Cities with Benefits*

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Abstract

This paper studies how local economic conditions shape the optimal design of unemployment benefits across space. I develop a theoretical framework that characterizes the insurance value and fiscal costs of unemployment benefits across locations as a function of (i) local prices, (ii) behavioral responses in unemployment duration and (iii) distortions to location choice. I estimate the fiscal costs of the unemployment duration and migration responses using novel quasi-experimental variation from Germany's Hartz IV reform, which tied benefit levels to local rents for long-term unemployed individuals. Unemployment durations respond similarly to benefit changes in high- and low-price locations. However, higher benefits in expensive regions attract jobseekers, raising fiscal costs. My findings imply that indexing unemployment benefits to local prices makes benefits excessively generous in high-price locations, and that redistribution towards regions with lower prices yields sizable welfare gains.

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1 Introduction

Unemployment benefits are a fundamental part of modern welfare states. Across OECD countries, governments spend around 1% of GDP on unemployment benefits, making them one of the largest public transfer programs for the working-age population (OECD, 2024). Designing unemployment benefits involves a key trade-off: more generous benefits provide greater insurance in case of job loss but reduce incentives to return to work, thereby raising fiscal costs (Baily, 1978; Chetty, 2006). To achieve the right balance between insurance and employment incentives, unemployment benefits should be tied to conditions that affect this trade-off, such as jobseeker characteristics or aggregate economic conditions (Schmieder and von Wachter, 2016; Kroft and Notowidigdo, 2016).[1]

Local economic conditions are an important yet understudied factor in shaping both sides of this trade-off. Regional differences in costs of living affect the insurance value of unemployment benefits: the same nominal benefits afford less consumption in expensive cities like New York and San Francisco, compared to regions with low costs of living (Diamond and Moretti, 2024). At the same time, local job-finding opportunities shape the fiscal costs of unemployment benefits. In regions with scarce job opportunities, jobseekers will struggle to return to work regardless of how intensively they look for jobs. In such areas, higher benefits may reduce search effort but have only small effects on unemployment duration, as jobs are difficult to find even with intensive search (Austin et al., 2018). However, higher benefits in economically depressed areas could attract jobseekers from other regions, increasing fiscal costs (Jofre-Monseny, 2014). If the value and costs of unemployment benefits vary with local economic conditions, a natural question arises: should unemployment benefits vary across space—and, if so, how?

In this paper, I address these questions in three steps. First, I develop a theoretical framework to study how optimal unemployment benefits should vary across space. Unemployment benefits are particularly valued by jobseekers in locations with high costs of living ("cities"). Yet, higher benefits in cities can raise fiscal costs through behavioral distortions along two margins—search effort and migration—whose fiscal impact depends on local job-finding opportunities. In the second step, I estimate the size of these two distortions using novel quasi-experimental variation from Germany's Hartz IV reform in 2005. In the last step, I combine theory and empirics to evaluate the welfare effects of redistributing benefits across space.

I begin by developing a theoretical framework that captures the trade-off between insurance and fiscal costs when designing unemployment benefits across space. In the model, risk-averse jobseekers choose their search effort and the location where they look for jobs. Locations vary in their prices, wages, and how efficient search effort is in generating job offers (e.g. Heise and Porzio, 2023). From the model, I derive the welfare effects of budget-neutral reforms that redistribute benefits across space. Higher benefits in expensive locations can improve welfare if the local unemployed value an additional euro of benefits more. However, redistributing benefits from low- to high-price locations may raise fiscal costs through two behavioral distortions. First, jobseekers exert less search effort when receiving higher benefits. Lower effort prolongs unemployment spells, but the size of this effect depends on local search efficiency. For instance, if labor markets in cities like New York and San Francisco are highly efficient in bringing jobseekers back to work, raising benefits in cities may lead to

[1] Policymakers likewise emphasize the importance of linking unemployment benefits to individual or aggregate conditions. According to the OECD (2023), "[the] design of unemployment benefit policies requires finding a delicate balance, between providing adequate support for those who lose their jobs, and maintaining work incentives. Finding this balance requires policies that [...] are carefully calibrated to individual and labour market conditions."

larger increases in unemployment duration—and consequently, greater fiscal costs. Second, jobseekers may migrate toward regions with higher benefits. Migration is welfare-relevant if movers to high-price locations impose additional fiscal costs at the destination. The model shows that three sufficient statistics summarize the fiscal costs of behavioral distortions when reallocating benefits to high-price locations: (i) differences in unemployment duration responses to changes in benefits between high- and low-price locations, (ii) the migration elasticity in high-price regions with respect to benefits, and (iii) the fiscal costs caused by movers.

In the second part of my analysis, I estimate these three sufficient statistics using quasi-experimental variation from Germany's Hartz IV reform—one of the most significant unemployment benefit system reforms in recent decades.[2] Hartz IV restructured benefits for the long-term unemployed—jobseekers who had exhausted their initial unemployment insurance and subsequently relied on unemployment assistance. Before the reform, the benefit level of unemployment assistance was primarily determined by labor earnings in the years before the unemployment spell. After the reform, long-term unemployed workers received a location-specific housing allowance and a lump-sum payment based on household composition. For a given unemployed worker, this housing allowance made benefits *relatively* more generous in high-price locations than in low-price ones, irrespective of the claimant's prior employment history.[3] To analyze the effects of regionally varying benefits, I combine newly digitized data sources and administrative records with three sources of quasi-random variation generated by Hartz IV: (i) a kinked benefit schedule, (ii) differential reform exposure depending on the risk of entering long-term unemployment, and (iii) age-based variation in access to housing assistance.

I start the empirical analysis by studying how benefit generosity affects local unemployment durations, and whether these effects vary across locations depending on local prices. I find that higher benefits prolong unemployment durations equally in high- and low-price locations. To identify these effects, I combine hand-collected data on local benefit generosity after Hartz IV with administrative data from a 4% random sample of long-term unemployment benefit claims. I use a fuzzy regression kink design, exploiting a reform-induced kink in the unemployment benefit schedule as a function of the claimants' housing costs (Card et al., 2015). I find that a €10 (2.4%) increase in benefits raises unemployment duration by about five days (1.5%). I document very similar duration responses between high- and low-price locations, with elasticities of 0.64 and 0.58, respectively. Moreover, longer unemployment durations do not translate into higher reemployment wages in either location. Using additional survey information on job search for a matched subset of long-term unemployed workers, I show that search efficiency is uncorrelated with local price levels, which rationalizes why durations respond similarly to benefit changes in both types of locations. These findings suggest that more generous benefits in high-price locations entail little additional fiscal cost from duration responses.

In the second part of the empirical analysis, I show that the unemployed move toward regions with higher benefits. I use a sample of displaced workers around the Hartz IV reform, drawn from a random 2% sample of labor market biographies in Germany. Using a dynamic difference-in-differences framework, I compare the location choices of displaced workers with high predicted risk of entering

[2] *The Economist* called Hartz IV "Germany's most important labour-market reform since the war" (The Economist, 2004). Similarly, the OECD characterized the broader Hartz reforms as "the most active and controversial series of reforms within the OECD area." (OECD, 2007, p.173)

[3] On average, the reform led to lower long-term unemployment benefits for most claimants. Price (2019) estimates that displaced jobseekers would on average expect a 24% drop in benefits (4.4% in net household income), relative to benefit entitlements before the reform. In the empirical analysis, I separate the effects of general benefit cuts and regionally varying benefit levels by exploiting age-based differences in access to housing assistance, which was introduced one year after the general reform.

long-term unemployment with those of displaced workers with low predicted risk. The results show that migration rates only increase among jobseekers with high chances of relying on long-term unemployment benefits, consistent with greater exposure to the reform among these workers. I estimate a migration elasticity of 0.26 with respect to the local level of unemployment benefits. To confirm that regional variation in benefits drives the observed migration responses, I exploit age-based differences in access to housing assistance—the main regional component of benefits after the Hartz IV reform. From February 2006 onward, jobseekers younger than 25 were no longer automatically entitled to housing assistance after moving. Comparing jobseekers in a narrow age window around this cutoff, before and after the reform, I show that access to housing assistance explains migration choices toward high-price locations.

In the third empirical exercise, I quantify the fiscal costs of moving toward high-price destinations. I find that movers generate €1193 (11.5%) higher costs in unemployment benefits during the first three years after relocating. To identify this effect, I compare the labor market trajectories of displaced movers with those of observationally similar displaced stayers in a matched event-study design. I find that movers to expensive locations impose higher fiscal costs, which persist up to six years after relocating. Movers exhibit lower re-employment rates, especially in the first year after relocating, and I provide evidence consistent with initial adaptation costs to the new local labor market. Together with the migration elasticity estimate, the additional fiscal costs of movers imply that providing more generous benefits in high-price locations raises fiscal costs.

Combining theory and empirical estimates yields the final key result of the paper: indexing unemployment benefits to local prices, as implemented after Hartz IV, results in excessively generous benefits in high-price locations. Welfare-improving reforms should therefore redistribute benefits toward locations with lower prices. Quantitatively, I estimate money-metric welfare gains of €0.16 per €1 transferred to the unemployed in low-price locations. Two forces contribute in roughly equal magnitudes to this result. First, utility gains among the unemployed in rural locations outweigh the utility losses among urban jobseekers (€0.09 per €1). Second, redistributing benefits toward low-price locations reduces fiscal externalities of unemployment benefits (€0.07 per €1). This fiscal improvement arises from reduced migration toward high-price locations and from slightly smaller duration responses in low-price regions.

Contribution to the literature. This paper contributes to several strands in the literature. First, a large literature studies the optimal design of unemployment benefits (Chetty, 2006; Schmieder and von Wachter, 2016), and how benefits should vary by individual characteristics (Michelacci and Ruffo, 2015; Andrews and Miller, 2013; Chetty, 2008), over the business cycle (Schmieder et al., 2012; Kroft and Notowidigdo, 2016; Piqueras, 2025) and during the unemployment spell (Kolsrud et al., 2018). I contribute to this literature by showing how local economic conditions affect the optimal design of unemployment benefits. Most closely related, Jung et al. (2023) study optimal regional unemployment benefits in a spatial search and matching model. In their optimality formula, local benefits are determined by local search externalities, moral hazard frictions as well as migration responses. My contribution relative to their work is twofold. First, I show that price differences—which are absent in their model—serve as an additional rationale for regionally-varying benefits. Through the lens of my model, price differences, *ceteris paribus*, justify sizable differences in nominal benefits to smooth consumption levels across jobseekers in different locations: a 10% difference in local price levels leads to 6.5% higher benefits for commonly found degrees of risk aversion. Second, I provide the first esti-

mates of local duration and migration responses using quasi-experimental variation. The migration elasticity in Jung et al. (2023), obtained by matching observed moving rates among the unemployed, is of similar magnitude to my reduced-form estimate.

Second, this paper adds to the body of work on (optimal) place-based policies (e.g. Fajgelbaum and Gaubert, 2020; Kline and Moretti, 2013; Gaubert et al., 2024), and more specifically, how social transfers should vary across space (Colas and McDonough, 2025). Glaeser (1998) studies whether transfer payments should be indexed to local prices, depending on spatial differences in marginal utilities and migration. I show theoretically that optimal regional transfers additionally depend on local responses in unemployment duration—a key dimension as more generous transfers have been linked to lower employment incentives across a range of programs (Autor et al., 2019; Jacob and Ludwig, 2012; Landais, 2015). While I find unemployment benefits to affect labor supply similarly across space for the long-term unemployed in Germany, this channel may matter more in settings with large regional differences in employment prospects for transfer recipients (Austin et al., 2018).

Third, I contribute to the literature on the welfare magnet hypothesis—the extent to which welfare programs attract potential claimants through in-migration. Existing work shows that more generous welfare programs attract international migrants (Razin and Wahba, 2015) and refugees (Agersnap et al., 2020), but finds mixed evidence for within-country mobility. Ferwerda et al. (2024) and McKinnish (2005) document that potential claimants move toward regions with more generous social assistance and family cash transfers, respectively, whereas McPherson (2024) finds no welfare-magnet effect of U.S. health insurance programs. The only study examining spatial variation in unemployment benefits, Jofre-Monseny (2014), documents a moderate migration response to local increases in benefit generosity across two Spanish regions. My contribution to this literature is twofold. First, I provide the first causal estimate of the migration elasticity with respect to unemployment benefits. Second, I estimate the fiscal costs of movers, which determine whether migration toward high-benefit locations has welfare consequences.

Finally, this paper contributes to the literature that studies the determinants of the spatial distribution of unemployment. Previous work has highlighted the roles of employer sorting (Bilal, 2023), persistence in labor demand shocks (Amior and Manning, 2018) or productivity differences across space (Kuhn et al., 2022) for explaining regional variation in unemployment rates. Other papers have argued that providing unemployment benefits—and social transfers more generally—dampens outmigration from areas with depressed economic conditions, thereby reinforcing regional inequalities (e.g. Notowidigdo, 2020; Hershbein and Stuart, 2023). I introduce a new determinant of regional unemployment: spatial variation in benefit *levels*. I show theoretically that regional unemployment is shaped not only by migration toward higher-benefit regions, but also by how local jobseekers adjust their unemployment duration in response to benefit generosity. Through the lens of my model and based on my empirical estimates for Germany, higher benefits in high-price locations may have increased the rate of jobseekers who enter long-term unemployment by 0.7 percentage points (6%).

Outline. Section 2 presents the theoretical framework. Section 3 explains the institutional context and Section 4 introduces the data. In Section 5, I estimate the local responses in unemployment duration to benefit changes in high- and low-price locations. Section 6 studies the effects of regionally-varying benefits on location choices and Section 7 estimates the associated fiscal costs of migrating. Section 8 combines theory and empirical estimates to calculate welfare effects. Section 9 concludes.

2 Theoretical Framework

This section presents a theory of how unemployment benefits should vary across space. It combines the canonical optimal social insurance framework as in Chetty (2006) with a static location choice model (e.g. Gaubert et al., 2024). The framework is in partial equilibrium, meaning that prices, wages, amenities and job-finding rates do not respond to the number of jobseekers in a location. In my empirical analysis, I study long-term unemployed workers in Germany, who account for roughly 5% of the labor force. This relatively small share motivates a partial-equilibrium focus.

2.1 Model Setup

A unit mass of jobseekers chooses a location j in which to search for a job, and how much search effort to exert. The model is static, and individuals enter the single period unemployed. Jobseekers are homogeneous except for their preferences over locations ϕ. They can only search for jobs in the location that they first choose and cannot apply to jobs elsewhere. Not allowing for directed search is motivated by the geographic scope of search in my data and consistent with previous work (e.g. Manning and Petrongolo, 2017): 80% of actively searching jobseekers do not send any applications to jobs more than 100 kilometers away within a month, in contrast to approximately ten local applications.

When employed (with probability s_j), workers receive utility from their real wage $U(w_j/p_j)$. If they instead remain unemployed (with probability $(1-s_j)$), they receive utility from real unemployment benefits $U(b_j/p_j)$. The job-finding rate s_j is the product of exogenous search efficiency z_j and the effort choice $e_j(b_j)$:

$$s_j = z_j \cdot e_j(b_j) \tag{1}$$

Through variation in search efficiency z_j across regions, this formulation captures regional differences in job-finding opportunities even for identical levels of search effort. This is consistent with recent evidence documenting substantial cross-regional variation in job-finding rates (e.g., Kuhn et al., 2022; Moretti and Yi, 2024).

