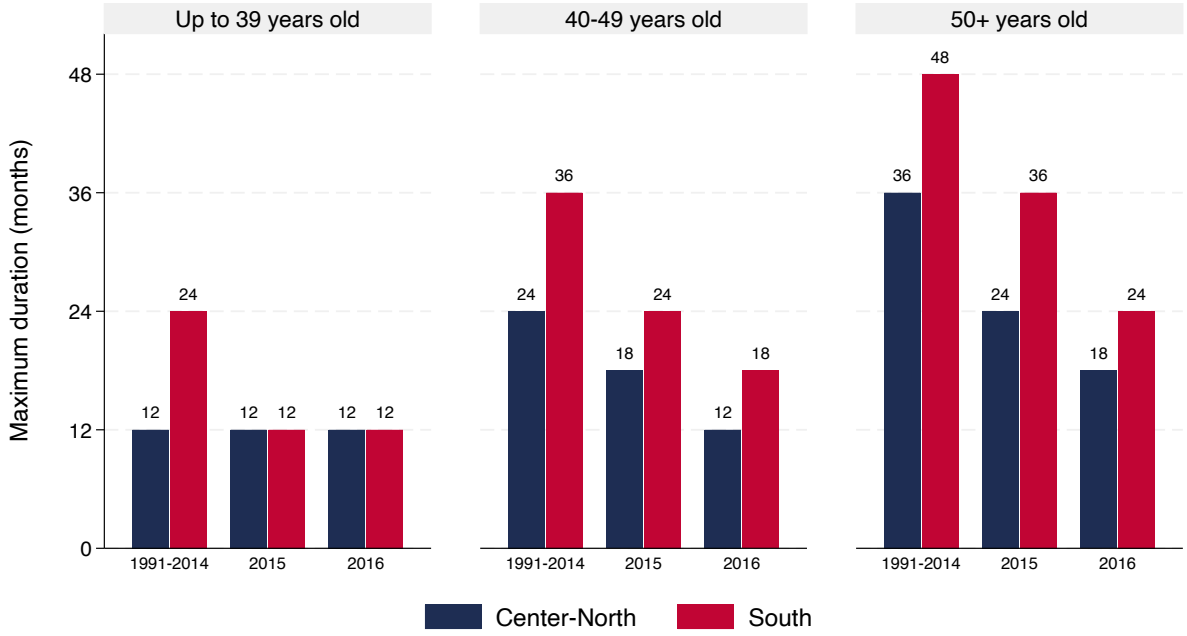
[9] In the first year, benefits were reduced by a 5.84% contribution adjustment. From the second year onward, the replacement rate was set at 80% of the initial benefit, subject to lower and upper bounds. For example, the monthly lower and upper ceilings were €969.77 and €1,165.58 in 2014, and €971.71 and €1,167.91 in both 2015 and 2016.

[10] Because the program applies to collective dismissals, manipulation of dismissal timing around age thresholds (Citino et al., 2026) is unlikely.

[11] Of Italy's 20 regions, 13 belong to the Center-North and 7 to the South.

[12] The reform also amended other parts of Law 223/1991: it changed the collective dismissal procedure and the penalties for violating it and introduced a dismissal levy on terminations from 1 January 2013. These changes applied nationally and took effect before the PBD reductions we exploit.

Figure 1: Maximum potential benefit duration (months) of the *Mobilità* program by age group and macro-area, 1991–2016



Notes. The figure reports the maximum potential benefit duration (PBD, in months) under the *Mobilità* program by age group (panels) and macro-area of dismissal (bars). PBD was constant from 1991 to 2014; the 2015 and 2016 bars reflect the phased reductions introduced by the Fornero Reform (Law 92/2012), described in Section 2.

workers started being covered by NASpI, the nationally uniform unemployment insurance system, introduced in 2015.

These age- and region-specific changes generate variation in PBD that is exogenous to individual worker and firm characteristics, conditional on age group and macro-area, providing the basis for our difference-in-differences design.

3 Theoretical Framework

To guide our empirical analysis, we develop a two-region random search model that illustrates how regional differences in PBD affect reservation wages, job-finding hazards, and internal migration. The model first shows that place-based generosity acts as a spatial wedge that keeps Southern workers in the region where offers are worse, and then generates three predictions on non-employment duration, migration, and job match quality. We also consider two extensions—heterogeneity by worker ability and endogenous search effort—that refine these predictions and motivate additional empirical tests. Online

Appendix B states the model formally, collects the derivations, and presents two further extensions, on age and on liquidity.

3.1 Baseline Model: Two-Region Search with Market Frictions

Consider the search problem of an unemployed worker in an economy with two regions, $r \in \{S, N\}$. The labor market is characterized by exogenous job arrival rates λ^r and wage distributions $F^r(w)$. We assume $\lambda^N > \lambda^S$ and $\lambda^S + \lambda^N \leq 1$, and that F^N first-order stochastically dominates F^S, capturing the superior economic conditions of the Center-North.

Workers receive unemployment benefits based on remaining PBD $\tau \in \{0, 1, \ldots, T^r\}$, entering unemployment with $\tau = T^r$, so that after t periods of unemployment a worker has $\tau_t^r \equiv \max\{T^r - t, 0\}$ periods left, with benefit flow $b(\tau) = b \cdot \mathbb{1}\{\tau > 0\}$; we set $U^r(\tau - 1) = U^r(0)$ when $\tau = 0$, so that once benefits are exhausted the worker remains in the zero-benefit state. Let $\delta^r \in [0, 1)$ denote the region-specific job separation probability, and let $\beta \in (0, 1)$ denote the discount factor. The value of holding a job with wage w in region r is:

$$W^r(w) = \frac{w + \beta\delta^r U^r(0)}{1 - \beta(1 - \delta^r)} \tag{1}$$

If a worker accepts a job in the other region, they incur a one-time migration cost $M \geq 0$.[13] The value of unemployment for a worker in region r with remaining PBD τ is:

$$\begin{aligned} U^r(\tau) = b(\tau) + \beta \Big[& \lambda^r \mathbb{E}_{w \sim F^r} \max\{W^r(w),\, U^r(\tau - 1)\} \\ & + \lambda^{r'} \mathbb{E}_{w \sim F^{r'}} \max\{W^{r'}(w) - M,\, U^r(\tau - 1)\} \\ & + (1 - \lambda^r - \lambda^{r'})\, U^r(\tau - 1) \Big] \end{aligned} \tag{2}$$

Reservation wages equate the value of a job to the value of continuing search. The local reservation wage $\bar{w}^r(\tau)$ satisfies $W^r(\bar{w}^r(\tau)) = U^r(\tau - 1)$, and the migration reservation wage $\bar{w}_S^N(\tau)$ satisfies $W^N(\bar{w}_S^N(\tau)) - M = U^S(\tau - 1)$.

The Spatial Wedge. Southern and Center-North workers apply different standards to the same Center-North offer. Because the value of unemployment depends on the entitlement only through the number of remaining periods, we compare a Southern and a Center-North worker at the same elapsed duration t since layoff. Define the spatial wedge as the

[13] A per-period cost m paid while employed away from the region of dismissal (rent, distance from family) is equivalent: since W^N is a present value, it amounts to a one-time cost $M = m/[1 - \beta(1 - \delta^N)]$, and all results below are unchanged.

gap between the reservation wage the Southern worker applies to a Center-North offer and the one the Center-North worker applies to it, $\Delta(t) \equiv \bar{w}_S^N(\tau_t^S) - \bar{w}^N(\tau_t^N)$. Since W^N is increasing,

$$\Delta(t) > 0 \quad \Longleftrightarrow \quad M + U^S(\tau_t^S - 1) > U^N(\tau_t^N - 1), \tag{3}$$

which holds whenever moving is costly ($M > 0$) or, at any elapsed duration $t < T^S - 1$, Southern PBD exceeds Northern PBD ($T^S > T^N$); and $\Delta(t)$ is increasing in both M and T^S. Place-based generosity thus operates like a moving cost: it makes Southern workers reject Center-North offers that an otherwise identical Center-North worker would accept, locking them into the region where offers are worse. This is consistent with the low pre-reform rate at which Southern workers were reemployed in the Center-North. Equalizing PBD shrinks the wedge, which is the mechanism underlying the first two predictions below; and where relocation is cheaper, more Center-North offers are just short of being worth accepting, so the migration response should be strongest there.

Prediction 1: Hazard Rates and Duration. At any elapsed duration t before the old entitlement would have run out, a reduction in T^S leaves a Southern worker with fewer remaining periods τ_t^S and so lowers the value of remaining unemployed, $U^S(\tau_t^S - 1)$. This reduces both $\bar{w}^S(\tau)$ and $\bar{w}_S^N(\tau)$, raising the job-finding hazard

$$h^S(\tau) = \lambda^S[1 - F^S(\bar{w}^S(\tau))] + \lambda^N[1 - F^N(\bar{w}_S^N(\tau))]$$

and shortening expected non-employment duration. The predicted effect of the reform on non-employment duration is therefore negative.

Prediction 2: Migration. As the value of remaining unemployed falls, the migration acceptance condition $W^N(w) - M \geq U^S(\tau_t^S - 1)$ becomes easier to satisfy, so Southern workers accept a wider range of Center-North offers following the cut in PBD in the South: the predicted effect on migration is positive. The response should accordingly be larger where relocating is cheaper, and hence stronger between bordering regions.

Prediction 3: Job Match Quality. The impact on job match quality, in this framework measured by accepted wages, is theoretically ambiguous due to two competing mechanisms. On one hand, the reduction in T^S lowers reservation wages, inducing workers to accept lower wage draws from any given distribution (the "search-haste" effect, negative). On the other hand, the reform induces a shift from F^S to the superior F^N distribution (the "reallocation" effect, positive). If the regional productivity gain compensates for the lower

selectivity, the net effect on accepted wages and match quality can be zero or positive. This prediction concerns the monetary dimension of match quality, but in the empirical analysis we also examine non-monetary dimensions (namely, employment stability and and sector switching) to assess whether shorter PBD pushes workers into lower-quality jobs.

3.2 Extensions

The baseline model assumes that workers are homogeneous in ability and that offers arrive at exogenous rates. We relax these two assumptions in turn. Both extensions leave the first and third predictions intact and sharpen the second.

Extension 1: Heterogeneity by Ability. Suppose worker ability $a \in \{H, L\}$ shifts the wage distribution such that F_H^r first-order stochastically dominates F_L^r. Because high-ability workers draw higher offers in expectation, they maintain higher reservation wages: $U_H^r(\tau) > U_L^r(\tau)$ and $\bar{w}_H^r(\tau) > \bar{w}_L^r(\tau)$.