Effort choice. Search effort causes disutility $\psi(e_j)$, a strictly increasing and strictly convex function. In equilibrium, jobseekers equate the marginal benefit with the marginal cost of search:

$$\psi'(e_j) = z_j \cdot \left[U\left(\frac{w_j}{p_j}\right) - U\left(\frac{b_j}{p_j}\right) \right] \tag{2}$$

Given the exogenous search efficiency (z_j), nominal wage (w_j) and price level (p_j) in a location, more generous benefits reduce search effort because they narrow the utility gap between employment and unemployment. Accordingly, more generous benefits increase the probability of remaining unemployment. Since effort is unobservable to the policymaker, benefits cannot be conditioned on it, which gives rise to moral hazard (e.g. Chetty, 2006).

Location choice. Anticipating the equilibrium effort choice, jobseeker i rationally expects utility

$$v_j(i) = s_j(b_j) \cdot U\left(\frac{w_j}{p_j}\right) + (1 - s_j(b_j)) \cdot U\left(\frac{b_j}{p_j}\right) - \psi(e_j(b_j)) + a_j + \phi_j(i) \tag{3}$$

from choosing location j. a_j captures exogenous amenities that increase the appeal of a location for all

residents. $\phi_j(i)$ are idiosyncratic taste shocks for locations, drawn i.i.d. from a Gumbel distribution with inverse scale parameter σ. In equilibrium, location j then hosts a fraction of jobseekers

$$N_j(\boldsymbol{b}) = \frac{e^{\sigma \cdot v_j(b_j)}}{\sum_k e^{\sigma \cdot v_k(b_k)}} \tag{4}$$

where v_j denotes the common appeal of location j:

$$v_j = s_j(b_j) \cdot U\left(\frac{w_j}{p_j}\right) + (1 - s_j(b_j)) \cdot U\left(\frac{b_j}{p_j}\right) - \psi(e_j(b_j)) + a_j \tag{5}$$

Jobseekers' welfare equals the ex-ante expected utility—before taste shocks are realized—from optimal location choice:

$$W(\boldsymbol{b}) = \frac{1}{\sigma} \ln\left(\sum_j e^{\sigma \cdot v_j(b_j)}\right) \tag{6}$$

The planner's problem. I assume that the planner has access to a fixed federal budget on unemployment benefits $\overline{B}$, and decides how to allocate this budget across regions to maximize jobseekers' welfare. A national budget constraint reflects that most unemployment benefit systems are financed at the national level. The benefits I study in the empirical context are financed from general government revenues rather than payroll contributions. This motivates the choice not to model benefit financing through taxes on the employed.

With these assumptions, I now formalize the objective function of the planner when choosing region-specific unemployment benefits $\{b_j\}_{j=1,\dots J}$. Specifically, the planner chooses $\{b_j\}_{j=1,\dots J}$ to solve the following constrained maximization problem:

$$\begin{aligned} &\max_{\{b_j\}_{j=1,\dots J}} W = \max_{\{b_j\}_{j=1,\dots J}} \frac{1}{\sigma} \ln\left(\sum_j e^{\sigma \cdot v_j(b_j)}\right) \\ &\text{s.t.} \quad \sum_j (1 - s_j(b_j)) \cdot N_j(\boldsymbol{b}) \cdot b_j \leq \overline{B} \end{aligned} \tag{7}$$

Increasing benefits in a location directly raises the utility of workers who are unemployed in that location. Moreover, the planner internalizes two behavioral responses to an increase in b_j. First, jobseekers might exert lower search effort, increasing the number of benefit recipients in j. This is the canonical behavioral response and often referred to as the moral hazard cost of providing unemployment insurance (e.g. Le Barbanchon et al., 2025). Second, more generous benefits may lead to in-migration. This channel matters to the extent that movers consume different benefit levels at their destination compared to the region they would have chosen in the absence of the benefit increase. While both behavioral responses do not have any first-order welfare effects as a consequence of the envelope theorem, they matter because of changes in the fiscal cost for the federal government. I will now formally characterize how these behavioral responses shape the optimal design of unemployment benefits across space.

2.2 Welfare Effects of Regional Unemployment Benefit Reforms

I first study the welfare impact of reforms, which reallocate unemployment benefits across in a budget-neutral way. I then turn to reforms in a model with two locations—a city and a rural region—which I will use for the empirical application. Before studying welfare implications, I make one definition

that simplifies the subsequent notation:

Definition 1. *Let ϵ_{1-s_j,b_j} denote the elasticity of unemployment duration with respect to local benefit changes:*

$$\epsilon_{1-s_j,b_j} \equiv \frac{d(1-s_j)}{db_j} \cdot \frac{b_j}{(1-s_j)} = -z_j \frac{de_j}{db_j} \cdot \frac{b_j}{(1-z_j e_j)} \tag{8}$$

Equation (8) captures that more generous benefits extend unemployment spells. $(1-s_j)$ can be interpreted as the fraction of the single period in the model that jobseekers remain unemployed. Importantly, this effect is—*ceteris paribus*—expected to be stronger in locations with high search efficiency z_j. Intuitively, when search effort is highly productive, even small reductions in effort can generate sizable increases in the time jobseekers spend in unemployment. With this definition in hand, I now turn to the main theoretical result of this paper:

Proposition 1. *A balanced budget reallocation of unemployment benefits between two locations c and r such that $\partial \ln b_c > 0 > \partial \ln b_r$ increases jobseekers' welfare if and only if*

$$\sigma \cdot (1-s_r) b_r + \frac{1+\epsilon_{1-s_r,b_r}}{U'\left(\frac{b_r}{p_r}\right)/p_r} > \sigma \cdot (1-s_c) b_c + \frac{1+\epsilon_{1-s_r,b_r}}{U'\left(\frac{b_c}{p_c}\right)/p_c} \tag{9}$$

Proof of Proposition 1. See Appendix C.1. □

This proposition demonstrates that redistributing benefits from location r to c improves welfare, if (and only if) the fiscal externalities of unemployment benefits, relative to their insurance value, are larger in r compared with c. Fiscal externalities arise for two reasons: migration (σ-terms), and local moral hazard $(1+\epsilon_{1-s_j,b_j})$. The local social value of unemployment benefits depends on the marginal utility of consumption when unemployed $(U'(b_j/p_j))$, and local prices. The effect of higher prices is *ex-ante* ambiguous. All else equal, higher local prices p_j increase the marginal utility of consumption, but imply only a small increase in consumption $(1/p_j)$ for each €1-transfer towards j. Which effect dominates depends on the curvature of consumption utility U.

This proposition nests prominent results in the literature on optimal unemployment benefits and the design of welfare benefits across space. With no geographic mobility, the inequality from Equation (9) can be expressed as the classic insurance-efficiency trade-off in the design of unemployment benefits (e.g. Baily, 1978; Chetty, 2006). Specifically, in the limit of no mobility ($\sigma \to 0$), redistributing benefits from r to c improves welfare if and only if:

$$\frac{U'\left(\frac{b_c}{p_c}\right)/p_c}{U'\left(\frac{b_r}{p_r}\right)/p_r} > \frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}} \tag{10}$$

A transfer of benefits from r to c is socially desirable if the gains among unemployed workers in c exceed the losses in r and are not offset by higher fiscal costs from moral hazard.

In the case of perfect mobility ($\sigma \to \infty$), the planner redistributes benefits toward the location where jobseekers impose a lower fiscal burden (i.e. to c if and only if $(1-s_r)b_r > (1-s_c)b_c$). This case mimics the results on the design of welfare benefits or local subsidies across space (e.g. Glaeser, 1998; Austin et al., 2018).

Mapping framework to data. To estimate the welfare effects of reallocating benefits across space, I make the following assumptions. First, I consider unemployment benefits across two locations—a city c and a rural region r—with the city being more expensive ($p_c > p_r$). Second, I impose a common functional form on consumption utility $U(\cdot)$—constant relative risk aversion (CRRA) with coefficient of relative risk aversion ρ (e.g. Landais and Spinnewijn, 2021; Piqueras, 2025). Under these assumptions, the following corollary summarizes the welfare effects of redistributing benefits from the rural region r to the city c.

Corollary 1. *Consider two locations c and r, and CRRA utility U over consumption. A balanced budget reallocation of unemployment benefits between two locations c and r such that $\partial \ln b_c > 0 > \partial \ln b_r$ increases jobseekers' welfare if and only if*

$$\left(\frac{\frac{b_c}{p_c}}{\frac{b_r}{p_r}}\right)^{-\rho} \cdot \left(\frac{p_c}{p_r}\right)^{-1} > \underbrace{\frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}}_{\text{Duration Responses}} + \underbrace{\frac{\epsilon_{N_c,b_c}}{(1+\epsilon_{1-s_r,b_r})N_r}}_{\text{Migration Response}} \cdot \underbrace{\frac{(1-s_c)b_c-(1-s_r)b_r}{(1-s_c)b_c}}_{\text{Fiscal Effect of Movers}_{r\to c}} \tag{11}$$

where ϵ_{N_c,b_c} captures the migration elasticity with respect to benefit changes in the city:

$$\epsilon_{N_c,b_c} \equiv \frac{dN_c}{db_c} \cdot \frac{b_c}{N_c} \tag{12}$$

Proof of Corollary 1. See Appendix C.2. □

The left-hand side captures the value of smoothing consumption across jobseekers in the two locations, which depends on two terms. On the one hand, higher benefits in the rural region compared with the city favor redistribution towards the urban region. On the other hand, the effects of price differences depends on the degree of risk aversion ρ (i.e. more generally the curvature of U). All else equal, higher prices in the city make transfers toward it desirable if $\rho > 1$, and undesirable if $\rho < 1$. In the famous case of log utility ($\rho \to 1$), the price ratio itself does not matter for the welfare impact of the reform (Campos and Reggio, 2016). Most previous estimates find $\rho > 1$, which, starting from nominally constant benefits ($b_c = b_r$), make increasing benefits in the high-price location desirable. I quantify the left-hand side of Equation (11) using data on prevailing benefits and prices, combined with an estimate of the coefficient of relative risk aversion from the literature.

The right-hand side captures the fiscal costs of redistributing benefits towards the city, which is shaped by two behavioral responses. First, changes in benefits influence search effort and, therefore, unemployment rates ($1-s_j$), which correspond to the share of the single model period that jobseekers spend unemployed (i.e. duration). If transfers from r to c prolong unemployment spells by more in the city, than they shorten spells in the rural region, they create additional fiscal costs. Second, more generous benefits in the city may attract jobseekers from the rural region. Importantly, the migration response only matters to the extent that movers impose different fiscal costs in the city relative to the rural region. This fiscal effect of movers can be directly expressed as the (relative) difference in benefits received between the two locations, which depends on benefit levels (b_j) and the times spent in unemployment ($1-s_j$). To quantify the right-hand side of Equation (11), I need to empirically estimate the following three sufficient statistics: (i) differences in duration responses to changes in unemployment benefits between high- and low-price locations, (ii) the migration elasticity of high-price regions with respect to benefits, and (iii) the fiscal costs caused by movers. I now outline the institutional context that I exploit to estimate each of these three sufficient statistics.

3 Institutional Context: Germany's Hartz IV Reform

I begin by outlining the general context of the Hartz IV reform before describing in detail how it altered long-term unemployment benefits. Because geographic mobility is a key outcome in my empirical analysis, I also discuss the specific mobility restrictions faced by individuals depending on the type of unemployment benefits they receive.

Reform context. Due to the growing number of workers relying on long-term unemployment assistance, the German government introduced a comprehensive set of labor market reforms—the Hartz reforms—in the early 2000s.[4] Hartz I to III deregulated the temporary employment sector, expanded tax incentives for mini-jobs, and reorganized the Federal Employment Agency. Hartz IV—arguably the centerpiece of the reform package and described as "Germany's most important labour-market reform since the war" (The Economist, 2004)—fundamentally changed the structure of long-term unemployment benefits. The new benefit system was, on average, less generous and no longer linked to previous labor earnings. Instead, payments were calculated in line with pre-existing welfare benefits. Under the new rules, long-term unemployed individuals received a lump-sum benefit together with a housing subsidy, capped at a locally determined rent ceiling that typically covered only lower-quality housing.

The reform announcement in mid-2004 triggered a wave of public protests. In addition to the benefit reductions, the housing subsidy component proved particularly controversial. Opposition groups—especially tenant-protection associations (*Mietervereine*)—voiced strong concern about the provision that housing subsidies would cover costs only up to a "locally appropriate" ceiling. They warned that such a cap could force many recipients to relocate to cheaper, lower-quality dwellings.[5] Despite these public concerns, the reform came into effect on January 1, 2005. Apart from minor adjustments and relabeling, the current long-term unemployment benefit system still closely follows the design introduced under Hartz IV.

3.1 Changes to Long-Term Unemployment Benefits

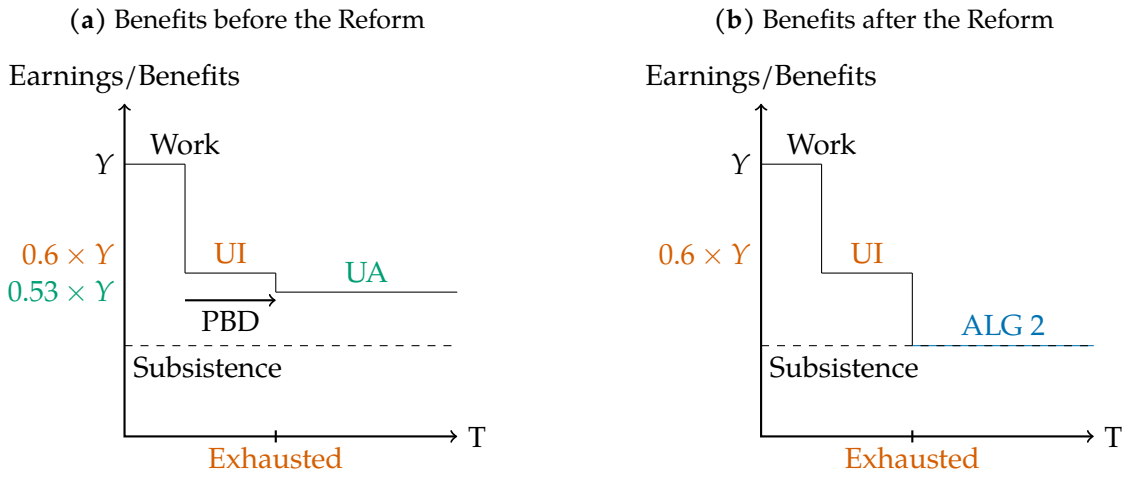
Benefits before Hartz IV. Before the reform, Germany's unemployment benefit system consisted of three tiers. Unemployed workers initially received time-limited unemployment insurance (UI, *Arbeitslosengeld*) and, if they had not returned to work upon exhausting UI, transitioned into time-unlimited, means-tested unemployment assistance (UA, *Arbeitslosenhilfe*), as illustrated in Figure 1a. If jobseekers received benefits below the (local) subsistence level, they could apply for supplementary social assistance (SA, *Sozialhilfe*), which covered the difference between their assessed needs and their household income. As of 2004, only 8% of unemployment benefit recipients simultaneously claimed social assistance (Federal Statistical Office, 2004). I therefore focus on the reform-induced changes to unemployment benefits for workers whose benefit entitlements exceeded their subsistence needs. For completeness, Figure D.1 shows the corresponding changes for workers with benefits below subsistence.

Workers receiving UI benefits were entitled to 60% (67% if they had dependent children) of their previous after-tax labor earnings. The potential benefit duration (PBD) depended on the worker's

[4] See Appendix Figure B.1 for trends in the stock of unemployed workers in the months leading up to the reforms.

[5] See as an example the corresponding press releases from the Mieterverein Witten and the Mieterforum Ruhr (accessed 10.04.2025).

Figure 1: Unemployment Benefits before and after the Reform



Notes: Panel (a) depicts the structure of unemployment benefits before the reform for a single worker without children, whose unemployment benefits are above subsistence. The red line illustrates the change in benefits from before to after the reform. Panel (b) shows the corresponding situation for unemployed workers after the reform. Abbreviations used: unemployment insurance (UI), unemployment assistance (UA), Arbeitslosengeld 2 (ALG 2), potential benefit duration (PBD).

age at UI entry and on the number of months worked during the seven years preceding the UI spell.[6] After exhausting their initial UI entitlement, long-term unemployed workers received 53% (57% if they had dependent children) of their previous after-tax labor earnings, conditional on meeting the means-testing requirements. In principle, UA benefits could be received until retirement, subject to annual means testing.

Benefits after Hartz IV. Hartz IV merged UA and SA together into a single system of subsistence benefits—*Arbeitslosengeld 2* (henceforth ALG 2)—which was no longer linked to previous labor earnings. Instead, long-term unemployed workers received lump-sum cash benefits (€345 in West, €331 in East Germany in 2005 for a single-person household)[7] in addition to coverage of their monthly rent and heating expenses (*Kosten der Unterkunft* (*KdU*)). During the first six months of receiving ALG 2, actual monthly rent expenditures were covered in full. After this period, housing subsidies were capped at the locally defined "appropriate" rent ceilings, which were typically set at the municipality×household size×year level. These ceilings were usually based on local rent indices and calibrated to cover the lower-price segments of the housing market. Appendix G provides two examples of rent ceilings as they were used in practice. In sum, the new formula implied that benefits would now depend mainly on the claimant's rent expenditure and household composition (Bundesagentur für Arbeit, 2005, p.4).

Changes in benefits. The change in long-term unemployment benefits affected jobseekers in mainly

[6]The formula for PBD in months is:

$$\text{PBD} = \min[\overline{P}(a), 2 \cdot \text{floor}(m/4)] \tag{13}$$

where $\overline{P}(a)$ is an age-specific maximum duration, and m represents months of working before unemployment. $\overline{P}(a)$ takes on the value 12 for workers below 45 (at UI entry), 18 for $a \in [45, 47)$, 22 for $a \in [47, 52)$, 26 for $a \in [52, 57)$ and 32 for $a \geq 57$. The second term shows that an additional four months of working before unemployment adds two months of potential benefit duration, if the age-cutoff is not binding.

[7]The €345 in West and €331 in East Germany refer to the base monthly allowance for a single-person household. Married couples would in addition receive 90% of that base allowance per spouse, and 60% (80%) for each dependent children below (above) 15.

two ways. First, benefits were on average less generous after the Hartz IV reform. Price (2019) estimates that the average UI recipient would experience an income loss of 4.4% if she entered ALG 2, relative to the pre-reform level of long-term unemployment benefits. In general, workers with high pre-unemployment labor earnings or high spousal income were most adversely affected by the reform[8] Second, following the reform, benefit levels changed if jobseekers relocated to areas with different price levels while unemployed, although some exceptions to this rule applied (see Section 3.2). In contrast, pre-reform long-term unemployment benefits were tied to prior earnings and thus remained constant regardless of the jobseeker's location during unemployment.[9] Thus, prior to the reform, moving to a more expensive labor market could result in real income losses for jobseekers.

To illustrate the real income losses associated with relocation, I simulate long-term unemployment benefits under the rules in place before and after the reform. I focus on real benefits in line with my location choice model in Section 2.1 and present nominal benefits in Figure E.2. Benefit entitlements before the reform include supplementary housing assistance (*Wohngeld*) in the total benefits that a jobseeker could potentially qualify for.[10] For the simulation, I consider a jobseeker in a single-person household with median previous earnings ($\approx$ 1750€ per month). I assume that she rents an apartment with housing costs at the local rent ceiling.[11] This implies an average housing expenditure share of approximately 36%, which is close to the observed mean in the data.

To calculate real benefits, I need to construct regional price indices. I follow the approach of Weinand and von Auer (2020), and construct the price index in region r at time t as a weighted average of regional price indices for housing, non-tradable services and tradable goods:

$$\ln P_{rt} = \omega_{rt}^{\text{housing}} \ln P_{rt}^{\text{housing}} + \omega_{rt}^{\text{services}} \ln P_{r}^{\text{services}} + \omega_{rt}^{\text{goods}} \ln P_{r}^{\text{goods}} \tag{14}$$

I obtain the housing expenditure share $\omega_{rt}^{\text{housing}}$ and price index from my assumption on rent expenditures. To calculate the weights ω_{rt} for services and goods, I assume that that 60% of the remaining income is spent on goods, and 40% on services. These shares are derived from the official post-reform formula for calculating long-term unemployment benefits. The subindices for goods and services are taken from from Weinand and von Auer (2020), based on price data from 2016. Finally, I normalize $\ln P_{rt}$ to one.

Figure 2 illustrates how real benefit levels (b_j / p_j) vary before and after the reform, depending on the residency location chosen by the jobseeker during unemployment.
Before the reform, jobseekers could face sizeable real income losses when relocating to a more expensive county. In my simulation, relocating from a location with average price ($\approx$ 680€ per month in real benefits) to one of the most expensive local labor markets like Munich, Frankfurt or Stuttgart could be associated with real income losses of up to 36%. After the reform, real benefits were nearly equalized across regions.[12] This reform to the benefit structure could have changed the incentives

[8] Since means-testing with respect to net household income was tightened, around 150.000 previous UA recipients lost eligibility, according to Bruckmeier and Schnitzlein (2007). They show that particularly couples without children with one partner being employed lost eligibility. Empirically, this largely affected women, and particularly women in East Germany.

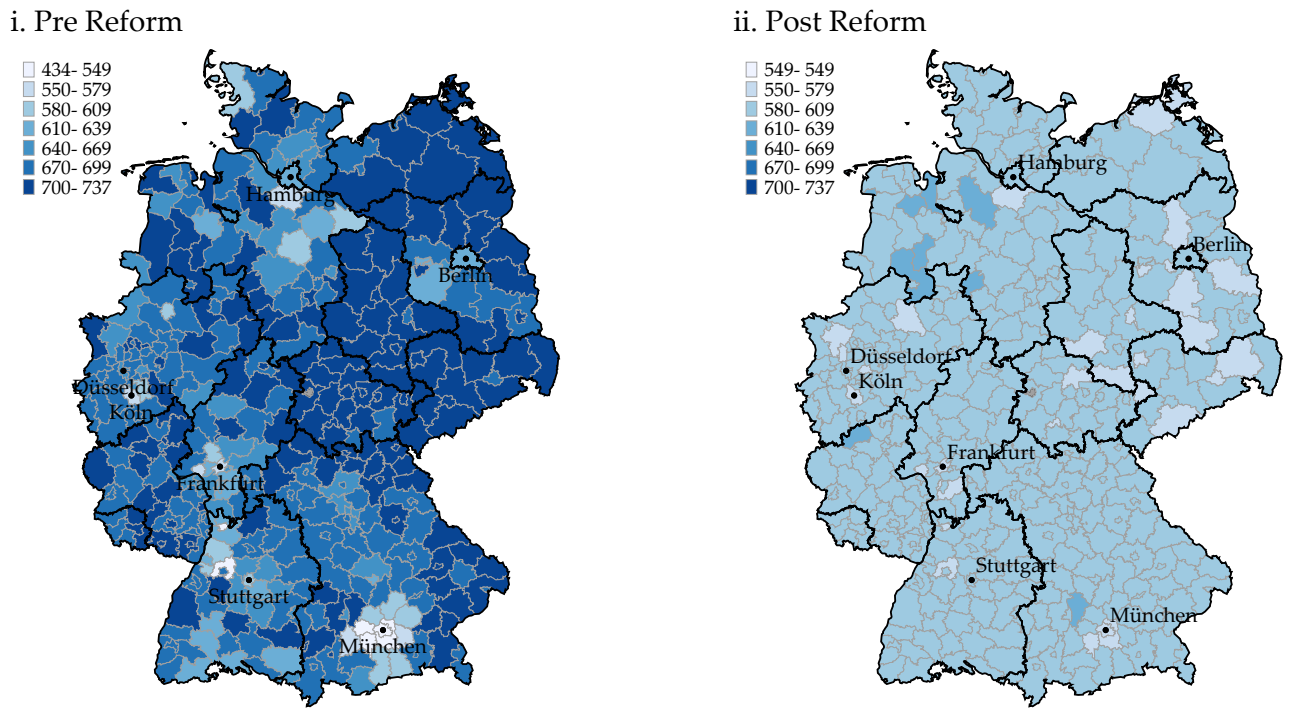
[9] While jobseekers could supplement their long-term unemployment benefits with housing assistance (*Wohngeld*), only 30% of long-term unemployed jobseekers claimed this additional support before the reform. Conditional on receiving, the housing assistance benefits averaged 100€ per month and were therefore usually not enough to cover the expenditure on housing.

[10] Details on how housing assistance was calculated before the reform can be found in Appendix E.2.

[11] For counties with variation in ceilings across its municipalities, I assume that the jobseeker locates in the municipality with the highest fraction of population.

[12] Although price differences in services can still lead to some variation in real benefit levels, the geographic dispersion is substantially reduced, as shown in Panel ii of Figure 2.

Figure 2: Simulated Real Long-Term Unemployment Benefits before and after the Reform



Notes: This figure shows simulated real benefits for long-term unemployed, single-person household with median previous earnings. Panel i. illustrates benefits in 2004, and Panel ii. in 2005. The regional price indices are based on Equation (14). Benefits include unemployment assistance (*Arbeitslosenhilfe*) and supplementary housing assistance (*Wohngeld*) before the reform, and long-term unemployment benefits (*Arbeitslosengeld 2*) after the reform. See text for further simulation details.

of jobseekers to migrate. Before testing this empirically, I now turn to the mobility restrictions that jobseekers faced when relocating during unemployment.

3.2 Mobility Restrictions for Unemployed Workers

Recipients of short-term unemployment benefits faced no restrictions on mobility, either before or after the Hartz IV reform. However, local Jobcenters required jobseekers to inform them of any change in residence at least one week in advance, in order to facilitate the transfer of the UI claim to the Jobcenter in the destination district.[13] Long-term unemployed workers before the reform were subject to the same set of rules when moving.

After the reform, recipients of long-term unemployment benefits were subject to stricter rules regarding mobility. Under the new benefit scheme, ALG 2 claimants were eligible for full reimbursement of their rent in the destination location (conditional on meeting the local ceiling) only if the responsible Jobcenter in that location had approved the move ex ante. Approval was typically granted when the new rent was deemed locally appropriate and when there were legitimate reasons for moving, such as an overcrowded dwelling or family reunification (§22, Paragraph 4, SGB II). In practice, ALG 2 recipients were therefore required to inform their Jobcenter of the intended destination dwelling and the reasons for moving before changing residence. If permission was granted, the local Jobcenter could cover relocation-related expenses such as moving costs or rent deposits (§22, Paragraph 6, SGB II). If the move was not approved, any rent or heating expenses exceeding the pre-

[13] If moves were not reported before six weeks after the move, the moving claimant could ex-post lose his benefits between the day of moving and his new registration at the destination Jobcenter.

vious level were not reimbursed by the Jobcenter. When analyzing the effects of regional benefit levels on location choices, I focus on migration that occurs during the unemployment insurance (UI) period—and, crucially, before individuals begin receiving long-term unemployment benefits. Migrants during the UI spell did not face any additional restrictions after the reform, whereas moving while on long-term unemployment benefits has become considerably more constrained after the reform.

ALG 2 claimants with less than 25 years of age faced even stricter mobiliy rules. In addition to the general restrictions applying to all ALG 2 claimants, young claimants were ineligible for *any* housing assistance until turning 25 if they moved to a new dwelling without prior approval from the Jobcenter. Moreover, this ineligibility could also apply if the move had taken place before entering ALG 2 (§22, Paragraph 5, SGB II). This additional restriction was not part of the original 2005 reform but was introduced as an amendment during the first half of 2006.[14] In my empirical analysis of location choices, I exploit this variation in mobility restrictions to identify how different components of the reform causally affected the migration behavior of unemployed workers.

4 Data

I combine administrative worker and jobseeker records provided by the Institute for Employment Research (IAB) with data on local rent ceilings after Hartz IV, which I digitized myself.

Data on local rent ceilings. One contribution of this paper is the collection and digitization of the first systematic database on local rent ceilings since Hartz IV. Specifically, I build a dataset of local rent ceilings at the municipality×household size×year-level for the time period 2005 to 2022. To do so, I compile roughly 4,000 internal guideline documents and information sheets for jobseekers from local Jobcenters and administrative units. I collect these documents from individual requests and from a private lawyer association.[15] These documents contain information on the currently applied rent ceiling by household size and residency location. Figure G.1 provides two examples of how these ceilings are reported. I digitize the ceiling reports using OpenAI's GPT-4o. More specifically, I first request from GPT-4o to detect all report pages with information on local rent ceilings, and specifically, whether the ceilings apply to rents with or without including bills (heating and utility costs). I then ask GPT-4o to assign the reported ceilings to the corresponding municipality×household size×year cell. I further record groups that are explicitly mentioned to be exempted from the ceiling, as well as whether Jobcenters do not lower the housing subsidy if the claimant's rent exceeds the local ceiling by only small amounts (*Kulanzgrenze*). I end up with ceiling information for 384 of the 401 counties.[16]

Administrative datasets. I use three datasets that cover jobseeker biographies. First, I use the Sample of Integrated Welfare Benefit Biographies (SIG), which covers a random 4% sample of long-term unemployment benefit recipients from 2007 onwards (Dummert et al., 2022). The dataset records date-stamped long-term unemployment spells, together with the entire labor market histories of all individuals in the claimant's household. It contains information on benefit amounts and available income at monthly frequency. Crucially, the SIG dataset includes information on housing costs, which I

[14] Jobcenters differed in how they applied these additional restrictions. While some withheld housing assistance only when a claimant moved out of the parental home, others applied the restriction to all relocations.

[15] I am grateful for the association *Tacheles e.V.* for sharing these documents with me.

[16] Time coverage varies across counties. For 46% of counties with information, the ceiling is observed in or before 2007, when the data on long-term unemployment spells begin. For 79% of counties, the ceiling is observed in or before 2009.

use to measure the difference between a claimant's actual rent and the local rent ceiling in the regression kink design. Because this variable records only the locally appropriate rent once the Jobcenter restricts housing assistance to the ceiling, I restrict the estimation sample to new long-term unemployment spells that were not preceded by ALG II receipt during the previous three months. This ensures that I accurately capture each claimant's true housing costs. I further restrict the sample by dropping single-parent, and disabled and pregnant claimants as these groups are usually exempted from the local rent caps. I also drop non-German citizens from the sample, as migrants can be subject to different caps, depending on their legal status, which is not recorded in the dataset. Finally, I restrict the sample to claimants that are registered as a jobseeker with the Jobcenter, a common restriction in the literature that studies the labor supply effects of unemployment benefits (e.g. Kolsrud et al., 2018). I end up with a sample of 147,256 long-term unemployment spells from the years 2007 to 2017.

Second, I use a 2% random sample of German employment biographies (SIAB, Frodermann et al., 2021), provided by the Institute for Employment Research (IAB). The dataset records the exact start and end dates of all employment and unemployment spells among workers in the sample. Moreover, it contains information on the type and amount of unemployment benefits as well as a comprehensive set of worker and workplace characteristics. Information on the residency location is available at the county-level (N = 401) from 1999 onwards and usually recorded at the beginning of a new spell. I use this dataset to study how location choices were affected by Hartz IV. I construct a sample of displaced workers, who entered UI between 1999 and 2013 ("cohorts"), thereby covering a wide window around the reform.[17] I characterize workers as (involuntarily) displaced if they claim UI within less than 90 days after job loss as voluntary quits would not be entitled for UI during this time period (following Price, 2019). I focus on workers who did not voluntarily quit their job to lower the risk of different selection into the sample as a consequence of the reform. I additionally restrict the sample to workers aged 16 to 64 at unemployment entry. Each displacement cohort consists of between 20,000 and 30,000 UI spells.