As the reform lowers the value of remaining unemployed for all types, both groups face a lower migration threshold. Because F_H^N places more mass above any given threshold than F_L^N does, high-ability workers are more likely to receive Center-North offers that exceed the migration threshold, which can generate a more intense migration response. This extension thus refines Prediction 2 by predicting stronger effects for high-ability workers.

Extension 2: Endogenous Search Effort Allocation. We extend the model by relaxing the assumption of exogenous arrival rates λ^r and allowing workers to allocate a share of search effort $p \in [0, 1]$ to the South and $(1 - p)$ to the Center-North. Arrival rates are determined by concave technologies $\lambda^S(p, \theta^S)$ and $\lambda^N(1 - p, \theta^N)$. The worker chooses p to equalize the marginal returns to search effort:

$$\frac{\partial \lambda^S}{\partial p}\Omega_S = \frac{\partial \lambda^N}{\partial (1-p)}\Omega_N \tag{4}$$

where Ω_r is the expected positive surplus of an offer from region r over the value of remaining unemployed, $U^S(\tau_t^S - 1)$. A reduction in T^S lowers $U^S(\tau_t^S - 1)$, which increases both surplus terms. Because the moving cost leaves many Center-North offers only just worth accepting, the surplus in the Center-North increases relatively more. To rebalance the returns to search, workers reduce the share of effort p allocated to the South, directing more effort toward the Center-North. This endogenous reallocation reinforces and extends Prediction 2: not only does the reform lower the threshold for accepting offers in the

Center-North, but it also increases the rate at which such offers arrive, amplifying the predicted migration response.

4 Data

We use matched employer–employee administrative records from the Italian Social Security Institute (INPS) covering the years 2011–2019.[14] The sampling frame is firm-based: a stratified random sample of firms together with a census of multinational companies operating in Italy. We observe the full workforce of these sampled firms, and because we then follow each worker's complete private-sector employment history over time, we also pick up their spells at other, non-sampled firms; we keep all such spells, inside and outside the sampling frame, to maximize sample size. We do not observe job spells in the public sector or self-employment. We refer to these data as the *employment relationships archive*.

The employment relationships archive contains detailed longitudinal information on employment contracts, including annual labor earnings, the number of weeks worked, contract type (permanent, temporary, or seasonal), work schedule (full-time or part-time), and broad occupation (blue-collar, white-collar, manager, apprentice, or other). The data also contain worker demographics such as gender, year of birth, migrant status, and province of residence, as well as firm characteristics including industry, establishment date, and, when applicable, closure date. In most cases, we aggregate 2-digit sectors following the Eurostat NACE Rev. 2 *A38* classification.[15] Importantly, the archive records the municipality of work, allowing us to track the geographic location of employment over time. Throughout the paper, we measure geographic mobility as reemployment in a macro-area different from the one of dismissal, that is, as *work* mobility rather than a change of residence; we use the terms migration, moving, and relocation in this sense. We rely on the municipality of work rather than the province of residence for two reasons. First, the administrative data report only the most recent province of residence, not its time-varying history, so residence cannot be used to infer workers' post-layoff location choices. Second, residence is measured with considerably more error than work location: workers often do not update their registered residence immediately after moving (Gargiulo et al., 2023) whereas the municipality of work is recorded contemporaneously with each job. Because our measure captures the location of reemployment, it may reflect cross-macro-

[14] Our sample period excludes the Great Recession and the COVID-19 pandemic.

[15] The A38 aggregation keeps most one-letter NACE sections intact but splits Manufacturing (C), Information and Communication (J), and Professional, Scientific and Technical Activities (M) into their constituent divisions. We depart from the standard only by further merging Real Estate through Education (O–Q) and Arts through Household Activities (T–U) into two single groups, yielding 34 rather than 38 categories.

area commuting rather than a change of residence; we address this in Section 6.5, where we compare bordering and non-bordering macro-areas, decompose moves by province, and measure the distance between the municipalities of dismissal and reemployment.

INPS maintains a separate archive, which we refer to as the *Mobilità* archive, recording each recipient's dismissal date and the start and end dates of benefit receipt. We use these records to identify *Mobilità* recipients and to reconstruct the total number of days on benefits.

4.1 Sample Construction

We construct our estimation sample by linking recipients in the *Mobilità* archive to their pre-layoff jobs in the employment relationships archive. To align the sample with the program's standard target population, we impose a conservative restriction: workers must hold a permanent, non-managerial, non-apprenticeship position both in the displacement job and in the year preceding entry into *Mobilità*. This excludes observations classified as apprentices, managers, or temporary employees in the displacement job and in the year before displacement (17.4% of observations). We then retain workers between 20 and 60 years old and one observation per worker-year: in the layoff year, the spell that triggered entry into *Mobilità*; in all other years, the spell with the highest annual earnings.

We use the dismissal date to link work spells in the employment relationships archive to the corresponding records in the *Mobilità* archive. However, the recorded dismissal dates are not always consistent across the two sources: discrepancies arise in 20.4% of cases. We retain observations for which the two dates coincide (79.6% of cases) or differ by no more than 30 days (6.1% of cases), and exclude all others.

Some workers experience more than one qualifying *Mobilità* spell within our sample period, each corresponding to a separate layoff event; we treat each spell as a separate observation, subject to the same sample restrictions as any other. Such spells account for 0.87% of observations in the full sample and 1.34% among workers under 40, our main sample of analysis.

We compute outcomes over the three years following the layoff. Specifically, we measure four sets of outcomes: (i) total days of *Mobilità* receipt; (ii) the duration of non-employment in the private sector, conditional on reemployment within three years;[16] (iii) indicators, defined for all displaced workers and equal to zero for those not reemployed three years after the layoff, for whether the worker is employed in a different macro-area

[16]Because the data do not cover employment in the public sector or self-employment, non-employment duration is the time without observed private-sector employment.

or in a region with a higher employment rate than the region of layoff; and (iv) post-layoff job quality, including weekly wages, firm wage premia,[17] macro-sector, and cumulative years in employment. Each job-quality outcome compares the job held three years after the layoff with the pre-layoff job, and is therefore measured conditional on being employed at that date; the exception is cumulative years in employment, which is defined for all displaced workers, with zeros for years without employment.

4.2 Summary Statistics

Table 1 reports descriptive statistics for three groups of workers under 40, our main estimation sample: employees who are not laid off during the sample period (columns 1–2), workers who are laid off and receive ordinary UI benefits (columns 3–4), and workers who are laid off and enter the *Mobilità* program (columns 5–6).[18] Workers entering *Mobilità* differ from both the first two groups. They are around 34 years old, about four years older than ordinary UI recipients, and include slightly smaller shares of women (37%) and migrants (11%). All *Mobilità* recipients in our sample held permanent contracts before layoff, reflecting both the program's eligibility rules and our sample restrictions, compared with 65% of ordinary UI recipients.[19] *Mobilità* recipients are also concentrated in more traditional industrial settings: 60% were employed in industry excluding construction, compared with 15% among ordinary UI recipients, while only 34% worked in services (6% in knowledge-intensive services and 28% in other services), compared with 58% among ordinary UI recipients.

Mobilità recipients also display stronger pre-layoff labor market attachment. They worked about 39 weeks per year, compared with 28 among ordinary UI recipients, and earned annual labor earnings of about €17,600, which is well above the €11,500 average among ordinary UI recipients and only slightly below the €18,500 average among workers who are not laid off. Weekly wages follow the same pattern: *Mobilità* recipients earned about €438 per week, compared with €394 among ordinary UI recipients and €474 among workers who are not laid off. Their firms are also older and larger. Firms that generate entries into the *Mobilità* program are, on average, 18 years old, compared with 11 years for firms associated with ordinary UI recipients, and the median firm employs 27 more workers. The large size reflects the program's institutional design (Section 2): collective

[17] Firm wage premia are estimated using the AKM two-way fixed-effects framework of Abowd et al. (1999) on the full INPS data.

[18] Online Appendix Table A.1 reports the same statistics with no age restriction.

[19] Law 223/1991 restricts *Mobilità* eligibility to workers on open-ended contracts. Some recipients in the administrative data are recorded as temporary, likely reflecting recording errors or contracts not yet converted at the time of dismissal, and we drop these observations as a conservative sample restriction.

Table 1: Summary statistics of worker characteristics, workers under 40

	(1)	(2)	(3)	(4)	(5)	(6)
	Not laid off		Laid off, ordinary UB		Laid off, Mobilità	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Age	30.87	5.43	29.85	5.53	33.83	4.09
Female	0.41	0.49	0.38	0.48	0.37	0.48
Migrant	0.14	0.35	0.20	0.40	0.11	0.32
Center-North	0.70	0.46	0.56	0.50	0.68	0.47
Annual earnings	18507.52	13217.38	11468.76	8907.57	17575.90	10519.37
Weekly wage	474.45	267.17	393.65	225.43	437.66	206.87
Weeks	36.59	16.76	28.26	16.88	39.01	15.27
Blue-collar	0.43	0.49	0.55	0.50	0.61	0.49
White-collar	0.35	0.48	0.19	0.39	0.39	0.49
Part-time	0.23	0.42	0.31	0.46	0.13	0.33
Permanent	0.74	0.44	0.65	0.48	1.00	0.00
Temporary	0.24	0.43	0.33	0.47	0.00	0.00
Seasonal	0.02	0.12	0.02	0.14	0.00	0.00
Industry excl. construction	0.27	0.44	0.15	0.35	0.60	0.49
Construction	0.04	0.20	0.11	0.31	0.06	0.24
KIS	0.13	0.33	0.07	0.26	0.06	0.24
Other services	0.43	0.50	0.51	0.50	0.28	0.45
Age of firm	16.47	12.90	11.31	10.31	17.70	13.44
Firm size (median)	6.00	6922.88	7.00	6357.60	34.00	1219.21
N. firms	1,311,779		487,676		21,981	
N. workers	6,158,600		1,012,744		74,328	
N. observations	22,737,732		1,235,578		74,782	

Notes. The table reports means and standard deviations between 2011 and 2016 for workers under 40 who are employed and not laid off (columns 1 and 2); laid-off workers receiving ordinary unemployment insurance benefits (columns 3 and 4); and laid-off workers in the *Mobilità* program (columns 5 and 6). For firm size, columns 1, 3, and 5 report the median rather than the mean, because the firm-size distribution is highly skewed; columns 2, 4, and 6 report the standard deviation.

dismissals applied only to firms with more than fifteen employees, while workers from smaller firms could enroll only voluntarily (Paggiaro et al., 2009).