Finally, I complement my analysis using data from PASS-ADIAB dataset (Dummert et al., 2023), a representative survey of households, with household members matched to their administrative labor market biographies. It contains information on job search, housing and geographic mobility, together with rich socio-demographic and economic information on the household and all its members. The variables on job search and housing costs allow me to study how jobseekers adjust their search effort as a function of varying unemployment benefits.

Regional unit of analysis. Across all datasets, I define cities as commuting zones in the top tercile of the rent distribution. Commuting zones are aggregation of counties—the most detailed geographic unit available across all datasets—and follow the definition from Kosfeld and Werner (2012), based on commuting flows. I obtain data on rents from the Microcensus—a 1% random sample of households— for the years 2002, 2006 and 2010.[18] During my sample period, roughly 50% of the German population lived in cities, as defined above. Figure B.3 shows the geographic distribution of cities.

[17] Prior to 1999, residency locations were not consistently recorded, which prevents me from using these years for my mobility results. I include displacement cohorts until 2013 to maximize sample power, while potentially being able to observe workers up to six years after displacement, which will be my event window in the matched event study analysis.

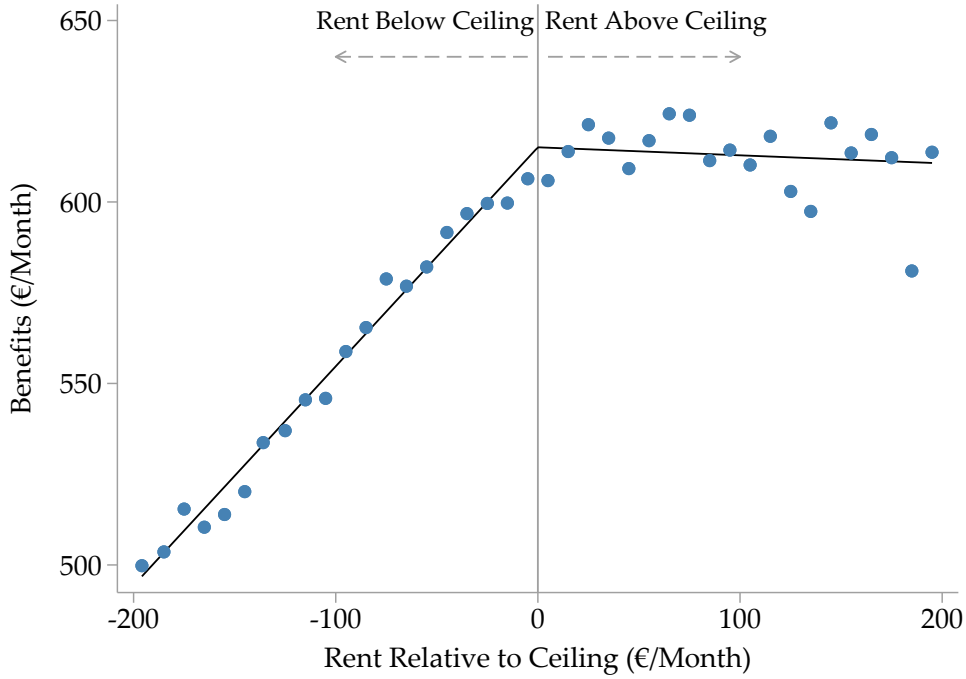
[18] Information on rents is only available in the Microcensus every four years, starting in 1998.

5 Duration Responses

In this section, I estimate how variation in benefit generosity affects local unemployment durations. Equation (11) shows that differences in the local duration responses to benefit changes between high- and low-price locations matter for the optimal allocation of unemployment benefits across space. I therefore estimate how unemployment benefits affect unemployment durations heterogeneously between low-price and high-price locations. For identification, I exploit the kink in the benefits schedule that arises from whether claimants have housing costs above or below the locally appropriate level in a regression kink design (RKD). As outlined in Section 3.1, Jobcenters are instructed to cap the housing subsidy at the locally appropriate level from the seventh month onwards (§22, paragraph 1, sentence 3, SGB II).

Figure 3 shows the benefit schedule for long-term unemployment claimants in my sample who have continuously received benefits for at least six months.

Figure 3: Long-Term Unemployment Benefits after Six Months around Local Rent Ceiling



Notes: This figure shows average monthly ALG 2 benefits for the sample of long-term unemployment claimants who claim benefits for at least seven months. Dots represent €10 bins.

The benefit schedule exhibits a clear kink at the cutoff: benefits rise with higher housing costs below the local ceiling but remain roughly flat above it, as Jobcenters are instructed to cover housing subsidies only up to the ceiling.[19]

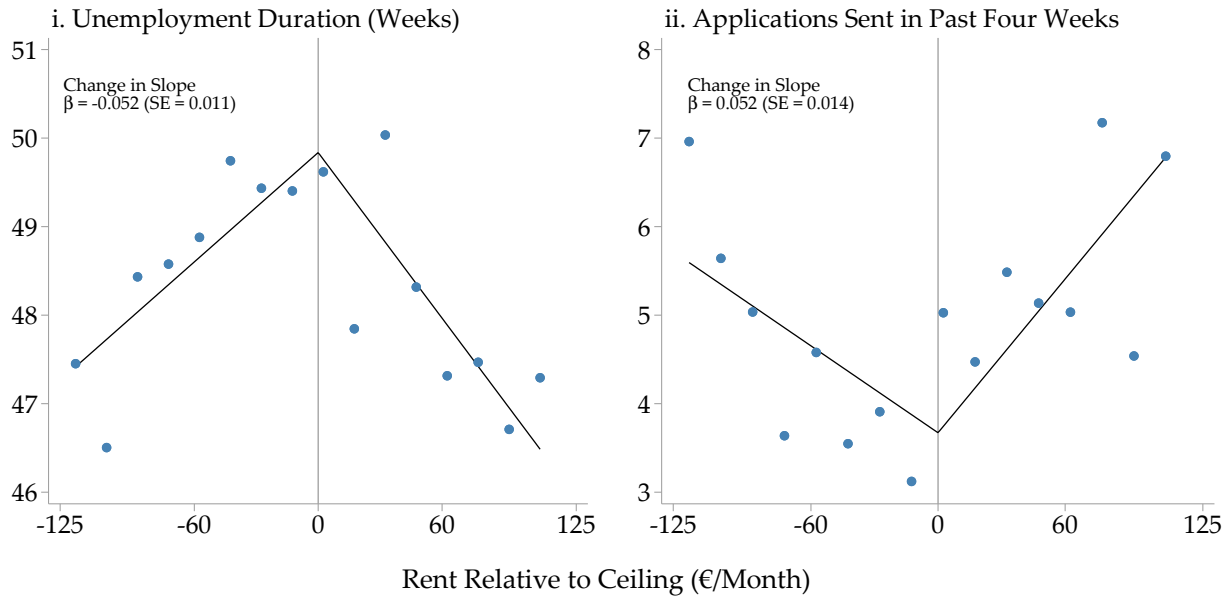
Figure 4 provides visual evidence of duration responses to the kinked benefit schedule. Throughout this part of the analysis, the primary outcome to capture duration responses is completed unemployment duration in line with previous work (e.g. Kolsrud et al., 2018; Landais, 2015). I assume that a single model period corresponds to three years and therefore right-censor completed duration at that horizon.[20] Panel i. shows a sharp kink in the unemployment duration around the kink. While

[19] It is worth noting that benefits do not increase one-for-one below the ceiling, as higher housing costs are typically associated with higher incomes, which are partially offset in the benefit calculation. Crucially, disposable income evolves smoothly around the cutoff.

[20] Table A.1 in the Appendix shows robustness to using different time horizons.

unemployment duration rises with higher rents below the ceiling, it declines once rents exceed the local threshold, since jobseekers are expected to cover the difference out of their lump-sum cash transfer. My model predicts that differences in job take-up are driven by adjustments in search effort. For a smaller sample of long-term unemployment spells, I observe direct measures of search effort from the matched survey. Panel ii. shows that effort—measured by the number of applications sent in the past four weeks—also exhibits a kink at the ceiling. Consistent with the model-implied mechanism behind the inverse V-shape in unemployment duration, jobseekers submit fewer applications as rents (and thus benefits) rise below the ceiling, while the opposite holds once rents exceed the threshold.

Figure 4: Duration and Effort Responses around Local Rent Ceiling



Notes: This figure shows duration and effort responses among long-term unemployment benefit claimants. Panel i. plots the average completed unemployment duration in weeks, right-censored at three years. Panel ii. shows the average number of applications (winsorized at the 99th percentile) sent during the previous four weeks. The running variable is the difference between the jobseekers' own rent and the local rent ceiling. The data is sliced into €15 bins, following the recommendation by Calonico et al. (2014).

Through the lens of the model, the term that captures differences in duration responses between high- and low-price locations is:

$$\underbrace{\frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}}_{\text{Duration Responses}}$$

I now outline how I leverage the kinked benefit schedule to estimate the local duration elasticities in urban and rural areas.

5.1 Identification

I use a fuzzy regression kink design with heterogeneous effects between cities and rural regions as in Naik (2025). The kink is fuzzy rather than sharp for two reasons.

First, compliance with the ceiling is imperfect, as Jobcenters may grant exemptions that permit housing subsidies to exceed the locally defined limit even after the seventh month of the claim. Internal guidelines used to process exemptions predominantly mention two reasons. The first is that claimant groups with higher housing needs—such as pregnant women or disabled individuals—are

usually entitled to housing assistance beyond the ceiling.[21] The second reason for exemption relates to relocation costs. Exemptions can be granted if moving expenses—borne by the Jobcenter—would exceed the savings from lowering the subsidy (*Wirtschaftlichkeitsprüfung*), or if the claimant can demonstrate that no housing unit with costs below the ceiling is available. For those jobseekers, who exit long-term unemployment before the seventh month ($\approx$ 40% of the sample), I have to predict their probability of being exempted, which in turn determines their benefit levels from the seventh month onward (see Appendix G.2). In my baseline specification, I assume that jobseekers for whom I impute the benefit schedule are aware of their probability of receiving an exemption. In a robustness analysis, I relax this assumption and show that the results are not sensitive to alternative assumptions about how jobseekers form expectations regarding the benefit schedule.

The second reason for using a fuzzy kink design is measurement error in the running variable. The exact ceiling on the housing subsidy—which includes rent (with utilities) and heating costs—can vary even among claimants in the same county$\times$household size$\times$year cell. On the one hand, some counties apply different ceilings across municipalities. On the other hand, a subset of Jobcenters adjusts allowable heating costs based on heating type and total building size. None of these details are observed in my administrative data. However, both sources of within-region variation are economically small in my sample.[22] For the baseline results, I assign jobseekers in locations with within-cell variation to the modal ceiling in their area (see Appendix G.1 for details). I also conduct robustness checks by excluding jobseekers from locations with municipal variation and by varying assumptions about heating type ceilings.

Estimation. I estimate a fuzzy RKD specification using local linear regression, which allows for heterogeneous responses of unemployment duration to the kinked benefit schedule across rural and urban locations. More specifically, for long-term unemployment spell s with household size h in location j and year t, I estimate the following second-stage equation:

$$y_s = \beta_0 + \beta_1 b_s + \beta_2 (b_s \cdot \mathbf{1}\{j(s) \in \text{City}\}) + \delta_1 (r_s - c_{jth(s)}) + \delta_2 [(r_s - c_{jth(s)}) \cdot \mathbf{1}\{j(s) \in \text{City}\}] + \eta_{jth(s)} + \epsilon_s \tag{15}$$

y_s is unemployment duration, censored at three years, while b_s refers to the benefits applicable from month seven of a long-term unemployment spell. $\eta_{jth(s)}$ denotes ceiling fixed effects, which are included to restrict comparisons to jobseekers within the same region–size–year cell. Standard errors are clustered at the ceiling level. I instrument the benefits variables exploiting the kink in the benefit

[21] As outlined in Section 4, I restrict the estimation sample to groups not explicitly mentioned in these guidelines. Nevertheless, among the remaining sample, about 20% of claimants who continuously receive benefits for at least six months obtain housing subsidies above the ceiling for reasons unobserved in the administrative data. This share is similar in cities (20.5%) and rural areas (19.7%).

[22] First, 81% of claimants live in counties without municipal variation in the ceiling, and for the remaining 19%, the across-municipality standard deviation is only 9 € per month. Similarly, during the sample period, variation in ceilings across heating configurations is modest—averaging 12 € per month—when focusing on the three most common heating types among long-term unemployment claimants, which together account for roughly 90% according to the German Socio-Economic Panel (SOEP, see Figure G.2).

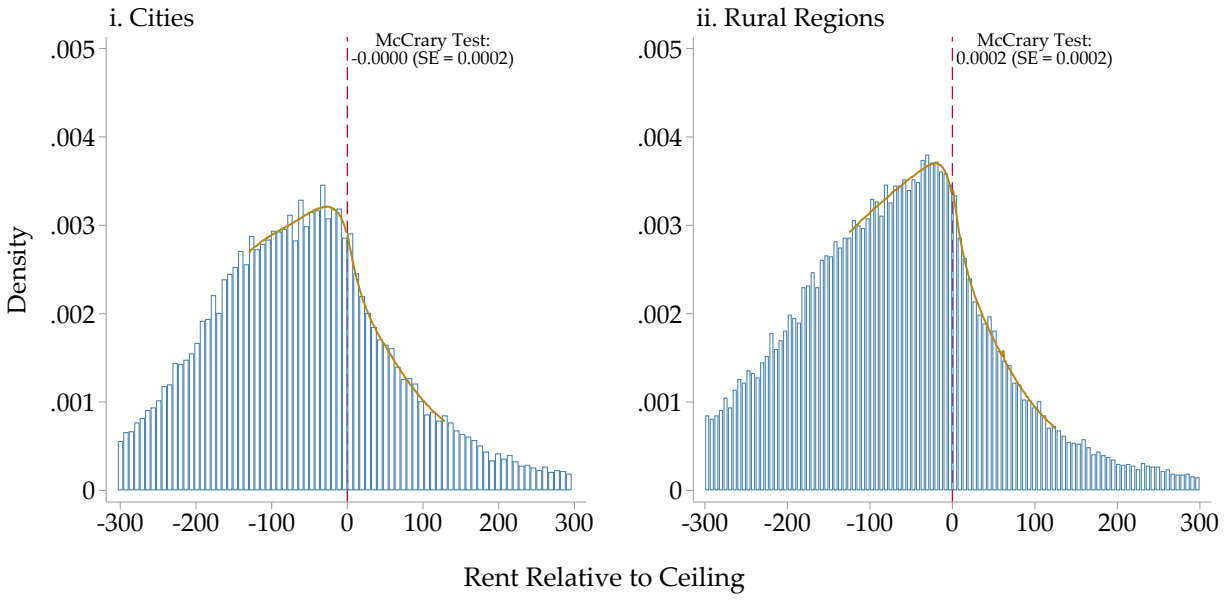
structure using the following first stage estimating equations:

$$\begin{aligned} b_s &= \alpha_0 + \alpha_1(r_s - c_{jth(s)}) + \alpha_2(r_s - c_{jth(s)}) \cdot \mathbf{1}\{j(s) \in \text{City}\} \\ &\quad + \gamma_1(r_s - c_{jth(s)}) \cdot \mathbf{1}\{r_s - c_{jth(s)} \geq 0\} \\ &\quad + \gamma_2(r_s - c_{jth(s)}) \cdot \mathbf{1}\{r_s - c_{jth(s)} \geq 0\} \cdot \mathbf{1}\{j(s) \in \text{City}\} + \eta_{jth(s)} + \nu_s \\ b_s \cdot \mathbf{1}\{j(s) \in \text{City}\} &= \phi_0 + \phi_1(r_s - c_{jth(s)}) + \phi_2(r_s - c_{jth(s)}) \cdot \mathbf{1}\{j(s) \in \text{City}\} \\ &\quad + \psi_1(r_s - c_{jth(s)}) \cdot \mathbf{1}\{r_s - c_{jth(s)} \geq 0\} \\ &\quad + \psi_2(r_s - c_{jth(s)}) \cdot \mathbf{1}\{r_s - c_{jth(s)} \geq 0\} \cdot \mathbf{1}\{j(s) \in \text{City}\} + \eta_{jth(s)} + \varepsilon_s \end{aligned}$$

I estimate these specifications using a Calonico et al. (2014) robust bandwidth of $h = |r_s - c_{jth(s)}| = 125$€ and a uniform kernel as in Card et al. (2015).[23]

Support for identifying assumptions. One key identifying assumption is that the rent distribution evolves smoothly around the cutoff. A potential concern is that unemployed workers expecting long unemployment spells might systematically sort into housing just below the threshold. Figure 5 presents a histogram and the estimated density, separately for jobseekers in urban and rural areas.