Overall, the descriptive statistics indicate that *Mobilità* primarily covered older, highly attached workers in permanent positions at established industrial firms, consistent with its original design to protect long-tenured employees affected by restructuring or plant closures.

5 Empirical Strategy

We exploit the differential reduction in *Mobilità* PBD across macro-areas induced by the Fornero Reform (Figure 1), focusing on workers under 40, for whom the reform provides the cleanest quasi-experimental variation. For this group, the reform fully eliminated the geographic PBD gap, reducing PBD for workers dismissed in the South while leaving it unchanged in the Center-North. This creates a clean treatment–control comparison, with workers laid off in the South as the treated group and those laid off in the Center-North as controls. For older workers, the design is less clean: the reform reduced PBD in both macro-areas and only narrowed the regional gap, so the identifying variation differs in both magnitude and interpretation. We report results for the older age groups separately in Section 6.6.

We estimate the following specification:

$$y_{i,t+3} = \sum_{k \neq 2014} \beta_k \, T_i \times \mathbf{1}\{t = k\} + \mathbf{x}_{it}'\delta + \kappa T_i + \psi_j + \eta_t + \epsilon_{it}, \tag{5}$$

where i indexes individuals and $t \in \{2011, \ldots, 2016\}$ the year of layoff. T_i is a treatment indicator equal to 1 if the layoff occurred in the South and 0 otherwise. We interact T_i with indicators for the year of layoff, $\mathbf{1}\{t = k\}$, excluding 2014 as the baseline pre-reform year, given that the reform took effect in January 2015. The dependent variables $y_{i,t+3}$ are measured three years after the layoff or, in some instances, as the cumulative value of the outcome over the three years following the layoff.[20] The vector $\mathbf{x}_{it}$ includes a quadratic polynomial in age and indicators for female and migrant workers. The specification further includes firm-of-layoff fixed effects ψ_j and layoff-year fixed effects η_t.[21] Standard errors are clustered at the firm-of-layoff level. The coefficients of interest are the β_k, which capture the differential effect of being dismissed in the South relative to the Center-North in year k, compared with the 2014 baseline.[22]

With firm-of-layoff fixed effects, identification comes from differential changes across

[20] We examine one- and two-year horizons as robustness checks. We do not consider longer horizons because outcomes for workers displaced in 2016 would then be measured during the COVID-19 pandemic.

[21] Firm fixed effects absorb T_i only for firms that dismiss workers in a single macro-area. In our sample, 449 of 21,981 firms (2.043%) dismiss workers in both the South and the Center-North, so T_i varies within the fixed effect for these firms; we therefore include T_i in levels, with coefficient κ.

[22] Two-way fixed effects estimators can place negative weights on treatment effects when adoption is staggered and already-treated units serve as controls (Goodman-Bacon, 2021); see Roth et al. (2023) for a survey. That concern does not arise here because treatment begins in 2015 for all workers under 40 dismissed in the South, never applies to those dismissed in the Center-North, and each worker enters the sample once. For the older age groups (Section 6.6), PBD falls in both macro-areas and in both reform years, so the variation there is one of continuous dose rather than staggered adoption.

dismissal cohorts within the same firm, net of aggregate year shocks. For a firm whose layoffs all occur in one macro-area, the firm fixed effect absorbs the level of the Southern indicator but not its interaction with year of layoff. The design therefore does not require firms that dismiss workers in both macro-areas: a firm dismissing only in the South contributes through differences between the cohorts it dismissed after the reform and those it dismissed in 2014, relative to the corresponding changes in the Center-North firms. This variation is widely available, since 67% of worker-level observations come from firms observed in at least two different layoff years. The β_k coefficients capture within-firm responses to the reform-induced change in PBD rather than time-invariant differences across firms or macro-areas.

The key identifying assumption is that, conditional on our fixed effects, outcomes for workers displaced in the South and the Center-North would have followed parallel trends absent the reform. This assumption requires the absence of confounding region-specific shocks around the time of the reform, no anticipatory responses in outcomes before implementation, and no differential compositional changes across groups that independently affect outcomes.

We assess the plausibility of this assumption using the event-study framework. Pre-reform coefficients close to zero and precisely estimated are consistent with parallel trends and with the absence of anticipation in outcomes. Compositional changes among workers were unlikely because of the rules governing collective dismissal: the criteria determining which workers a firm dismissed were set by national law and were unchanged by the reform (Section 2). However, the reform may have changed the composition of dismissing firms. We examine whether the reform induced compositional changes in displaced workers or dismissing firms by testing for differential changes in pre-layoff worker and firm characteristics across treatment groups. As a further check, we apply the same event-study specification to workers receiving ordinary UI benefits, a placebo group whose PBD was unaffected by the *Mobilità* reform, to rule out confounding trends common to all UI claimants. We also examine anticipation in terms of timing of layoffs. Section 6.5 presents these diagnostics, which support the plausibility of the identifying assumptions underlying our design.

A final concern is interference between the treatment and control groups. Workers dismissed in the Center-North serve as controls, and treated Southern workers who relocate enter the Center-North labor market, so the treatment may affect the outcomes of the very workers against whom it is measured, violating the stable-unit-treatment-value assumption. Section 6.2 discusses this issue, showing that the reform-induced flow is small relative to the Center-North labor market, and that the direction of migration flows around

the reform is inconsistent with contamination.

6 Results

This section presents the empirical results, following the structure of the model's predictions. Section 6.1 documents the reform's effects on benefit receipt and non-employment duration (Prediction 1); Section 6.2 presents the migration response, its heterogeneity by worker ability, and how the response depends on destination labor demand (Prediction 2); Section 6.3 examines job match quality (Prediction 3), and Section 6.4 expresses the main effects in euros. Section 6.5 collects the validation and robustness analyses, and Section 6.6 reports results for the older age groups. Online Appendix Table A.2 reports the 2015 and 2016 point estimates underlying the main event-study figures for all three age groups.

6.1 Benefit Receipt and Non-Employment Duration

Effect on benefit receipt. We begin by examining whether the reform's reduction in *Mobilità* PBD translated into shorter *actual* benefit receipt—the direct effect of the reform. Extensive prior work has shown that changes in PBD affect actual benefit receipt (e.g., Card et al., 2007; Lalive, 2008; Johnston and Mas, 2018). For each worker in our sample, we compute the total days of *Mobilità* receipt in the three years following dismissal and estimate equation (5). Figure 2a displays the results. Actual benefit receipt fell by approximately 200 days in both 2015 and 2016, equivalent to 54 percent of the 365-day (12-month) PBD reduction introduced by the reform. This magnitude is similar to estimates reported by Johnston and Mas (2018) for the United States.[23] The reform thus substantially reduced actual benefit receipt. Having established this direct effect, we next examine whether the reduction in benefit receipt translated into shorter non-employment duration.

Effect on non-employment duration. A central question in the UI literature is whether more generous benefits reduce search effort and prolong non-employment spells (e.g., Meyer, 1990; Lalive, 2008; Card et al., 2007). Our setting provides a complementary perspective, allowing us to examine whether a reduction in PBD accelerates reemployment. We estimate equation (5) with the outcome being the duration of non-employment in the private sector, conditional on finding a job within three years of dismissal.[24]

[23] Johnston and Mas (2018) examine a reduction in UI benefit duration in Missouri and find that reducing PBD by one month lowers actual UI receipt by about 15 days on average.

[24] Non-employment duration is defined as time without observed employment in the private sector, as we do not observe active job search, public-sector, or self-employment spells in our data. Because our question

Figure 2b reports the effect of the reform on post-layoff non-employment duration. For workers laid off in 2015 and 2016, the 12-month reduction in PBD shortens non-employment spells by about 150 days (147 days in 2015 and 150 days in 2016), consistent with Prediction 1. This magnitude is somewhat larger than the estimate reported by Johnston and Mas (2018), who find that a one-month reduction in PBD reduces non-employment duration by about 10 days in the context of a UI duration cut in Missouri. Our estimates indicate that lower PBD significantly accelerates reemployment among workers who find a private-sector job within three years of displacement.

We examine whether the reduction in non-employment duration differs by the type of contract in the first post-layoff job (Online Appendix Figure A.1). In 2015, non-employment spells shorten significantly for workers entering both permanent and temporary jobs, with a larger reduction for permanent contracts, although the two estimates are not statistically different from each other. In 2016, both estimates are negative but imprecise. These subsample estimates are noisier than the baseline, including in the pre-reform years, but they suggest that, among those dismissed in 2015, the reform shortened non-employment for workers entering both types of jobs, rather than only shifting workers toward temporary employment.

6.2 Internal Migration

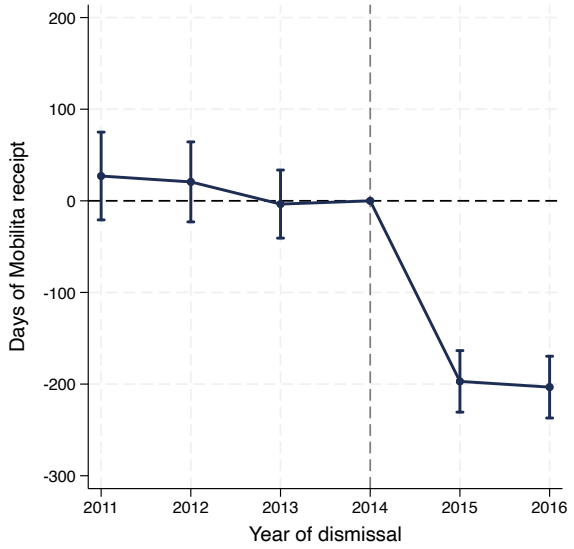
Our central finding is that equalizing *Mobilità* PBD across macro-areas substantially increases geographic mobility toward stronger labor markets, consistent with Prediction 2. We estimate equation (5) for the probability that a worker displaced in one macro-area is employed in the other three years later, with workers not employed at that point coded as zero, and report results in Figure 3.