Figure 5: Density of Rents around the Ceiling by Cities and Rural Regions



Notes: This figure shows the density of rents around the local rent ceiling. Panel i. shows the density for cities and Panel ii. for rural regions. The yellow line shows the density, which is estimated separately for below and above the cutoff in a bandwidth of $h =$ €125. The bin widths of the histogram and the modern McCrary tests follow Calonico et al. (2014).

The figures show no visual evidence of bunching just below the ceiling—an impression confirmed by Modern McCrary tests (Calonico et al., 2014) using a fifth-order polynomial ($p < 0.81$ and $p < 0.49$ in cities and rural regions, respectively). Moreover, the derivative of the estimated density evolves smoothly around the threshold. Two factors could explain the absence of bunching. First, I observe rents at the onset of long-term unemployment, typically following employment or unemployment insurance spells, meaning jobseekers would have had to sort into housing prior to their long-term

[23] Due to potential measurement error in my running variable and heterogeneous treatment effects, I follow Naik (2025) and do not use regularization to select the bandwidth. Appendix A.2 shows my main results as a function of the selected bandwidth.

Table 1: Heterogeneous Effects of Benefit Changes on Unemployment Duration

	Reduced Form			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}$	-0.053***	-0.054***	-0.048***			
	(0.014)	(0.014)	(0.016)			
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}\cdot\mathbf{1}\{\text{City}\}$	0.004	0.010	0.011			
	(0.021)	(0.020)	(0.026)			
b				0.064***	0.070***	0.067***
				(0.017)	(0.018)	(0.024)
$b\cdot\mathbf{1}\{\text{City}\}$				0.018	-0.001	-0.002
				(0.033)	(0.031)	(0.043)
Elasticities						
ϵ_{City}				0.78	0.64	0.60
				(0.27)	(0.24)	(0.36)
ϵ_{Rural}				0.54	0.58	0.55
				(0.15)	(0.15)	(0.20)
Ceiling FE	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
Mean Duration	48.5	48.5	48.5	48.5	48.5	48.5
Kernel	uniform	uniform	triangular	uniform	uniform	triangular
Observations	90,259	89,661	89,661	90,259	89,661	89,661

Notes: This table shows heterogeneous effects of benefit changes on unemployment duration in weeks. Columns (1) to (3) show the reduced form and columns (4) to (6) the IV estimates. Unemployment duration is measured in weeks and right-censored at three years. Benefits correspond to monthly benefits that jobseekers receive from the seventh month onward of their long-term unemployment spell. The elasticities are obtained by multiplying the IV-estimate with the ratio of average benefits and unemployment duration, separately for the city and rural location. Standard errors are clustered at the ceiling level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

benefit claim. Second, Jobcenters and local governments are encouraged to update ceilings regularly—usually every two years—which makes it difficult to sort into contracts just below the ceiling. Finally, Figure B.4 shows no evidence of differential selection on observables around the kink; predicted unemployment duration based on a rich set of pre-determined covariates evolves smoothly through the cutoff.

5.2 Results

Main results. Table 1 shows the main reduced form and IV results when successively adding pre-determined covariates (columns (2) and (5)) or using a triangular kernel (columns (3) and (6)). The reduced form results confirm the negative change in slope to the right of the ceiling from Figure 4, but show no sign of heterogeneous responses in duration to the kinked benefit schedule. Columns (4) to (6) show the IV estimates and the corresponding elasticities. The latter are obtained from multiplying the IV-estimates with the ratio of average benefits to average unemployment durations in the estimation samples in urban and rural locations. Across all specifications, jobseekers significantly increase their duration with higher benefits. The estimate for jobseekers in rural locations implies that jobseekers on average increase their unemployment duration by ≈ 0.07 weeks for every 1€-increase of benefits. The corresponding elasticities vary between 0.54 and 0.78 across the specifications and

locations, which is just below the estimates of Kolsrud et al. (2018) when using a similar kink in unemployment benefits in Sweden that applies at later stages of the spell. While the effect for jobseekers in cities is more imprecisely estimated, the point estimate in the most saturated models (columns (5) and (6)) is very close to zero. In the preferred specification from column (5), the elasticities are estimated to be very close to each other with a point estimate of 0.64 and 0.58 in cities and rural areas, respectively. Importantly, I estimate the ratio of elasticities—the key object from Equation (11):

$$\frac{1+\hat{e}_{\text{City}}}{1+\hat{e}_{\text{Rural}}} = 1.040 \quad (\text{SE} = 0.18) \tag{16}$$

Altogether, these estimates indicate that labor supply responses to changes in benefit generosity do not differ significantly between urban and rural locations.

Robustness. I conduct a series of robustness checks to assess the sensitivity of the result that unemployment duration responses do not significantly differ across rural and urban locations. Table A.1 reports heterogeneous effects of benefit changes when censoring unemployment duration at different thresholds. Across all specifications, the estimated duration elasticities are very similar between city and rural areas, with differences ranging from 0.04 to 0.08. Table A.2 shows the main results across a range of bandwidths. The estimates are robust for both wider and narrower windows around the ceiling, although they lose statistical significance for bandwidths below or equal to 75€.

5.3 Mechanism

Through the lens of the model, local responses in unemployment durations are shaped by two forces:

$$\frac{d(1-s_j)}{db_j} = -\frac{de_j}{db_j} \cdot z_j = \underbrace{z_j \frac{\frac{U'(b_j/p_j)}{p_j}}{\psi''(e_j)}}_{=-de_j/db_j} \cdot z_j$$

On the one hand, variation in benefit generosity may induce responses in search effort, as shown in Figure 4. This effort response depends on local factors, namely search efficiency, the marginal value of real benefits, and the optimal effort choice before the benefit change.[24] On the other hand, even identical effort responses can translate into different duration effects if the efficiency of effort varies across space. *A priori*, it is unclear how these forces vary with local prices. In the United States, job-finding rates tend to be higher in large—and typically more expensive—commuting zones (Moretti and Yi, 2024), whereas the evidence for Germany is more mixed (Haller and Heuermann, 2020). Moreover, previous work has generally not isolated search efficiency by holding effort constant when examining the spatial distribution of job-finding rates. I draw on a sample of long-term unemployed workers for whom I observe search effort in survey data, which makes it possible to investigate how both—effort responses and search efficiency— correlate with local prices.

I start by testing whether adjustments in effort vary systematically between urban and rural locations. I again exploit the kink in long-term unemployment benefits around the local rent ceilings using the same reduced form equation as in Section 5.2.[25] Instead of unemployment duration, I now

[24] Since $\psi''(\cdot) > 0$, higher initial effort leads—*ceteris paribus*—to a smaller adjustment in effort. Intuitively, because effort costs are convex, small reductions in effort are sufficient to restore the first-order condition after a change in benefits when starting from a high initial level of effort.

[25] In this exercise, I only estimate the reduced-form equation, as I only observe the self-reported benefit level in the survey data. However, I do not find evidence of heterogeneous first-stage effects in the main regression kink results reported in

Table 2: Heterogeneous Effects on Search Effort

	$\mathbf{1}\{$ Searched $\}$ (1)	Applications (2)	Unsolicited Applications (3)	Hours Searched (4)
Reduced Form				
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}$	0.002*	0.058***	0.021**	0.035**
	(0.001)	(0.019)	(0.009)	(0.016)
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}\cdot\mathbf{1}\{\text{City}\}$	0.001	-0.040	0.023	-0.001
	(0.002)	(0.026)	(0.033)	(0.038)
Ceiling FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Mean Effort	0.5	4.1	1.3	3.4
Observations	6,064	6,064	6,064	849

Notes: This table shows heterogeneous effects between city and rural on various measures of search effort. Columns (1) to (3) refer to search effort measures four weeks preceding the survey interview. Column (4) refers to average weekly hours searched during the four weeks, which is only available for the survey waves 2015 and 2016. Unsolicited job applications refer to applications sent to employers without a posted vacancy. Standard errors are clustered at the ceiling level. Asterisks indicate *** p $<$ 0.01, ** p $<$ 0.05, * p $<$ 0.1.

examine the effects of the kinked benefit schedule on several measures of search effort: whether individuals searched during the past four weeks, the number of applications and unsolicited applications submitted, and the number of hours spent searching during the past week—the latter being available only for two survey waves. Table 2 shows no evidence of heterogeneous responses in search effort. Although the sample around the cutoff with matched survey responses is considerably smaller than the full sample ($N \approx 6,000$), all effort measures exhibit a positive and significant change in slope around the rent ceiling. This pattern is consistent with the graphical evidence in Panel ii. of Figure 4, which shows that search effort increases once jobseekers exceed the local ceiling—relative to observations to the left of the cutoff. However, effort responses do not vary systematically between cities and rural areas: none of the interaction effects are significant, and the point estimates show no systematic sign towards stronger or weaker responses in search effort to the kinked benefits between high-rent and low-rent locations.

Next, I examine whether search efficiency z_j varies systematically with local prices. I construct measures of local search efficiency using the survey-linked sample. Figure 6 plots a binned scatter plot of search efficiency and commuting-zone rent levels. Panel i. depicts raw search efficiency—the local job-finding rate divided by average search effort—while Panel ii. shows residualized search efficiency after controlling for the composition of jobseekers across commuting zones.[26] Neither measure of search efficiency is correlated with local rents, mirroring the limited geographic variation in job-finding rates among the long-term unemployed in Germany.

Taken together, these results suggest that while the long-term unemployed reduce their search effort in response to more generous unemployment benefits, the associated effects on unemployment

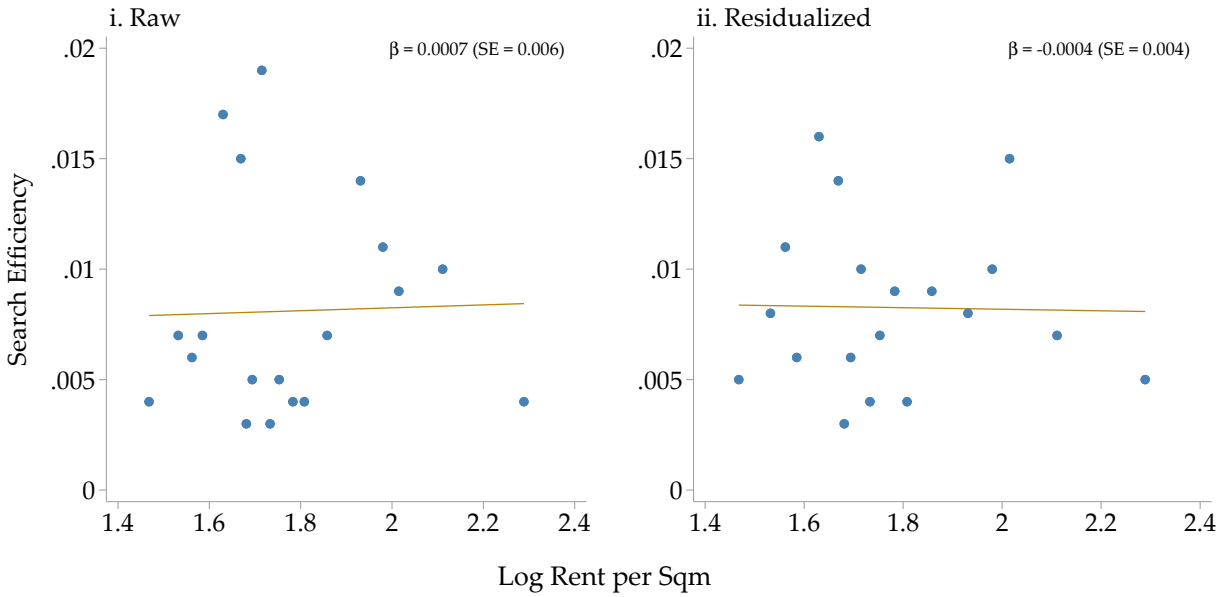
Table 1, which suggests that the reduced-form estimates are informative about the local effects of benefit changes.

[26] Specifically, I run the following regression for individual i in month t and commuting zone j:

$$\mathbf{1}\{\text{Finds Job}\}_{it} = \beta_0 + \beta_1 \#\text{Applications}_{it-1} + X_{it}'\gamma + \eta_{j(it)} + \epsilon_{it} \tag{17}$$

I estimate this equation using a linear probability model, and a Poisson regression. To obtain residualized search efficiency, I take the commuting-zone fixed effects and normalize the estimate to have the same mean and standard deviation as raw search efficiency.

Figure 6: Correlation between Search Efficiency and Rents



Notes: This figure shows a binned scatter plot between local rents and search efficiency, both measured at the commuting zone. Panel i. plots raw search efficiency z_j, the ratio between the job-finding rate and average search effort. Panel ii. shows residualized search efficiency, which comes from the individual-level regression in Equation (17) and accounts for the local composition of jobseekers. Local rents correspond to average rents per square meter during the sample period (2007-2021), obtained from the Microcensus.

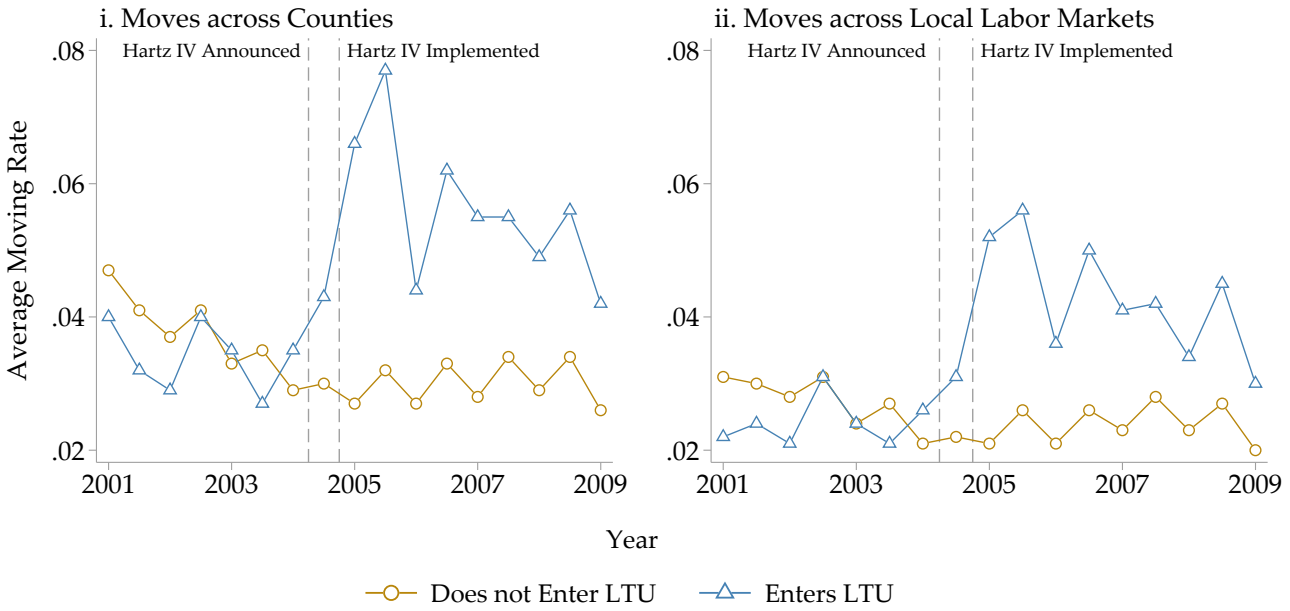
duration are similar across city and rural locations. The ratio of duration elasticities—the parameter capturing duration responses in the optimality formula—between urban and rural areas is close to one (see Equation (16)), indicating that redistributing benefits across the two locations does not generate additional fiscal costs through differential duration responses. These uniform duration responses reflect similar adjustments in search effort and the absence of a systematic correlation between local search efficiency and prices.