For workers laid off in 2015, the reform increases the probability of being reemployed in a different macro-area by 5.5 percentage points, or 70.5 percent relative to the pre-reform treated group mean of 7.8 percent (Online Appendix Table A.2, column 3). Pre-

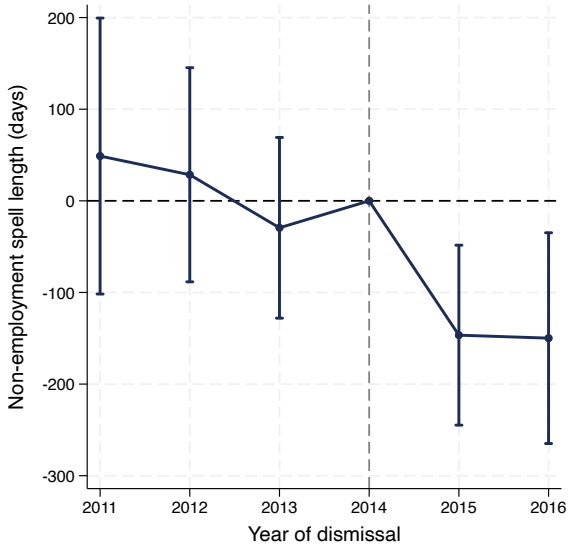
concerns when and where displaced workers find new jobs, time to reemployment is the appropriate outcome, consistent with recent work that focuses on reemployment rather than formal unemployment duration (Johnston and Mas, 2018; Karahan et al., 2025). Moreover, flows into informality, self-employment, or the public sector are unlikely to be large enough to drive non-employment duration or the results documented below. In particular, estimated informal employment is between 9.8 and 12.4 percent of dependent employment in 2014, with no sign of a strong increase over the following years (Dalla Zuanna et al., forthcoming), and most of it is *partial* informality—a worker who remains formally employed while also working some hours off the books—rather than a transition out of recorded employment altogether, which is the margin that would affect our estimates. Full transitions from private-sector employee status to self-employment average 2.1 percent per year over the period 2010–2019 (our computation using EU-SILC data), and transitions between private- and public-sector employment are even smaller, less than 1 percent of private-sector employment (INPS, XXII Annual Report, chapter 3.6.4).

Figure 2: Effect of the reform on benefit receipt and non-employment duration

(a) Days of *Mobilità* receipt



(b) Non-employment spell length (days)



Notes. The figure reports event-study coefficients from equation (5) for workers under 40. Panel (a) shows the effect on total days of *Mobilità* benefit receipt within three years after layoff. Panel (b) shows the effect on the duration of non-employment spells, conditional on finding a job within three years. All regressions include firm-of-layoff and layoff-year fixed effects, a quadratic in age, and indicators for female and migrant workers. Standard errors are clustered at the firm-of-layoff level. Online Appendix Table A.2 (columns 1–2) reports the corresponding 2015 and 2016 point estimates.

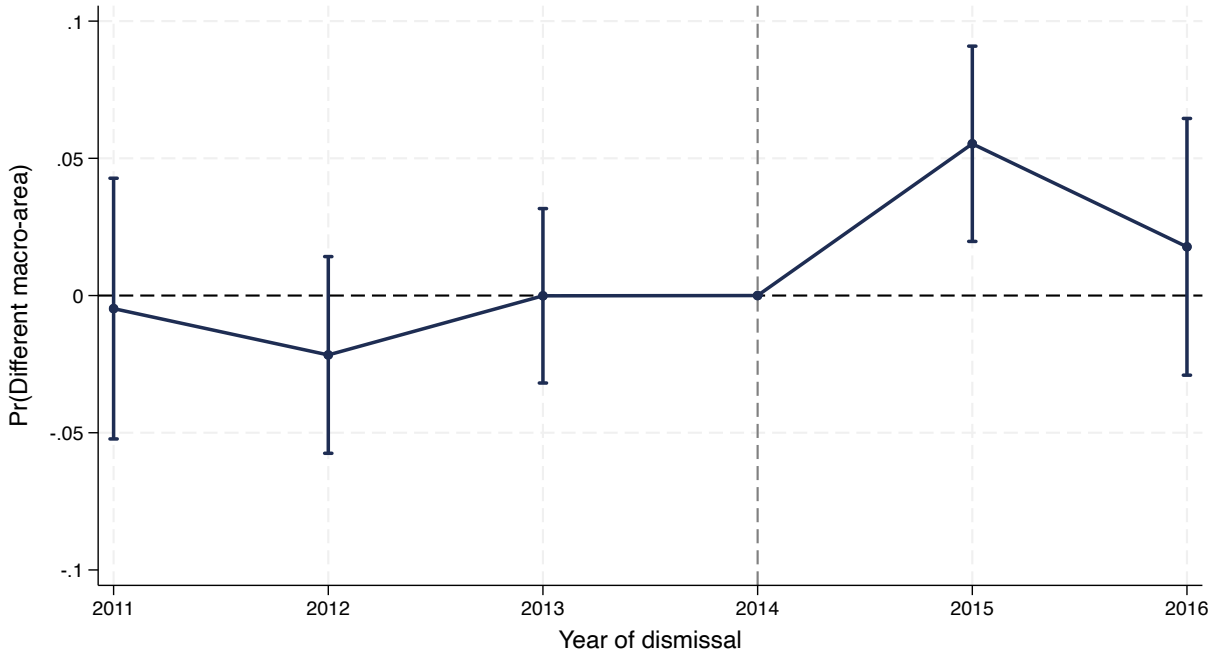
reform coefficients are close to zero and precisely estimated, supporting the parallel trends assumption.

Migration responses are directed toward regions with stronger labor markets. The estimated effect is similar when the outcome is defined as reemployment in a region with a higher employment rate than the region of dismissal: about 5 percentage points in 2015, compared with 5.5 in the baseline (Online Appendix Figure A.3). This reflects workers sorting toward labor markets with stronger employment conditions rather than relocating randomly across macro-areas.

Online Appendix Figure A.2 shows that the overall migration effect reflects a clear increase in South-to-Center-North mobility and a moderate decline in Center-North-to-South mobility; the former drives most of the aggregate effect.[25] This decomposition of flows by direction also bears on the interference concern raised in Section 5. Interference between the treated and control groups could occur through two channels, both of which would raise mobility in the opposite direction. First, control workers who lose Center-North jobs to arriving Southern workers would widen their search to other regions, including

[25]Migration rates are residualized by firm-of-layoff and layoff-year fixed effects.

Figure 3: Probability of working in a different macro-area post-layoff, event-study estimates

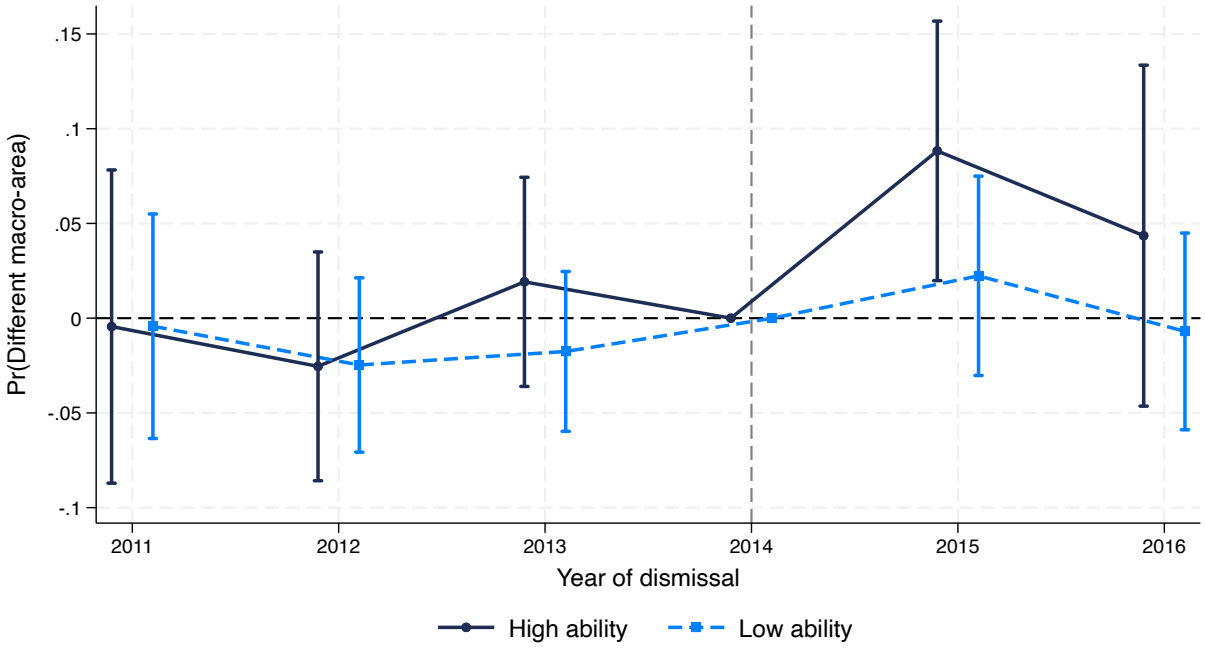


Notes. The figure presents event-study coefficients from equation (5) showing the effect of the reform on the probability that workers under 40 are employed in a different macro-area from their macro-area of dismissal three years after the layoff. The outcome is defined for all workers; those not employed at that point are coded as zero. Each point measures the change relative to 2014 (the omitted year). The specification includes firm-of-layoff and layoff-year fixed effects, a quadratic in age, and indicators for female and migrant workers. Standard errors are clustered at the firm-of-layoff level. Online Appendix Table A.2 (column 3) reports the corresponding 2015 and 2016 point estimates.

the South. Second, with fewer Southern job seekers competing for Southern jobs, those jobs would become easier for Center-North workers to obtain. Both channels predict a rise in Center-North-to-South mobility after the reform, and we observe the opposite. Although the evidence is descriptive, the sign of the pattern is informative: the control group shows no sign of the response that interference would produce. The magnitude of the reform-induced flow is also too small to generate meaningful contamination: applying the estimated 5.5 percentage-point effect to the 1,905 under-40 *Mobilità* recipients dismissed in the South in 2015 implies about 105 additional South-to-Center-North moves, a negligible fraction of the Center-North hiring that year (i.e., 0.004 percent of approximately 2.6 million hires).

Beyond the average effect, migration responses are concentrated among high-ability workers, consistent with evidence that migrants are positively selected on ability (Borjas et al., 1992; Bound and Holzer, 2000; Bütikofer and Peri, 2021). We measure worker ability using individual fixed effects from an AKM wage regression estimated on the full INPS

Figure 4: Heterogeneous analysis by worker ability



Notes. The figure presents event-study coefficients from equation (5) estimated separately for workers below and above the median of the ability distribution, where ability is proxied by individual fixed effects from an AKM wage regression. The outcome is the probability of reemployment in a different macro-area three years after the layoff. Firm-of-layoff and layoff-year fixed effects and worker-level controls are included. Standard errors are clustered at the firm-of-layoff level. Online Appendix Table A.3 reports the corresponding point estimates and the p-value for the high- versus low-ability difference.

sample.[26] We re-estimate equation (5) using a fully interacted specification that allows the treatment effect to differ between below- and above-median ability workers. Figure 4 and Online Appendix Table A.3 showed a marked difference in point estimates: above-median ability workers experience a large and statistically significant increase in migration probability of 8.8 percentage points in 2015, whereas the effect for below-median ability workers is small and insignificant throughout. The gap is economically large but estimated imprecisely; the difference in coefficients has a p-value of 0.17 (Online Appendix Table A.3). This pattern is consistent with Extension 1 of the theoretical framework: when PBD falls, high-ability workers are more likely to receive job offers in the Center-North that exceed their moving costs, making relocation more attractive.[27]

[26]The AKM model regresses log wages on worker and firm fixed effects, along with a quadratic in age and year fixed effects (Abowd et al., 1999). We use worker fixed effects as a proxy for worker ability, following Card et al. (2013).

[27]This offer-based mechanism is observationally equivalent to one in which high-ability workers are complementary to Center-North firms (e.g., agglomeration-driven sorting); our design cannot separately identify the two.

For workers laid off in 2016, the migration effect is positive but small and statistically insignificant. Figure 2b shows that workers laid off in 2016 found jobs more quickly following the reform, at a pace similar to workers laid off in 2015. Together with the null migration result, this suggests that faster reemployment was absorbed by local labor markets rather than driven by geographic relocation, consistent with vacancies in the Center-North becoming scarcer in 2016. We probe this interpretation directly below.

Destination demand and the 2016 null effect. The muted 2016 response is unlikely to reflect a fading incentive: the geographic PBD gap was fully eliminated in 2015 and remained closed in 2016, and benefit receipt and non-employment duration continued to fall for the 2016 cohort. We instead trace it to the demand-side pull of destination regions. Equalized PBD lowers the value of remaining unemployed in the South, but relocation occurs only if workers receive sufficiently attractive offers in the Center-North. When destination demand is weak, workers may still respond to lower PBD, but by finding jobs faster locally. We test this by asking whether the migration response varies with workers' *ex ante* exposure to Center-North demand.

We construct three demand measures from a worker's pre-layoff region and two-digit sector, combined with pre-reform (2011–2014) mobility flows of similar workers: (i) destination labor demand, the hiring rate in the worker's sector weighted by predicted destinations; (ii) predicted Center-North vacancies, a leave-one-destination-out sector-wide hiring rate that isolates vacancy availability from conditions in any single destination; and (iii) the intensity of the 2015 permanent-hiring boom in predicted destinations. Each measure assigns demand based on predicted rather than realized destinations: because the origin cells and mobility weights are all fixed before the layoff, the measures are purged of the worker's own post-layoff location choice. We also build a composite index, the average of the three standardized measures, which pools their common signal and attenuates the noise in any single one. For each measure, we split workers at the sample median and estimate an event study fully interacted with the high-demand indicator. The full estimates are reported in Figure 5 and Online Appendix Table A.4.[28]

The results are consistent with demand-pull, though the evidence is suggestive. In

[28]The sample-median split classifies roughly half of workers as high-demand in every cohort by construction, so it cannot speak to whether the pool of strong destinations itself shrank after 2015. As a robustness check, Online Appendix Figure A.4a instead fixes a threshold in *levels* (destination hiring rate of 18.2%, chosen from the pre-reform 2011–2014 relationship between demand and mobility; see the figure for details) so that the high-demand share can vary by cohort. Online Appendix Figure A.4b shows that this reproduces the qualitative 2016 pattern—positive for high demand (0.036), negative for low demand (-0.039)—though neither coefficient nor their difference ($p = 0.17$) is significant, and the high-demand share does not fall sharply from 2015 to 2016 under this measure (83% in both years).

2015, the migration response is positive for both the high- and low-demand groups under every measure, , with point estimates, if anything, larger for the low-demand group, and the high-minus-low differences are statistically indistinguishable from zero (p-values 0.30–0.58). This is consistent with Center-North demand being uniformly strong that year: permanent employment grew by about 2 percent in 2015 alone, compared with 1.6 percent over the following three years combined. Demand appears to have been strong enough to pull workers regardless of their specific sector exposure. In 2016, the pattern shifts: the migration effect turns negative for the low-demand group under all four measures (-0.02 to -0.04), while remaining positive for the high-demand group (0.03–0.04). Neither set of coefficients is individually significant, and the high-minus-low differences are not statistically significant (p-values 0.13–0.32), so we read the sign reversal as indicative rather than conclusive. It is nonetheless consistent with the 2016 null reflecting a change in the margin of response: workers whose predicted destinations had cooled responded to lower PBD by accelerating *local* reemployment rather than relocating, while those still facing strong Center-North demand continued to move.

6.3 Job Match Quality

Monetary dimensions. Having established that reduced UI generosity accelerates reemployment and increases migration toward higher-employment regions, we now examine whether shorter PBD comes at the cost of poorer job matches. As shown in Section 3, the effect on job match quality is theoretically ambiguous: shorter search may worsen matches, but access to stronger labor markets may improve them. We estimate equation (5) on two monetary dimensions of post-layoff job quality measured at the job held three years post-layoff: the probability that weekly wages exceed those of the pre-layoff job, and the probability of being at a firm with a higher AKM wage premium. Because we do not adjust for differences in cost of living, our wage measures are nominal.

Figure 6a shows that the reform has no statistically significant effect on the probability that post-layoff nominal weekly wages exceed pre-layoff wages of workers dismissed in the South, despite the shorter period of job search. The point estimates are positive in both cohorts, and the confidence intervals rule out declines larger than 5.1 percentage points.

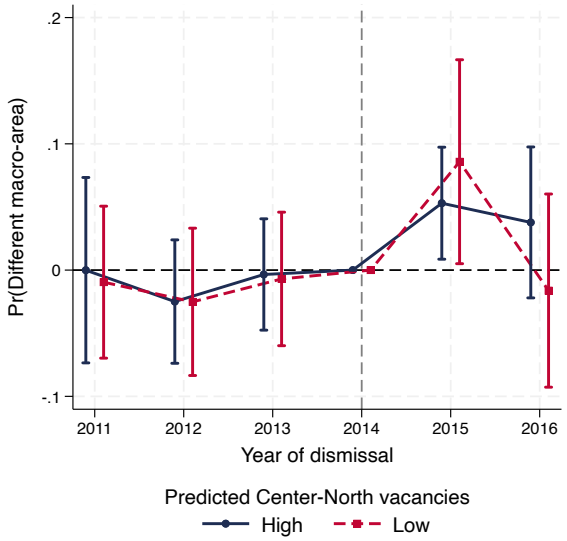
Our estimates on firm wage premia move in the other direction. Figure 6b shows that workers dismissed in the South become 5.7 percentage points less likely to be employed, three years after the layoff, at a firm with a higher AKM premium than their pre-layoff firm. This result is significant at the 10 percent level. However, the 2013 coefficient is negative, statistically significant, and larger in magnitude than the 2015 estimate, so we interpret

Figure 5: Heterogeneous effects on migration by destination labor market conditions

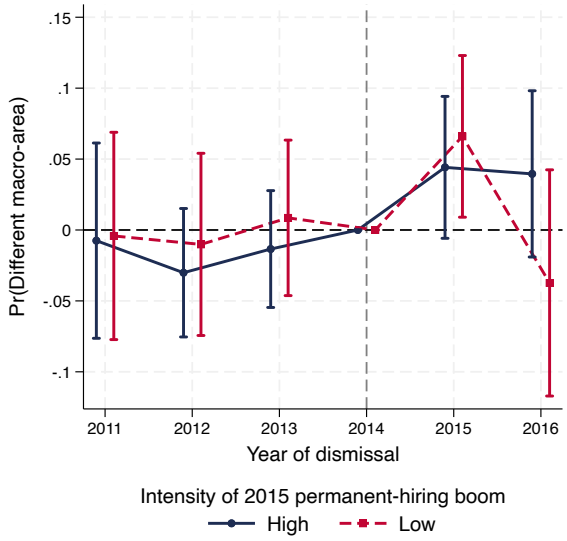
(a) Destination labor demand



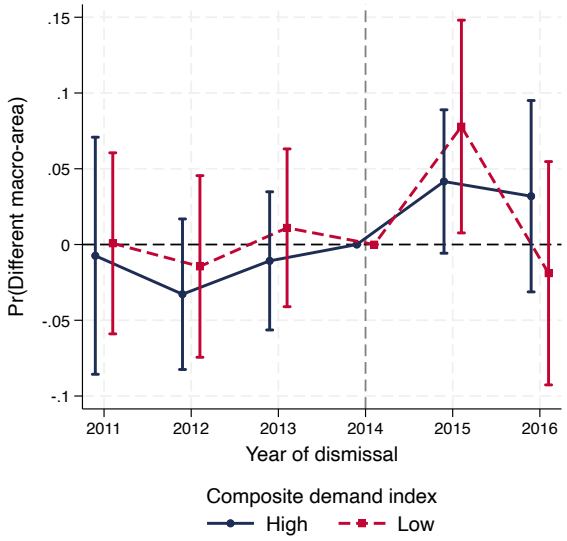
(b) Predicted Center-North vacancies



(c) Intensity of 2015 permanent-hiring boom



(d) Composite demand index



Notes. The figure presents event-study coefficients for the probability of reemployment in a different macro-area, for workers under 40. Each panel splits the sample by a different measure of labor market conditions at the destination: destination labor demand (panel a), predicted Center-North vacancies (panel b), the intensity of the 2015 permanent-hiring boom (panel c), and a composite demand index (panel d), constructed as the average of the three measures in panels (a)–(c), each standardized to mean zero and unit standard deviation in the pre-reform period. High and low groups are defined by whether the destination measure is above or below the sample median computed over all years in the analysis sample; for the composite index, the split uses the median among workers under 40 laid off in 2011–2016. Coefficients are estimated from a pooled stacked regression that interacts all regressors (dismissal-year effects, treatment indicator, event-study interactions, individual controls, and firm fixed effects) with the high-demand indicator, so that the point estimates reproduce those of separate subgroup regressions while providing correct standard errors for the high-minus-low test. The specification includes firm-of-layoff and layoff-year fixed effects, a quadratic in age, and controls for female and migrant status. Standard errors are clustered at the firm-of-layoff level. Online Appendix Table A.4 reports the corresponding point estimates and p-values for the 2015 and 2016 reform cohorts.

the result on this outcome with caution. As Table 1 shows, *Mobilità* firms are substantially larger than other firms, including those whose laid-off workers receive ordinary UI. Large employers pay more (Brown and Medoff, 1989), largely reflecting higher wage premia rather than worker composition (Bloom et al., 2018). For the sample of workers in *Mobilità*, improving on the wage premium of their pre-layoff firm is challenging, especially given the shorter search period resulting from the reform.