6 Migration Response

In this section, I estimate the migration elasticity with respect to benefits in high-price locations. According to Equation (11), this elasticity—together with the fiscal effect of movers (see Section 7)—determines the fiscal costs of providing more generous benefits in high-price areas due to migration. I examine how the introduction of rent-indexed benefits under the Hartz IV reform affected the location choices of the unemployed. The analysis exploits quasi-experimental variation in exposure to the reform, comparing groups that were more strongly affected by the new benefit rules with those that were weakly exposed in a dynamic difference-in-differences framework. In this analysis, I focus on mobility after displacement and before (potentially) entering long-term unemployment. I do so for two reasons. First, this timing of moves aligns with the model, in which jobseekers choose their location after displacement, before knowing with certainty whether they will find a job in their destination of choice. Second, under the post-reform ALG 2 system, policymakers introduced additional barriers to migrating when simultaneously claiming ALG2 benefits (see Section 3.2), which likely attenuates the migration effects when studying the long-term unemployed directly.

Figure 7 shows average moving rates during unemployment insurance (UI) spells, comparing jobseekers who subsequently claim long-term unemployment benefits with those who exit UI before

Figure 7: Average Migration Rates among Jobseekers by Subsequent Unemployment Status



Notes: This figure shows average moving rates of displaced workers during unemployment insurance, depending on whether they enter long-term unemployment after UI (blue) or find a job at least three months before exhausting benefits (orange). Spells are assigned to semesters based on the semester in which their UI spells ends, which is the moment when residency changes are recorded in the data.

exhaustion. Panel i. displays moving rates across counties, while Panel ii. shows moves across commuting zones. Across both outcomes, jobseekers who later claim long-term unemployment significantly increase their migration rates once Hartz IV is announced and implemented. After the reform, their migration rates remain 3-4 percentage points higher than those of jobseekers who exit UI before entering long-term unemployment and who show no comparable increase in mobility.

6.1 Identification

One identification concern with the raw time series shown in Figure 7 is that the reform may have altered selection into long-term unemployment, potentially driving the observed changes in migration rates. Therefore, instead of assigning treatment status based on an endogenous outcome—entering long-term unemployment—I define the treatment group based on *predicted* long-term unemployment risk, which uses pre-determined characteristics at displacement and *pre-reform* data. Specifically, I use a training sample of unemployment spells that started before 2004 to predict which spells are more likely to enter long-term unemployment, based on characteristics at unemployment entry. Following Mueller and Spinnewijn (2023), I use a rich set of socio-demographic worker characteristics, information on the previous job and data on the worker's employment biography preceding unemployment. In Appendix F, I enumerate the full set of variables and show the predictive performance of my most saturated model. Reassuringly, when using the same outcomes as in Mueller and Spinnewijn (2023), my prediction algorithm achieves similar predictive power in the test sample prior to the reform, and only slightly weaker performance in the post-reform sample. Since I discretize the predicted risk variable by yearly quantiles, I am primarily concerned with the ranking of risk rather than its absolute level. Panel ii. of Figure F.1 in the Appendix shows that measurement error does not appear to be systematically correlated with the risk ranking: the actual probability increases approximately linearly with the (mis-measured) predicted probability of entering long-term unemployment.

I then assign a spell s from displacement cohort c (year) its treatment status by discretizing the continuous predicted long-term unemployment risk measure into a binary variable as follows:

$$\text{Treated}_s^{\text{Risk}} = \begin{cases} 1 & \text{if spell } s \text{ is in the top P percent of LTUE risk} \\ & \text{among displacement spells from cohort } c \\ 0 & \text{if spell } s \text{ is in the bottom P percent of LTUE risk} \\ & \text{among displacement spells from cohort } c \end{cases} \tag{18}$$

In my baseline results, I set P to 16.6, which corresponds to the fraction of spells that enter long-term unemployment during the pre-reform period. I later show robustness to different cutoffs. Similar to recent work on the effects of minimum wages (Cengiz et al., 2022) and the elimination of gender preferences in job vacancies (Card et al., 2021), I then use this predicted exposure measure in a dynamic difference-in-differences design.

Formally, my baseline dynamic difference-in-differences (DiD) estimating equation for spell s is:

$$y_s = \sum_{k \neq 2004} \delta_k \cdot (\text{Treated}_s^{\text{Risk}} \times \mathbf{1}\{t(s) = k\}) + \beta_1 \text{Treated}_s^{\text{Risk}} + \tau_{t(s)} + \epsilon_s \tag{19}$$

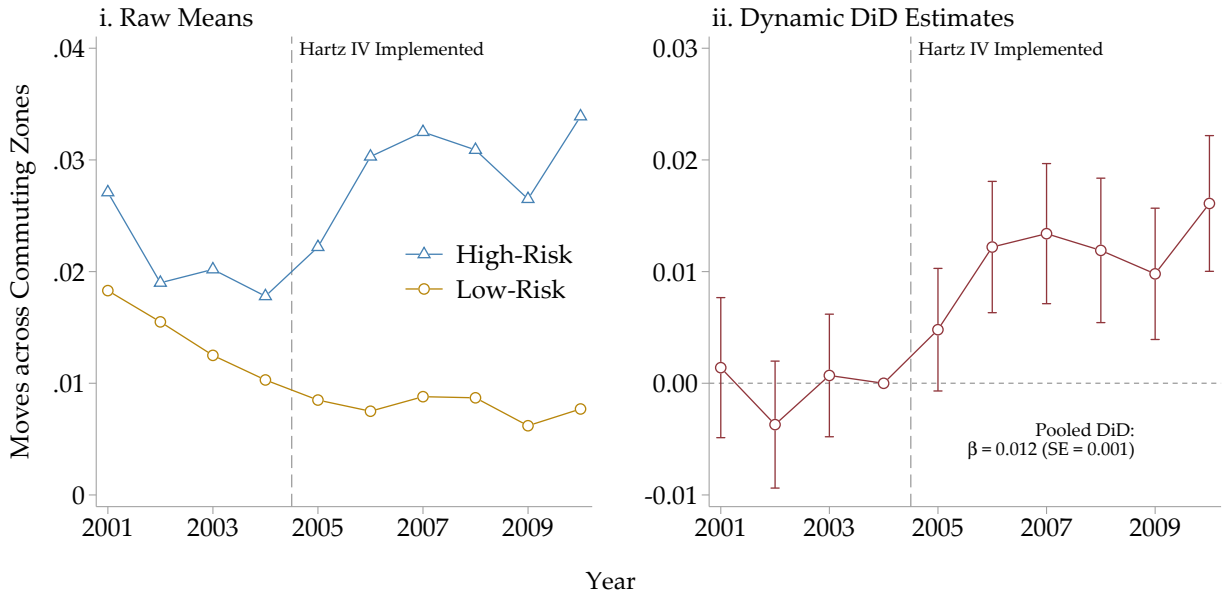
where y_s is an indicator variable for moving. t refers to the year in which the unemployment insurance spell concludes and any change in residency is recorded. I define a year ranging from July of the previous year to June of the contemporaneous year following Dauth and Eppelsheimer (2020). I normalize coefficients relative to 2004 (July 2003 to June 2004), the last year before reform announcement and implementation (see Figure B.2). I also show the main results using semesters instead of years. I cluster standard errors at the spell level and show robustness by constructing confidence intervals using a bootstrap procedure.

6.2 Results

DiD results for general moves. Figure 8 shows the estimated dynamic DiD coefficients (δ_k) from Equation (19) for moves across commuting zones. Panel i. shows that jobseekers with low- and high risk of entering long-term unemployment follow similar trends in migration before the reform. After the reform, only high-risk workers start migrating more. Panel ii. shows that this increase in migration is statistically significant and economically sizable. The pooled difference-in-difference estimate, where I consider high-risk spells from July 2004 onward as treated, suggests an increase in mobility of 1.2 percentage points—a 57% increase relative to pre-reform migration rates. Figure B.5 in the Appendix shows that the timing of effects aligns with the reform: High-risk jobseekers start migrating more in the semester of reform announcement (July to December 2004), and the effect stabilizes at around 1.25 percentage points from the second half of 2005 onward.

Robustness of DiD results for general moves. To test robustness to the choice of threshold percentile (P) for defining high risk, I estimate the static difference-in-differences version of Equation (19) separately by risk decile, keeping the control group fixed as in the main sample (bottom sixtile). As shown in Figure B.6, I only find significant increases in mobility after the reform among the two highest risk deciles, with the strongest effects observed for the top decile. In Table A.3 in the Appendix, I estimate the static DiD model, but restricted to jobseekers who experience unemployment both before and after the reform, with both spells classified as either low or top risk. This approach

Figure 8: Means and Dynamic DiD Estimates for Moves across Commuting Zones



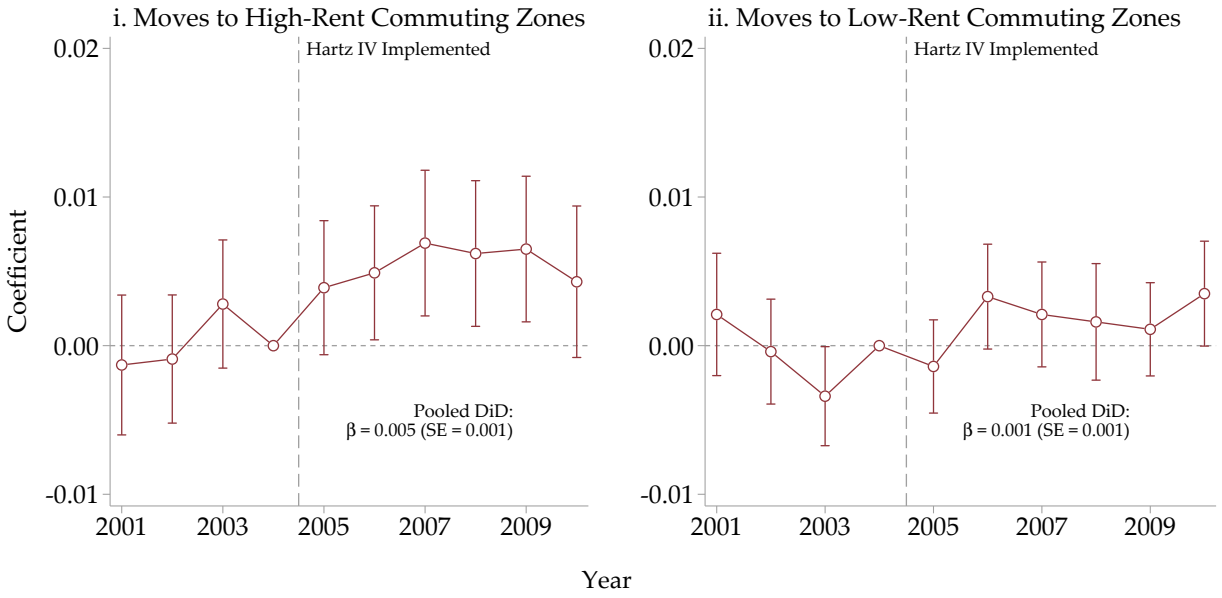
Notes: This figure shows means and dynamic difference in difference (DiD) estimates for moves across commuting zones. Panel i. shows raw means, and Panel ii. shows the estimated δ_k coefficients from Equation (19). The treatment group in year t consists of unemployment spells, which are in the top sixtile ($\approx$ 16.6%) of the long-term unemployment risk distribution and terminate UI recipience in year t. The control group consists of unemployment spells, which are at in the bottom sixtile ($\approx$ 16.6%) of the long-term unemployment risk distribution and terminate UI recipience in year t. Capped spikes denote 95 percent confidence intervals.

exploits within-worker variation in migration, thereby mitigating concerns about unobserved factors that may have changed differentially across high- and low-risk groups after the reform. I again only find significant increases in migration rates among jobseekers with high risk and therefore exposure to the reform.

Direction of moves. I next examine where treated jobseekers move and whether the reform primarily increased migration toward high-price urban areas. Specifically, I reestimate Equation (19) using as an outcome an indicator moves from low-rent (bottom tercile of rents) to high-rent (top tercile of rents). I contrast these moves toward high-rent locations with moves in the opposite direction. Figure 9 shows the estimated dynamic DiD coefficients. Moves toward high-rent destinations rise significantly after the reform, whereas moves toward low-rent destinations show no systematic change after the reform for jobseekers with high long-term unemployment risk. This pattern is even more pronounced when I classify destinations by population density (Appendix Figure B.7): mobility increases only toward high-density commuting zones. Both results are confirmed when comparing average destination–origin differences in characteristics for high-risk and low-risk jobseekers who moved after the reform (Appendix Figure B.8). Among high-risk displaced workers, moves are directed toward more populous, dense, and expensive locations relative to their origins. However, differences in job-finding and unemployment rates are small, indicating that these destinations are not associated with better labor market conditions. In contrast, moves among control-group jobseekers are less clearly directed toward urban destinations.

The results in this section have shown that jobseekers who were exposed to the reform start moving more, and that moves are disproportionately directed towards more expensive and denser locations. I now turn to the key migration parameter of interest according to optimal regional benefits in Equation (11)—the migration elasticity of the city with respect to unemployment benefits ϵ_{N_c,b_c}

Figure 9: Dynamic DiD Estimates of Directed Moves by Commuting-Zone Rent Levels



Notes: This figure shows the dynamic difference-in-difference estimates from Equation (19). Panel i. shows moves from commuting zones in the bottom tercile of rents toward commuting zones in the top tercile of rents. Panel ii. displays estimates for moves from commuting zones in the top tercile of rents toward commuting zones in the bottom tercile of rents. Jobseekers in the treated group belong to the top 16.6% of predicted long-term unemployment risk, whereas jobseekers in the control group belong to the bottom 16.6%. Capped spikes denote 95 percent confidence intervals.

6.3 Estimating the Migration Elasticity of Cities

If the taste shocks $\phi_j(i)$ in equation (21) follow a Gumbel distribution with inverse shape parameter σ, and jobseekers have CRRA preferences, the migration elasticity ϵ_{N_c,b_c} can be expressed as:[27]

$$\epsilon_{N_c,b_c} = \sigma \cdot (1 - s_c) \cdot \left(\frac{b_j}{p_j}\right)^{1-\rho} \cdot (1 - N_c) \tag{20}$$

N_c denotes the share living in the city. Given a value for the coefficient of relative risk aversion ρ obtained from the literature, the only remaining unknown parameter is σ, which I estimate in this section to recover the migration elasticity.