Non-monetary dimensions. Beyond wages and firm quality, we examine two further dimensions of job match quality: employment stability and sectoral mobility. Longer employment spells suggest better matches, while sector switching may indicate the presence of adjustment costs or a deterioration in match quality.

Figure 6c shows that the reform leads workers dismissed in the South to accumulate approximately 0.25 additional years of employment in the three years following displacement. Combined with the finding that these workers find jobs more quickly, this indicates that faster reemployment did not come at the cost of less time in work.

Figure 6d shows that workers dismissed in the South in 2015 are 9.6 percentage points less likely to be working in a different macro-sector three years after displacement. This effect may reflect both the labor market advantage of relocating to the Center-North and changes among those who remain in the South. Online Appendix Table A.5 reports the corresponding point estimates for all four job quality outcomes.

Taken together, the monetary and non-monetary results do not fully resolve the ambiguity of Prediction 3. The reallocation and the search-haste effects are both present and influence different dimensions of job match quality. Reduced selectivity may appear in the wage premium of the reemploying firm. The effects on nominal pay, the employment workers accumulate, and sectoral attachment are either null or positive. The cost of faster search emerges, if at all, in the quality of the reemploying firms.

6.4 Policy Accounting

We now express the estimated effects in euros. We convert each worker's days of *Mobilità* receipt using the statutory benefit schedule, and measure post-layoff earnings as the sum of annual labor earnings over three years, with zeros for years without observed employment; both are deflated by the CPI of the year of payment. Estimating equation (5) on these outcomes, *Mobilità* payments fall by €5,195 per treated worker for the 2015 cohort (s.e. 541) and €5,263 for the 2016 cohort (s.e. 538), or 67 and 68 percent of the control-group mean, with pre-reform coefficients below 2.5 percent of that mean. The saving is proportionally

smaller than the 73 percent reduction in days of receipt, because the reform removed entitlement at the end of the spell, where benefits are paid at 80 percent of the initial allowance. Cumulative earnings are higher for treated workers, by €1,854 (s.e. 2,124) and €3,293 (s.e. 2,872), but are estimated too imprecisely to support a strong claim; the 95 percent confidence interval for the 2015 cohort rules out three-year earnings losses larger than about 8 percent of the control mean. The reform therefore delivered a large and precisely estimated fiscal saving with no measurable earnings cost to displaced workers. To assess efficiency, the two figures should not be netted, since lower benefit payments are a transfer from displaced workers to the public budget whereas changes in earnings reflect output. From the workers' perspective, however, both affect income: because earnings gains are too small and imprecise to offset the loss in benefits, workers' gross income—benefits plus earnings—over the same three-year window fell by about €3,300 for the 2015 cohort and €2,000 for the 2016 cohort. Online Appendix C details the construction and reports the full set of estimates.

6.5 Robustness

Selection on workers and firms. Our difference-in-differences design assumes that the reform did not affect the composition of laid-off workers or of dismissing firms. To verify this, we estimate the following specification using outcomes measured two years *before* the layoff year:

$$y_{i,t-2} = \beta\ T_i \times \mathbf{1}\{t \geq 2015\} + \mathbf{x}'_{it}\delta + \psi_j + \eta_t + \epsilon_{it}, \tag{6}$$

where $y_{i,t-2}$ denotes a worker's outcome two years before the layoff year t. The specification follows equation (5), except that T_i is interacted with a single post-reform dummy rather than yearly indicators. If the reform changed the composition of laid-off workers or dismissing firms, we would expect β to be statistically different from zero. We estimate this specification for two sets of outcomes: workers' pre-layoff employment characteristics and the characteristics of their dismissing firms. We report standardized coefficients to facilitate the comparison of the effects across variables.

Online Appendix Figure A.5a shows that the reform does not differentially affect a wide range of workers' pre-layoff characteristics, including the cumulative number of jobs, occupational and sectoral mobility, work location, earnings, and contract type. The only statistically significant coefficient is on the probability of changing occupation; it is smaller than 15 percent of a standard deviation and economically negligible. Online Appendix Figure A.5b shows similarly reassuring results for several firm characteristics, including firm size and age, industry composition, and AKM firm wage premia.

Together, these results support the identifying assumption that the reform did not induce differential selection among laid-off workers or dismissing firms.

All leads and lags. Our baseline specification measures outcomes cumulatively over the three years following layoff. To verify that the results are not sensitive to this choice, we estimate the following alternative event-study specification separately for each year:

$$y_{i,t+h} = \sum_{k \neq 2014} \beta_k T_i \times \mathbf{1}\{t = k\} + \mathbf{x}_{it}'\delta + \psi_j + \eta_t + \epsilon_{it}, \tag{7}$$

where $h \in \{-3, -2, -1, 0, 1, 2, 3\}$ indexes years relative to the layoff year. Online Appendix Figure A.6 and Online Appendix Table A.6 report results for workers under 40. Pre-reform leads are close to zero, supporting parallel trends, while post-reform lags confirm the main findings: for workers laid off in 2015, the reform increases the probability of moving to a different macro-area by approximately 5 percentage points. The effect for workers laid off in 2016 is positive and marginally significant (10% level) one year after layoff, but statistically insignificant at longer horizons. Results for older age groups (Online Appendix Figure A.7) are consistent with the patterns documented in Section 6.

Firm size and the 15-employee threshold. The 15-employee threshold is institutionally salient for two reasons that could shape our estimates. First, firms with more than 15 employees could automatically enroll collectively dismissed workers in the *Mobilità* program, while enrollment was voluntary in smaller firms (Paggiaro et al., 2009).[29] Second, the same threshold governs employment protection, as firms above it face substantially higher firing costs (Leonardi and Pica, 2013). Both mechanisms change discontinuously at the threshold and could in principle confound our estimates. To address this concern, we restrict the sample to firms with more than 15 employees, holding the enrollment and dismissal-cost regime constant across treated and control workers. This yields a 2015 migration effect of 5.4 percentage points, essentially identical to the baseline (Online Appendix Table A.8, column 2), indicating that our main results are not an artifact of the institutional mechanisms tied to the 15-employee threshold. The event-study estimates for this subsample are virtually indistinguishable from the baseline in every year (Online Appendix Figure A.8).

Bordering versus non-bordering regions. Moving costs are the central friction dampening migration in our framework (Sjaastad, 1962; Kennan and Walker, 2011). This yields a

[29]The 15-employee threshold is also used in other Italian labor market regulations. For *Mobilità* specifically, evidence suggests that most eligible workers in smaller firms voluntarily enrolled (Paggiaro et al., 2009).

testable prediction: migration responses should be stronger where relocation is cheaper. We first isolate moves between macro-areas that share a regional border, where geographic proximity lowers the cost of relocating. Online Appendix Figure A.9 and Online Appendix Table A.8 (column 3) are consistent with this prediction: the migration response reaches roughly 7 percentage points in both 2015 and 2016, well above the full-sample effects of 5.5 and 2 percentage points. A concern with this subsample is that these moves are short: workers may be commuting across the border rather than relocating, since commuting and migration are alternative ways of reaching a distant labor market (Monte et al., 2018). Commuting would not weaken our central finding: a commuter, like a mover, responds to shorter benefits by taking a job in a stronger labor market rather than remaining non-employed, which is what our workplace-based measure of mobility captures. It would, however, leave the model's mechanism untested: the model describes a relocation decision in which workers weigh access to better job offers against moving costs. The non-bordering subsample (column 4) isolates the migration margin: when the origin and destination macro-areas do not share a border, commuting is infeasible, and any increase in the probability of working elsewhere must reflect an actual move. For workers laid off in 2015, the effect remains positive and close in magnitude to the baseline, at 4.6 percentage points, statistically significant at the 10 percent level. This indicates that the baseline response captures genuine relocation. The 2016 response, by contrast, appears only among bordering pairs, consistent with the demand-side interpretation above: as Center-North demand cooled, only the cheapest moves remained worthwhile. The bordering and non-bordering estimates thus show that the migration response reflects genuine relocation and that it is strongest where moving costs are lowest, following the gradient the model predicts. Even so, a 4.6 percentage point response means most displaced workers do not relocate even once benefits became less generous, consistent with North–South moving costs—including differences in the cost of living—remaining economically significant.

As a further check, we use the underlying *province* of reemployment—a finer unit than the macro-area measure used throughout the paper—to decompose moves by distance. We split the probability of reemployment in a different province into three mutually exclusive margins: a move to a different province within the same region, a move to a different region within the same macro-area, and a move to a different macro-area. Short, commuting-consistent moves should load on the first margin, while genuine long-distance relocation should load on the last. Online Appendix Figure A.10a shows that the reform response is concentrated almost entirely in the between-macro-area margin, with the within-region and within-macro-area/between-region margins both small and statistically insignificant, corroborating the interpretation that the baseline response reflects

genuine long-distance relocation rather than commuting. As a complementary check, Online Appendix Figure A.10b plots the great-circle distance between the municipality of dismissal and the municipality of reemployment three years later, unconditional on reemployment: the reform increases this distance by 41.0 km in 2015—71 percent of the South's 2011–2014 mean distance of 57.5 km ($p < 0.01$)—and by a statistically insignificant 19.9 km in 2016, well beyond what a border-crossing commute would entail and reinforcing that the moves we measure are genuine relocations rather than commuting.

Accounting for anticipation and bunching. Treatment status in equation (5) is assigned by the calendar year of layoff, so a worker dismissed in December 2014 enters the omitted 2014 cohort while one dismissed in January 2015 enters the cohort whose coefficient β_{2015} identifies the first full year of the reform. If firms or workers anticipated the 1 January 2015 benefit cut and timed dismissals to fall just inside the more generous pre-reform regime, December entries would be relatively more numerous and January entries relatively less numerous than in a typical year—a concern for identification only if this anticipation differs systematically between the South and the Center-North, since a common seasonal pattern is absorbed by the area and year fixed effects. Online Appendix Figure A.11 shows that *Mobilità* entries are strongly seasonal in both areas (panel a) and that, although seasonality is broadly similar in the South and the Center-North, the South's share of entries rises relative to the Center-North's in December 2014 and falls in January 2015 (panel b). A formal test confirms this: the South's January entries fall significantly more than the Center-North's at the 2015 PBD cut, the only benefit-schedule change for this age group (a differential of -1.609 log points, $p < 0.001$). At the January 2016 year boundary—when PBD for workers under 40 does not change further—the same test yields a smaller, only marginally significant differential (-0.367 log points, $p = 0.086$; Online Appendix Table A.7), comparable to the differential timing also detectable around non-reform year boundaries more generally, which motivates controlling for area-specific seasonality throughout the year. To ensure this does not bias our estimates, we re-estimate equation (5) adding month-of-layoff indicators interacted with the South indicator T_i, which absorbs the area-specific within-year redistribution of layoff timing in every month, not only around the reform step. The resulting migration estimates are virtually unchanged—6.0 versus 5.5 percentage points in 2015 and 2.5 versus 1.8 in 2016 (Online Appendix Figure A.12 and Online Appendix Table A.8, column 5)—so differential anticipation of the benefit cut does not drive our results.