Deriving the estimating equation. To estimate σ, I use variation in expected local benefit levels across risk groups—arising from the Hartz IV reform—as before. To measure benefit levels at the local level and to increase sample size, I conduct the estimation at the commuting-zone level and subsequently aggregate the resulting migration elasticities to the city level, where the city is defined as the top tercile of commuting zones in terms of rent—following the definition in this paper. The location choice Equation (3) provides the basis for deriving the estimating equation for the migration elasticity. Incorporating risk group and year indices, the probability that jobseekers from risk group g in year t choose commuting zone j can be expressed as:

[27] This assumption on the taste shocks is very common in the literature, see for instance Gaubert et al. (2024). For a derivation of Equation (20), see Appendix H.1.

$$\pi_{jgt} = \frac{\exp\left[\sigma \times \left(s_{jgt} \cdot U\left(\frac{w_{jgt}}{p_{jt}}\right) + (1 - s_{jgt}) \cdot U\left(\frac{b_{jgt}}{p_{jt}}\right) - \psi(e_{jgt})\right)\right]}{\sum_k \exp\left[\sigma \times \left(s_{kgt} \cdot U\left(\frac{w_{kgt}}{p_{kt}}\right) + (1 - s_{kgt}) \cdot U\left(\frac{b_{kgt}}{p_{kt}}\right) - \psi(e_{kgt})\right)\right]} \tag{21}$$

Scaling this equation by the number of jobseekers from group g in year t and taking logs yields a prediction on the (log) number of jobseekers from risk group g in commuting zone j and year t:

$$\log(N_{jgt}) = \sigma \cdot s_{jgt} U\left(\frac{w_{jgt}}{p_{jt}}\right) + \sigma \cdot (1 - s_{jgt}) U\left(\frac{b_{jgt}}{p_{jt}}\right) + \eta_{gt} - \sigma \cdot \psi(e_{jgt}) \tag{22}$$

The group-year fixed effect η_{gt} absorbs the (log) number of jobseekers of a group in a year and the inclusive value from Equation (21). Estimating this equation requires assumptions about the coefficient of relative risk aversion ρ and effort cost function $\psi(\cdot)$, which I specify in the next paragraph. Crucially, omitting effort costs when estimating Equation (22) could lead to an overestimation of σ, even when using an instrument for the expected utility of unemployment. In particular, any positive local shock to benefits reduces effort costs, which might additionally attract jobseekers from other locations. Consequently, when estimating Equation (22), I include a proxy for effort costs, constructed from the model-implied optimal effort level given observed wages and benefits.

Parametrizing the utility and effort cost functions. In line with Corollary 1, I assume that jobseekers have CRRA preferences, and I assume a coefficient of relative risk aversion equal to $\rho = 3$. This value is in the middle to of the range of estimates that are regularly used in the literature (Landais and Spinnewijn, 2021). Regarding effort costs, I assume a functional form commonly used in the literature (e.g. Faberman et al., 2022; Heise and Porzio, 2023):

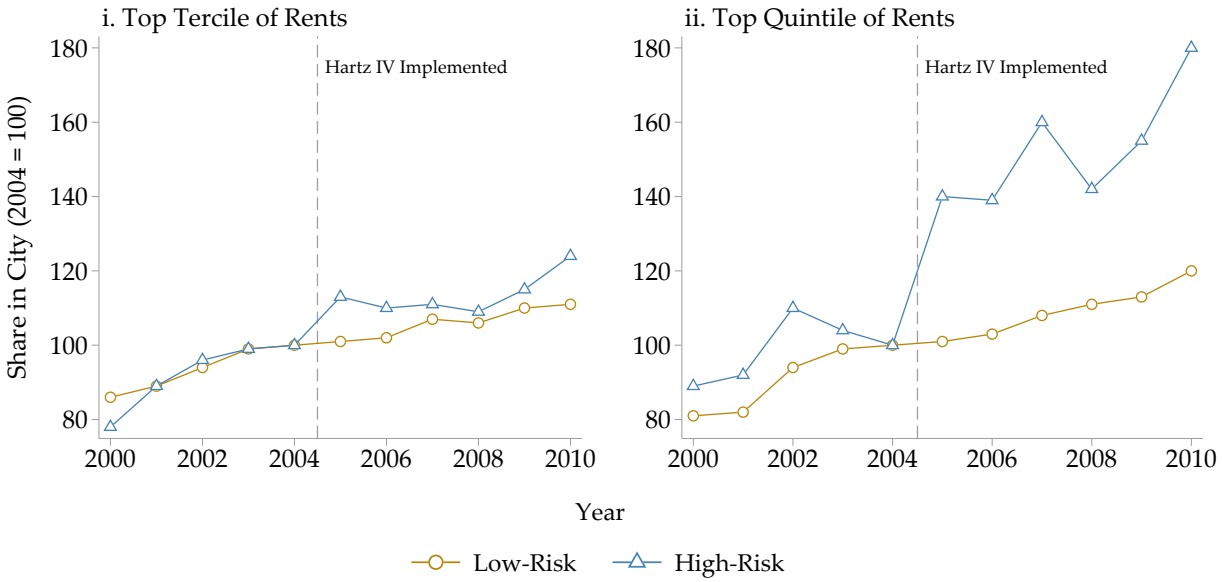
$$\psi(e_{jgt}) = k_g \cdot \frac{e_{jgt}^{1+(1/\gamma)}}{1 + (1/\gamma)} \tag{23}$$

γ corresponds to the search elasticity—capturing how effort responds to changes in the utility gap between the employed and unemployed state—and k_g governs the (group-specific) average effort levels. I estimate the search elasticity γ using a sample of jobseekers with matched survey information on search effort. In Appendix H.2, I derive the corresponding estimating equation and discuss the identification strategy. In my preferred specification (Table H.1, column (2)), I estimate a search elasticity of $\gamma = 0.261$, which is close to the estimate from Faberman et al. (2022).[28]

Estimating the migration elasticity. With the subutility function $U(\cdot)$ and the effort cost function $\psi(\cdot)$ calibrated, I now turn to estimating the parameter of interest σ. To remain consistent with my identification strategy for studying migration responses among the unemployed, I exploit differential exposure to the Hartz IV reform that arises from pre-determined risk levels at displacement. Specifically, I compare the location choices of high- and low-risk displaced workers across commuting zones that were differentially affected by the Hartz IV–induced change in long-term unemployment benefits. The estimating equation for risk group g in commuting zone j and year t is given by:

[28] When using the same effort measure – the number of applications – they estimate a search elasticity for workers who are on the job of $\gamma = 0.385$ (Table 3, p.1754 Faberman et al., 2022) .

Figure 10: Normalized Shares for Jobseekers that Live in High-Rent Locations



Notes: This figure shows the normalized shares of jobseekers that live in a city at the end of their UI spell. Panel i. defines cities as commuting zones in the top tercile of the rent distribution, whereas panel ii. only considers commuting zones in the top 20% of rents as cities. High-risk jobseekers correspond to the top 20% of predicted long-term unemployment risk, and low-risk jobseekers to the bottom half.

$$\log N_{jgt} = \sigma \cdot (1 - s_{jgt}) U\left(\frac{b_{jgt}}{p_{jt}}\right) + \sigma \cdot s_{jgt} U\left(\frac{w_{jgt}}{p_{jt}}\right) - \sigma \cdot \psi(e_{jgt}) + \eta_{gt} + \eta_{jg} + \eta_{jt} + \varepsilon_{jgt} \tag{24}$$

N_{jgt} is the stock of displaced jobseekers in location j from risk group g at the end of their UI spell in year t. Low-risk workers are displaced jobseekers in the bottom half of predicted risk, whereas unemployed workers from the top sixtile are considered high-risk, in line with the previous definition. s_{jgt} corresponds to the observed job-finding rate before entering long-term unemployment. b_{jgt} denotes simulated long-term unemployment benefits. Before the reform, benefits are computed using the replacement rate formula and therefore depend on previous earnings. After the reform, I assume recipients live in housing priced at the local ceiling. In both regimes, I assume jobseekers live in single-person households. w_{jgt} are observed re-employment wages. I normalize real benefits and wages such that consumption equals one at the median real benefit level, similar to Giannone et al. (2023).[29] To compare my migration response to previous estimates, I also estimate a specification that assumes log utility in a robustness analysis, which is the usual approach in the literature (e.g. Kalin et al., 2024; Muñoz, 2023). I estimate Equation (24) using all low- and high-risk displacement spells, rather than restricting the sample to individuals with observed effort. Accordingly, I proxy for effort costs using the model's first-order condition on effort (Equation (H.5)), together with observed wages, benefits, and search efficiency. Estimating this equation at the risk group–region–year level allows for including commuting zone–year fixed effects η_{jt}, which absorb contemporaneous shocks specific to a region but common across risk groups. I further include group-region fixed effects η_{jg} to control for permanent differences in location preferences between low- and high-risk jobseekers.

I instrument expected utility from real benefits with the interaction $\mathbf{1}\{g = \text{High Risk}\} \times \mathbf{1}\{t \geq$

[29] When using CRRA utility and real monthly benefits and wages (without normalization), expected utility differences across locations are very small, which leads to very large estimates of σ. This does not affect the migration elasticity—the object of interest—as σ is subsequently multiplied with $(b_j/p_j)^{1-\rho}$, which would re-normalize the large estimate for σ.

Table 3: Migration Elasticity Estimates

	Log Number of Jobseekers				
	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
$(1-s)\cdot\frac{\left(\frac{b_j}{p_j}\right)^{1-\rho}-1}{1-\rho}$	4.856***	1.523***	4.744***	3.715*	4.552**
	(0.927)	(0.029)	(1.002)	(2.114)	(2.210)
Migration Elasticity					
ϵ_{city}	0.282	0.088	0.275	0.216	0.264
	(0.054)	(0.047)	(0.058)	(0.123)	(0.128)
Commuting Zone-Year FE		✓		✓	✓
Effort Control					✓
Kleibergen-Paap F-stat.			65.0	45.0	46.9
Observations	3,068	3,068	3,068	3,068	3,068

Notes: This table shows the estimated coefficients from Equation (24), assuming CRRA preferences with coefficient of relative risk aversion $\rho = 3$. I instrument expected utility from real benefits with the interaction $\mathbf{1}\{g = \text{High Risk}\} \times \mathbf{1}\{t \geq 2005\} \times \text{Baseline Rent}_r$. Effort is proxied based on the first-order condition in Equation (H.5). The city migration elasticity ϵ_{city} is computed following Equation (20). Standard errors are clustered at the risk group× commuting zone level. Asterisks indicate *** p < 0.01, ** p < 0.05, * p < 0.1.

2005$\} \times$ Baseline Rent$_r$, leveraging the fact that the reform led to smaller benefit losses in high-price locations and disproportionately affected high-risk workers. This strategy relies on the assumption that high-rent locations did not become particularly more attractive to high-risk individuals after the reform for reasons unrelated to the benefit changes. Figure 10 provides graphical evidence that high-rent locations became particularly more attractive to high-risk workers following the implementation of the Hartz IV reform, with no signs of pre-trends in the years leading up to the reform.[30] Panel i. shows the time series for high-rent locations defined as commuting zones in the top tercile of baseline rents, while Panel ii. presents the corresponding series for the normalized share of jobseekers residing in commuting zones in the top quintile. Comparing the relative effect sizes across the two panels motivates using the continuous rent measure as an instrument, since the effects appear stronger when focusing on commuting zones at the very top of the rent distribution.

Table 3 presents the main estimates for σ, along with the estimate for the migration elasticity in the city—the key object of interest for Equation (11). To obtain the latter, I compute the migration elasticity for each commuting zone j based on Equation (20), and then aggregate up across all commuting zones in the top tercile of rents.[31] Across all specifications, the estimates for σ are statistically significant and fall within the range of 1.5 to 4.9. The IV estimates vary between 3.7 and 4.7 across all models, remaining robust when adding commuting zone-year fixed effects or controlling for (proxied) effort costs. The migration elasticity from the preferred model in column (5) corresponds to 0.264

[30] I show normalized shares as the model equivalent from the location choice equation would be $\log(N_{gjt}/N_{gt})$.

[31] More specifically, I compute:

$$\epsilon_{\text{city}} = \sigma \cdot (1 - \pi_{\text{city}}) \cdot \frac{\sum_{j \in \text{city}} \pi_j (1 - s_j) \cdot \left(\frac{b_j}{p_j}\right)^{1-\rho}}{\pi_{\text{city}}} \tag{25}$$

To compute this elasticity, I use average job-finding rates s_j, real benefits b_j/p_j and population shares for the whole sample of displaced jobseekers during the sample period from 2000 to 2010.

($p < 0.05$). Since the probability of actually relying on (long-term) unemployment benefits is relatively low in my sample (around 12%), the implied migration elasticity is smaller than the estimated inverse scale parameter σ. Assuming log utility instead of CRRA preferences yields slightly lower but similar estimates for the migration elasticity, as summarized in Table A.5

6.4 Mechanism

I now turn to the question of whether the regional component of the post-reform benefit formula—housing assistance—affects the migration choices of the unemployed. I exploit an institutional age cutoff, which discouraged jobseekers below 25 years to move before entering ALG 2. As outlined in Section 3.2, jobseekers below 25, who moved to a new flat, risked losing eligibility for housing assistance until they turned 25, even if that move occurred before claiming long-term unemployment benefits (§22 Paragraph 5, SGB II). Since this age cutoff was only introduced in February 2006, I can study whether potentially restricting housing assistance access for jobseekers aged 24 or below changed their migration choices differentially one year after implementation of the general Hartz IV reform. If housing assistance mattered for location choices of jobseekers, one would expect new long-term unemployment entrants below 25 years to behave differently from jobseekers of at least 25 years from 2006 and onwards.

In this analysis, I consider jobseekers as treated, if they enter long-term unemployment after their UI spell. While selection into long-term unemployment may have changed as a result of the reform, there is little reason to believe that this selection evolved differently for jobseekers under 25, particularly from 2006 onward. Although using predicted long-term unemployment risk to assign treatment status would be preferable, the small sample size of jobseekers under 30 in the top risk quantiles renders this approach infeasible. The control group is defined similar to the previous analysis. I consider displaced workers with low predicted risk of entering long-term unemployment as untreated. To have a sufficiently large control group that allows me to compare moving patterns for narrowly defined age groups, I keep the bottom half of the predicted risk distribution in my control group as opposed to the bottom sixtile as in the previous analysis. Figure B.6 illustrates that I estimate zero mobility effects for bottom half of predicted risk, suggesting that this group of displaced spells shows only negligible mobility responses to the reform.

I then estimate separate dynamic difference-in-difference specifications for jobseekers who enter long-term unemployment and the low-risk displaced. More specifically, for a spell s from one of the two groups g and that ends in year t, I estimate:

$$y_s^g = \alpha_0^g + \sum_{k \neq 2004} \delta_k^g (\mathbf{1}\{t = k\} \times \mathbf{1}\{\text{Age} \geq 25\})_s + \alpha_1^g \{\text{Age} \geq 25_s\} + \lambda_{t(s)}^g + X_s' \eta^g + \epsilon_s \qquad (26)$$

y_s is a moving indicator variable of interest, for example moving to an urban destination. X_s consists of worker characteristics at entry of the UI spell such as sex, education and migration background. I restrict the sample to age groups 20 to 29 to limit the comparison to age groups just around the cutoff.