Alternative sample selection choices. Next, we show that the results do not depend on how the estimation sample is built. A first concern is representativeness: we observe the full workforce only at firms in the random sample or the multinational census, and we also pick up incidental spells at other firms whenever these workers later move there. Excluding only the multinationals that fall outside the random sample leaves the 2015 effect at 4.5 percentage points (Online Appendix Table A.8, column 6), close to the baseline, with a small, insignificant 2016 effect. A stricter check drops every firm outside the core random sample—multinationals and destination firms alike, losing two-thirds of the sample—and yields a similar 2015 effect of 4.8 percentage points, again with a small, insignificant 2016 effect (column 7). Representativeness is therefore not driving our results. A second concern is the baseline restriction to workers on permanent contracts in non-managerial, non-apprenticeship occupations. Relaxing it to include all workers regardless of contract type or occupation, managers and apprentices among them, yields a 2015 effect of 5.1 percentage points (column 8; Online Appendix Figure A.13). A third concern is the procedure that links *Mobilità* enrollment records to workers' administrative employment spells in order to date the layoff: widening the matching window from 30 to 90 days leaves the estimate essentially unchanged, at 5.6 percentage points (column 9; Online Appendix Figure A.13). Across all four checks, the 2015 migration response stays within one percentage point of the baseline, confirming that it is not an artifact of the sample restrictions or of the *Mobilità*-date matching procedure.

Establishment fixed effects. Firm-of-layoff fixed effects absorb the South main effect T_i only for firms that dismiss workers in a single macro-area; as noted above, 449 of 21,981 firms (about 2 percent) dismiss in both. We re-estimate equation (5) replacing firm fixed effects with establishment fixed effects, defined as the interaction of firm and macro-area, so that T_i is fully absorbed even for these firms. The 2015 effect is 4.7 percentage points (Online Appendix Table A.8, column 10), close to the baseline estimate, and the 2016 effect remains small and insignificant, confirming that the handful of firms dismissing on both sides of the macro-area boundary do not drive our results.

Ordinary UI recipients. As a final robustness check, we apply the same event-study specification to workers receiving ordinary UI benefits, a placebo group whose PBD was unaffected by the *Mobilità* reform.[30] Migration effects that mirrored our main estimates would suggest the presence of confounding trends or shocks common to all UI claimants

[30]Ordinary UI was itself reformed nationally during our window, with ASpI replacing the previous scheme in January 2013 and NASpI replacing ASpI from May 2015. Neither reform differentiated by macro-area.

rather than effects specific to the reform. Because this placebo group exhibits a positive pre-2015 trend in relocation, we additionally control for a linear trend in the migration outcome. Online Appendix Figure A.14a reports both sets of estimates (with and without the linear trend control), while Online Appendix Table A.8 (column 11) reports the trend-controlled estimates. Once the pre-existing trend is accounted for, the effect is no longer statistically significant: any apparent migration response among ordinary UI recipients is merely the continuation of that trend rather than an effect of the reform. The placebo is precisely estimated: the 95 percent confidence interval rules out migration responses above 0.5 percentage points in 2015 and 1.3 percentage points in 2016, against the 5.5 percentage point effect we estimate for *Mobilità* recipients. This supports the interpretation that the migration responses we document reflect the equalization of place-based *Mobilità* generosity and not spurious trends in the data.

Concurrent national reforms. The first reform year, 2015, coincides with two national labor market policies that were followed by the 2015 permanent-hiring boom: the Jobs Act's graded-security contract and the 2015 budget law's temporary exemption from social security contributions—both for new permanent hires. Two features of our design address the concern that these policies, rather than the PBD equalization, drive our estimates. First, both policies were national: their rules were identical in the South and the Center-North, so any common effect is absorbed by the layoff-year fixed effects, and only regionally differential effects could threaten identification. Second, the placebo tests this possibility directly. Ordinary UI recipients faced the same national policies, so if those policies alone had made Southern workers more mobile, we should see a differential migration response for this group as well. The placebo estimates show no such effect. The same is true for employment: once we net out the placebo group's pre-existing trend, the South–Center-North gap in cumulative years employed is small and statistically indistinguishable from zero in both 2015 and 2016 (Online Appendix Figure A.14b), indicating that the Jobs Act's graded-security contract did not itself generate a differential employment response across macro-areas.

Clustering of standard errors. Our baseline specification clusters standard errors at the firm-of-layoff level, but treatment is assigned at a more aggregate geographic level (South versus Center-North), so firm-level clustering could understate standard errors if outcomes are correlated within province or region of layoff. Online Appendix Table A.9 reports the main migration result clustering instead at the province of layoff (107 clusters) and at the region of layoff (20 clusters). The 2015 effect remains statistically significant under both

alternatives; because asymptotic cluster-robust inference is unreliable with only 20 regions, we also report a wild cluster bootstrap p-value for the region-clustered specification, which confirms significance at the 5 percent level ($p = 0.030$). The 2016 effect remains small and statistically indistinguishable from zero throughout ($p = 0.270$ under the region-level wild bootstrap).

6.6 Results for Older Age Groups

We estimate the same event-study specification, equation (5), for workers aged 40–49 and 50 or older. Because the reform reduced PBD in both macro-areas and narrowed, but did not eliminate, the geographic PBD gap for these groups, the Center-North is a lower-dose rather than a zero-dose comparison, and the coefficients should be read as dose–response effects rather than a clean treatment–control contrast. Figure 7 presents these effects.

Figure 7a shows that reductions in PBD translate into lower benefit receipt in a pattern that closely tracks changes in the geographic PBD gap (Figure 1). For workers aged 40–49, the gap narrows from 12 to 6 months in 2015 and remains at 6 months in 2016, corresponding to reductions in benefit receipt of approximately 108 and 129 days, respectively. For workers aged 50 or older, there is no differential effect in 2015, when PBD fell uniformly across regions, while in 2016 the narrowing of the gap by 6 months leads to a reduction of about 102 days in benefit receipt. Pre-reform coefficients are flat in both groups, supporting parallel trends.

Figure 7b shows that non-employment effects for workers aged 40–49 are cohort-dependent. In 2015, when the geographic PBD gap narrowed from 12 to 6 months,[31] the impact on non-employment is small and statistically insignificant. By contrast, workers displaced in 2016, facing the same gap but lower absolute PBD levels, spent approximately 161 fewer days in non-employment when dismissed in the South. This pattern suggests that, among older workers, more persistent benefit reductions strengthen incentives to accept available jobs. For workers aged 50 or older, non-employment effects are negligible throughout, consistent with the unchanged geographic gap in 2015 and only a modest narrowing in 2016. These workers are more insulated from PBD cuts, likely due to an age-specific institutional feature: workers aged 50 or older meeting certain eligibility criteria could access *Mobilità lunga*, an extended income support bridging unemployment and early retirement (Paggiaro and Trivellato, 2002; Paggiaro et al., 2009).

Figure 7c shows that migration effects decline with age. Workers aged 40–49 experience

[31] PBD fell from 36 to 24 months for workers dismissed in the South and from 24 to 18 for workers dismissed in the Center-North.

a statistically significant increase in migration probability of approximately 3 percentage points in 2015, about half the magnitude of the effect on younger workers, and no significant effect in 2016. Workers aged 50 or older show no migration response in either year.

The weaker effects among older workers reflect both the dose–response nature of their treatment and life-cycle patterns. Older workers in *Mobilità* have lower reemployment rates, partly because they are more likely to use the program as a bridge to retirement (Paggiaro and Trivellato, 2002; Paggiaro et al., 2009). Moreover, as workers age, moving costs are higher and the time over which to recoup the gains from relocation is shorter, weakening the migration response (Sjaastad, 1962; Kennan and Walker, 2011; Molloy et al., 2011; Bound and Holzer, 2000). These results corroborate the main findings: the reform's effects on benefit receipt, non-employment, and migration scale with the intensity of the PBD reduction.

7 Conclusion

This paper provides causal evidence on how place-based heterogeneity in UI generosity shapes labor reallocation and post-layoff job quality. We exploit a reform that progressively reduced the potential benefit duration of Italy's *Mobilità* program for workers dismissed in the South, where it was historically more generous. We compare workers dismissed in the South with those dismissed in the Center-North in a difference-in-differences design. Workers under 40 provide the cleanest treatment–control comparison: for this group, the reform fully equalized PBD across macro-areas in 2015, cutting it by 12 months in the South while leaving it unchanged in the Center-North.

We find that shorter PBD accelerates reemployment and substantially increases migration toward stronger labor markets, with mixed effects on the quality of post-layoff job matches. A 30-day cut in PBD reduces benefit receipt by about 16 days and non-employment by about 12 days, implying a strong behavioral response. Our central contribution concerns the migration response: workers under 40 exposed to a 12-month PBD reduction became 5.5 percentage points more likely to be reemployed in the Center-North, where employment rates are higher, an increase of about 70 percent. The response is concentrated among high-ability workers, consistent with their stronger expected wage offers making relocation more attractive net of moving costs, and among workers laid off in 2015, the first year of the reform. It also appears to depend on conditions in destination labor markets: when Center-North demand cooled in 2016, workers appear to have responded to lower PBD by reemploying faster locally rather than by relocating. Reduced UI generosity

thus promotes geographic reallocation mainly when stronger labor markets offer jobs.

For older workers, the reform reduced PBD in both macro-areas and narrowed, but did not eliminate, the geographical gap, generating dose–response rather than treatment–control variation. Their responses move in the same direction as those of younger workers and scale with the size of the PBD reduction they faced: reductions in benefit receipt closely track the narrowing of the geographic gap, while the migration response is about half as large for workers aged 40–49 and absent for those aged 50 or older, consistent with higher moving costs and shorter horizons over which to recoup them.

Faster reemployment comes with mixed effects on the quality of job matches. Treated workers accumulate more employment post-layoff, preserve their sectoral attachment, and show no measurable change in nominal wages. However, they appear less likely to improve on the wage premium of the firm that dismissed them, which may not translate into large long-run earnings losses (Lachowska et al., 2020). These estimates cover all workers dismissed in the South, not only those who move to the Center-North: because who moves is an outcome of the reform, we cannot estimate separate effects for movers and stayers.

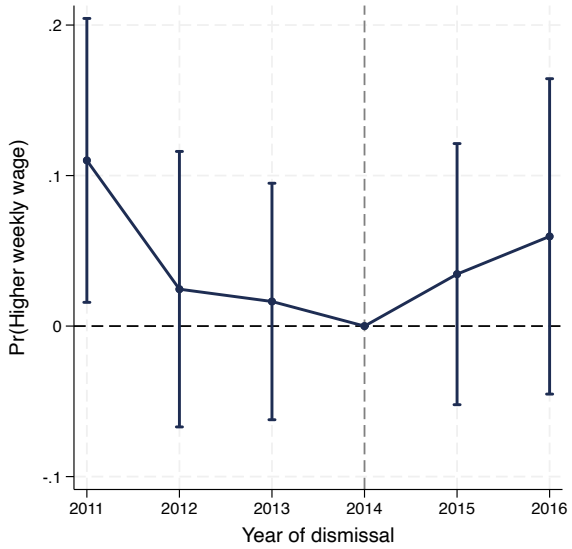
Taken together, our results suggest that geographically differentiated UI systems can generate an unintended distortion: by raising the value of remaining unemployed in weaker labor markets, place-based generosity dampens incentives for geographic reallocation and may thereby perpetuate the regional disparities it is designed to offset. Equalizing benefit durations led workers to relocate toward higher-employment areas without reducing their employment or nominal pay, while saving the public budget about €5,200 per treated worker, although workers' combined income from benefits and earnings fell over the three-year window.

Our estimates capture individual responses and are therefore partial equilibrium in nature: if large numbers of workers relocated, wages and employment would adjust in both origin and destination labor markets. Our design cannot capture these adjustments, but the finding that migration hinges on destination demand suggests this feedback would be operative: an inflow large enough to absorb destination vacancies would dampen further migration, so individual-level responses need not aggregate linearly. Quantifying these general equilibrium adjustments, which macroeconomic work on UI suggests can be substantial (Kekre, 2023), is the natural next step.

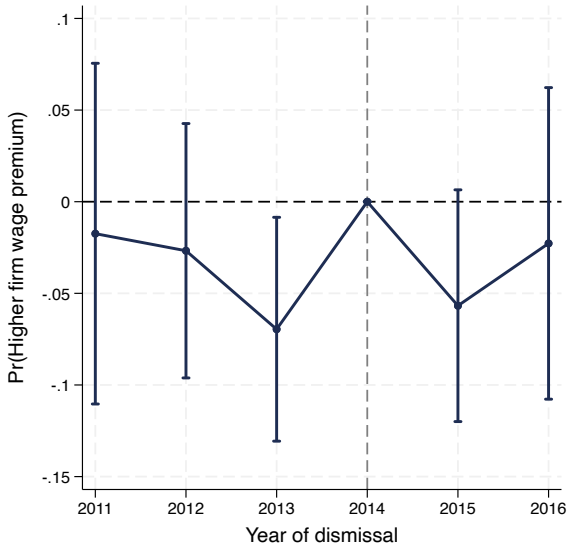
The lesson of the reform extends beyond Italy to the many countries in which unemployment protection varies across regions or labor-market segments: the spatial design of social insurance matters, and generosity concentrated where labor markets are weak carries a cost in forgone reallocation that a uniform system does not.

Figure 6: Effect on job quality dimensions

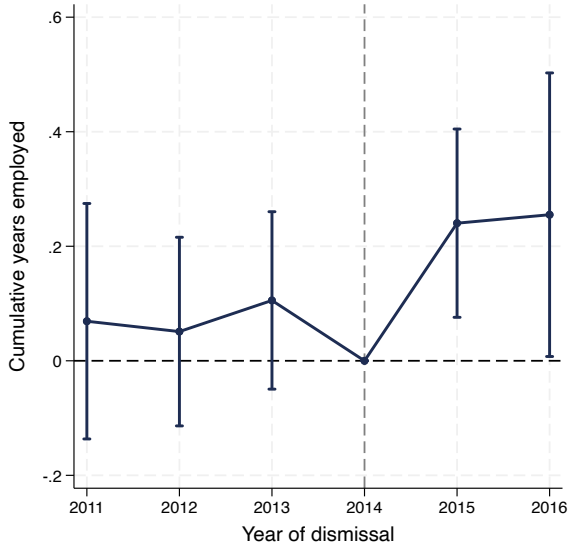
(a) Pr(higher weekly wage than before layoff)



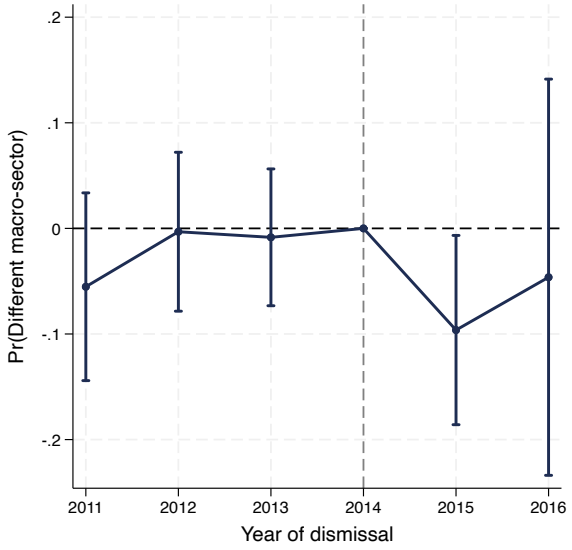
(b) Pr(higher firm premium than before layoff)



(c) Cumulative years employed



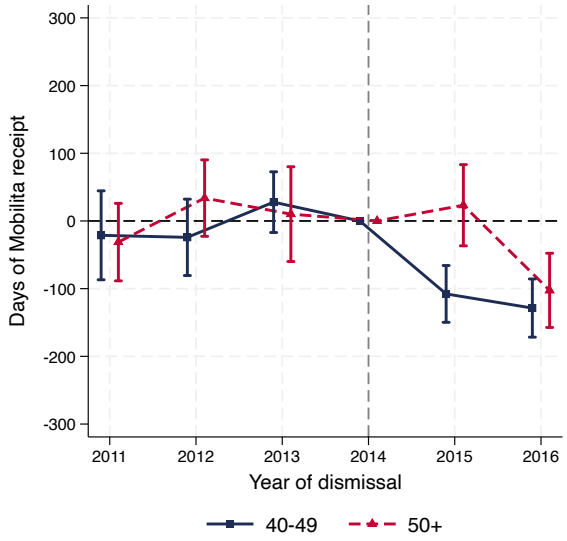
(d) Pr(diff. macro-sector than before layoff)



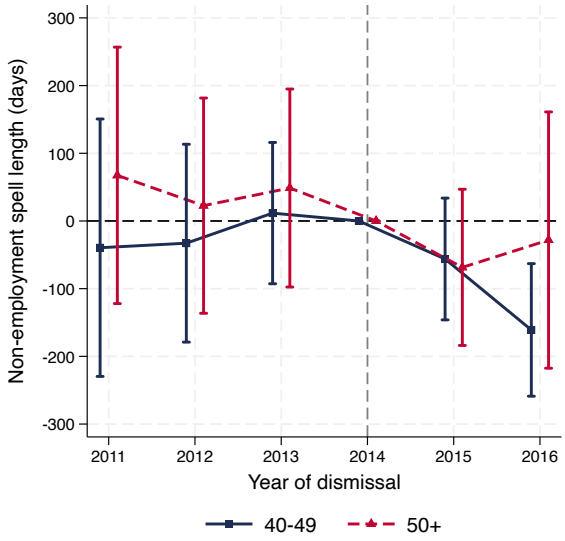
Notes. The figure reports event-study coefficients from equation (5) for workers under 40 on four dimensions of post-layoff job quality. Panel (a): probability that the weekly wage exceeds the pre-layoff wage. Panel (b): probability of being at a firm with a higher AKM wage premium than the pre-layoff firm. Panel (c): cumulative years employed in the three years following dismissal. Panel (d): probability of working in a different macro-sector from the pre-layoff job. Panels (a), (b), and (d) are measured three years after the layoff and estimated on workers employed at that date, with panel (b) further restricted to firms with an estimated AKM premium; panel (c) includes all workers, with zero for years without employment. All regressions include firm-of-layoff and layoff-year fixed effects, a quadratic in age, and indicators for female and migrant workers. Standard errors are clustered at the firm-of-layoff level. Online Appendix Table A.5 reports the corresponding point estimates.

Figure 7: Effects on benefit receipt, non-employment duration, and migration for older workers

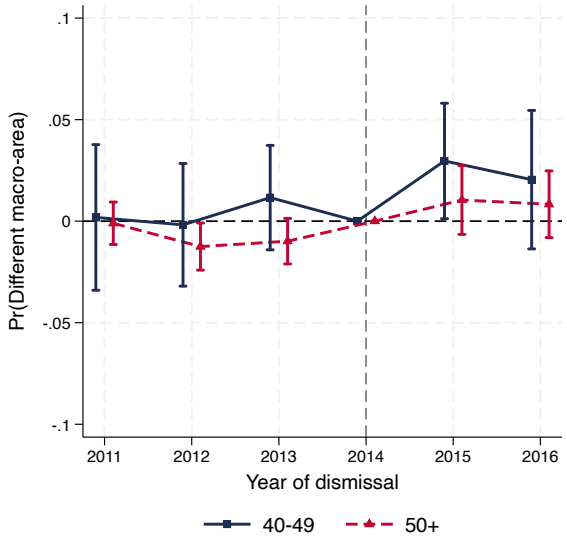
(a) Days of *Mobilità* receipt



(b) Non-employment spell length (days)



(c) Pr(different macro-area)



Notes. The figure presents event-study coefficients from equation (5) for workers aged 40–49 and 50+. Panels (a), (b), and (c) show effects on days of *Mobilità* receipt, non-employment spell length, and the probability of reemployment in a different macro-area, respectively. All regressions include firm-of-layoff and layoff-year fixed effects, a quadratic in age, and indicators for female and migrant workers. Standard errors are clustered at the firm-of-layoff level.

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