Figure 11a shows the estimated profiles for moves towards cities, separately for spells entering long-term unemployment (Panel i.) and for spells at low-risk of entering long-term unemployment (Panel ii.). Panel i. shows that among entrants into long-term unemployment, jobseekers aged 25 to 29 *and* jobseekers aged 20 to 24 were significantly more likely to move towards cities in the first year once the reform became active. However, consistent with not being eligible for housing assistance after

Figure 11: Estimated Profiles for Moves towards Cities by Treatment and Age Groups

Notes: This figure shows the implied profiles for moves towards urban destinations (a) and towards rural regions (b) from estimating Equation (26). Cities and rural areas are defined according to the Thünen classification (see Meister et al., 2019). Panel i. shows the estimated profile for UI claimants that enter long-term unemployment. Panel ii. depicts the corresponding profile for UI claimants with low predicted risk of entering long-term unemployment (bottom 50% of predicted risk score). Capped spikes denote 95 percent confidence intervals.

relocating, unemployed workers at the age of 24 or below no longer have significantly higher mobility towards cities from 2006 and onwards, compared to before the reform. This is in stark contrast to claimants aged 25 to 29, who demonstrate consistently higher moving rates towards cities throughout all years after the reform. None of the two patterns can be found for UI spells at low risk of long-term unemployment (Panel ii.), in line with these groups unlikely being affected by the reform.

Figure 11b illustrates that the dynamics for moves towards rural destinations do not follow the same dynamics among spells entering long-term unemployed as moves towards cities. In particular, moves towards rural areas are below their pre-reform levels for both age groups. Moreover, the additional restrictions for claimants aged 24 and below do not seem to have affected that age group

differently from 2006 and onwards. In Appendix Figures B.9 and B.10, I document similar dynamics when classifying moves by rents of the destination instead of population density.

These results suggest that access to housing assistance plays a crucial role in shaping the locations to which jobseekers migrate. Appendix Table A.4 further shows that eligibility for housing assistance also influences the type of flats to which the unemployed move. For this analysis, I use information from the matched survey data on the individual housing situation and again exploit the age cutoff in eligibility for the housing subsidy. Specifically, I estimate among jobseekers who enter long-term unemployment (previously "treated") the following specification:

$$\Delta \log y_{it} = \beta_0 + \beta_1 \mathbf{1}\{\text{Age}_{it} \geq 25\} + \beta_2 \mathbf{1}\{\text{Moved}_{it}\} + \beta_3 (\mathbf{1}\{\text{Age}_{it} \geq 25\} \times \mathbf{1}\{\text{Moved}_{it}\}) + \Delta X_{it}' \gamma + \epsilon_{it} \tag{27}$$

where Δy_{it} captures changes in housing characteristics for jobseeker i between the years t and $t-1$. Table A.4 indicates that movers who qualify for the housing subsidy relocate to flats that are 12 log points more expensive (27 log points in terms of price per square meter) than their previous dwelling, whereas movers without access (those under 25) show no increase in rents.

Together, these results support the previous results by showing that moves among claimants with access to housing benefits seem to be directed towards more expensive locations and individual housing units, compared to movers with more restricted access.

7 Fiscal Effect of Movers

In the previous section, I documented that rent-indexed long-term unemployment benefits increase migration toward destinations with higher benefits—typically more urban and expensive regions. Equation (11) highlights that the fiscal externality of these moves crucially depends on whether the movers rely more or less on unemployment benefits at their destination, compared to their counterfactual benefit receipt at origin. I therefore now turn to how the labor market trajectories evolve around moving toward a city, using jobseekers with similar observable characteristics prior to displacement at the same origin to construct a counterfactual. My main estimation sample consists of jobseekers who were displaced after the reform, namely between 2004 and 2013. I extend the cohort window to 2013 (instead of 2010) to increase the number of movers. The results are quantitatively similar when only using the cohorts up to 2010 as in Section 6.

7.1 Identification

Matching algorithm. I deploy a propensity score matching event study-design to compare the labor market trajectories of displaced movers to displaced stayers with similar pre-displacement characteristics. More precisely, I use a two-step matching procedure similar to Schmieder et al. (2023). I first construct cells based on the year of displacement, the predicted long-term unemployment benefit risk quintile, sex, household composition (with or without children), a broad industry classification of the previous job (manufacturing or services) and location prior to displacement (east × population quintile).[32] By choosing these narrowly defined cells, I aim to match movers to stayers exactly on a rich set of characteristics, while still achieving a high matching rate. Within each of these cells, I estimate

[32] Schmieder et al. (2023) also match exactly on year of displacement, sex and industry. Huttunen et al. (2018), who also study the labor market trajectories of movers relative to stayers, additionally control for an urban/rural indicator prior to displacement.

a propensity score for moving towards a city using a rich set of worker characteristics and assign the displaced stayer with the closest propensity score without replacement. In the propensity score regression, I use a quadratic term in age, indicators for education and migration background as well as the worker's log wage, unemployment benefits and employment status in the two years leading up to displacement as predictors.

Table 4 shows average characteristics of my sample of displaced movers (column (1)) and the sample of matched stayers (column (2)). Across the set of variables shown, movers differ very little from their matched stayers. This also holds for the set of variables, which are not included in the propensity score prediction, as for instance characteristics during the past three or five years prior to displacement. Moreover, even though I do not match exactly on the origin county, the location characteristics at entry into unemployment insurance are balanced between movers and stayers. I provide evidence on the balance of additional covariates—which are unobserved in the main administrative dataset (SIAB)—using matched survey data from the PASS-ADIAB dataset in Table A.6.[33]

Event Study. Using the matched mover-stayer sample, I then estimate the following equation for displaced worker i of displacement cohort c at time t:

$$y_{ict} = \sum_{\substack{k=-4 \\ k\neq -1}}^{6} \delta_k \cdot (\mathbf{1}\{\text{Moved}\}_i \times \mathbf{1}\{t = c + k\}) + \sum_{\substack{k=-4 \\ k\neq -1}}^{6} \gamma_k \times \mathbf{1}\{t = c + k\} + \pi_t + \alpha_i + X_{it}'\beta + \epsilon_{ict} \quad (28)$$

y_{itc} is the labor market outcome of interest, mainly annual (or cumulative) unemployment benefits received. π_t and α_i are year and worker fixed effects, respectively. X_{it} controls for age. The coefficients of interest are δ_k, which measure the differential impact of moving to a city on labor market outcomes k years around displacement, compared to staying in a non-urban location. The estimation sample is balanced for the entire event time, as I keep individuals in the sample even if they have no affiliation with social security in a given year (as e.g. Schmieder et al., 2023).

7.2 Results

Figure 12 shows the means and estimated event study coefficients for the main outcome of interest—unemployment benefits received— among workers that were displaced between 2004 and 2013. Figure 12 shows cumulative benefits received over the event time, normalized to zero in the year before displacement, to capture the cumulative fiscal burden imposed by movers to high-rent destinations.[34] Figure B.11 in the Appendix shows the effects on annual instead of cumulative benefits received.
Movers and stayers have similar benefit trajectories and levels before displacement. As a consequence of losing their job, received benefits increase in the immediate years after job loss. Notably, movers consume higher benefits during most years after displacement, as cumulative benefits increase for all post-displacement years (Panel ii.). Appendix Figure B.12 shows that movers consume higher

[33] In particular, the survey items in this dataset allow for examining household composition, receipt of other welfare transfers, and housing characteristics in the years preceding displacement. I apply the same matching algorithm as in the main analysis. Due to the limited number of city-bound movers with survey responses prior to displacement, I consider all moves across commuting zones as treated in this exercise.

[34] I compute unemployment benefits by multiplying the number of days on benefits (both UI and LTU) within a year with the daily benefit generosity. As I don't observe the benefit generosity when receiving long-term unemployment benefits after the reform, I use the simulated benefits from programmatic rules as outlined in Section 3.1. I show robustness to different assumptions on the simulated benefit levels.

Table 4: Characteristics of Matched Movers and Stayers after the Reform

Variable	(1) Mover	(2) Stayer	(3) Difference (1 - 2)	(4) SE Difference
A. Worker Characteristics				
Female	0.502	0.502	0.000	0.014
Age	32.7	32.5	0.201	0.282
Non-German	0.100	0.100	0.000	0.008
Low-Skilled	0.163	0.157	0.012	0.010
Medium-Skilled	0.715	0.737	-0.022	0.012
High-Skilled	0.123	0.106	0.017	0.009
B. Last Job Characteristics				
Monthly Wage	1819	1794	25.6	29.7
Tenure (years)	2.372	2.379	-0.011	0.073
Parttime	0.132	0.129	0.003	0.009
Regular Job	0.827	0.845	-0.018	0.010
C. Prior Characteristics				
Last 3 Years				
Average Monthly Wage	1745	1728	16.6	29.2
Share Unemployed	0.098	0.103	-0.005	0.004
Last 5 Years				
Average Monthly Wage	1691	1689	2.0	28.8
Share Unemployed	0.114	0.122	-0.008	0.005
D. Origin Location Characteristics				
Log Population	12.17	12.18	-0.007	0.016
Log Rent	1.67	1.66	0.009	0.004
Log GDP per Worker	11.00	10.99	0.001	0.004
Job-Finding Rate	0.041	0.042	-0.002	0.001
Number of Workers	2663	2663		

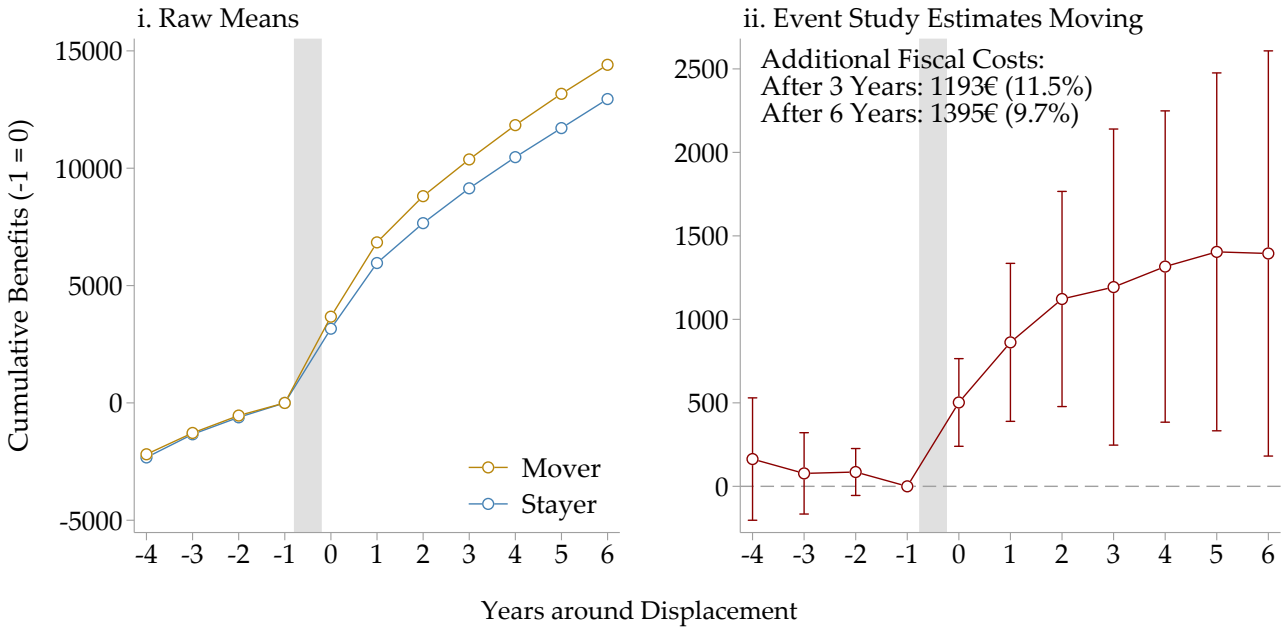
Notes: This table shows mean characteristics of displaced workers in the matched mover-stayer sample for displacement cohorts 2004 to 2013. Movers refer to displaced workers, who move to a city after displacement. Stayers refer to displaced workers, who stay in their rural location after displacement. For details on the matching algorithm, see text. Wages are in Euros in 2015 prices.

UI benefits in the first two years after displacement, whereas the subsequent effect is coming from higher long-term unemployment benefits. These effects are primarily driven by spending more days on unemployment benefits (Figure B.13), and less by differences in the benefit level.

My estimates from Panel 12 suggest that movers have received an additional €1193 in unemployment benefits after three years, and €1395 after six years, relative to the counterfactual of being displaced and staying in a rural region. To remain consistent with the timing of the static model that I have used, I use the estimate from three years after displacement. Relative to total benefits in high-rent locations at displacement, my estimate corresponds to an additional fiscal cost of €1193/€10374 $\approx$ 11.5%.[35] Taken at face value, this number corresponds to the fiscal costs imposed by movers to higher-benefit destinations as in Equation (11).

[35]The 95% confidence interval is [2.4%, 20.6%].

Figure 12: Means and Matched Event Study Estimates for Unemployment Benefits Received



Notes: This figure shows raw means and matched event study estimates from Equation (28) for cumulative unemployment benefits received. Benefits are computed by multiplying the number of days on unemployment benefits (UI and LTU), multiplied with the daily benefit rate. The treated group consists of movers, who relocate to a commuting zone in the top tercile of rents during unemployment insurance. The control group consists of matched stayers, who remain in a rural area after displacement. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals.

7.3 Mechanism

I first show in Figure 13 that the increase in unemployment benefit consumption is mirrored by lower re-employment rates after displacement.

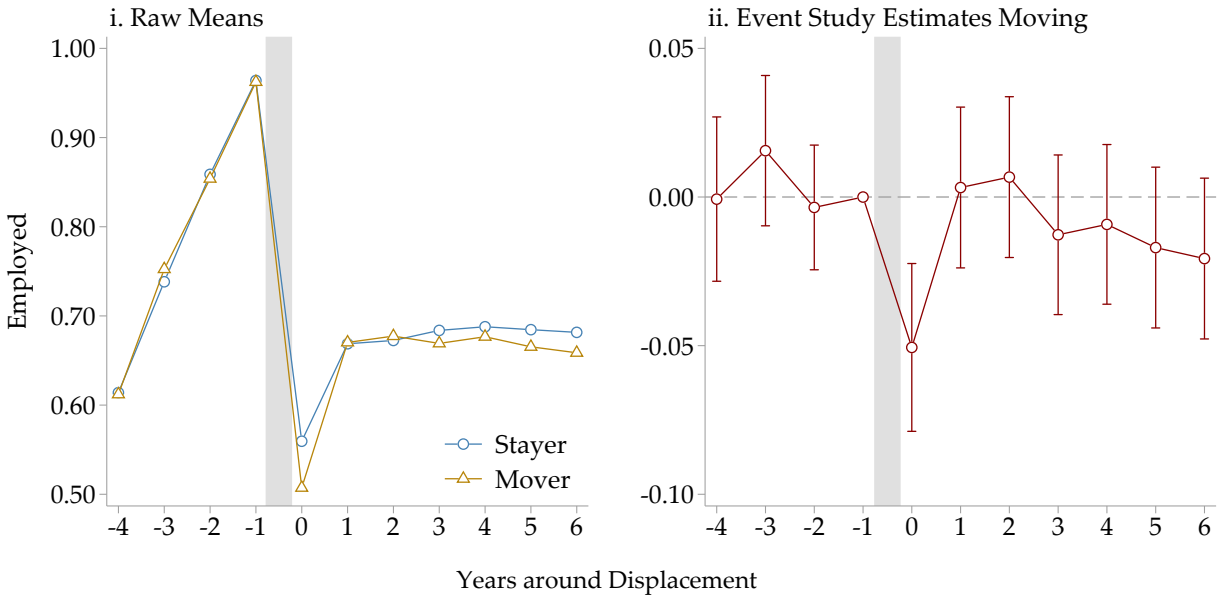
Movers and stayers follow similar employment trajectories before displacement. Employment rates rise in the period leading up to displacement, since eligibility for unemployment insurance requires prior contributions to social security. The first year after displacement shows the largest divergence between the two groups, with movers exhibiting re-employment rates about 5 percentage points lower than those of stayers. Subsequently, movers to urban locations catch up with stayers, showing only slightly lower employment rates from the third year after displacement onward.

Next, I examine the dynamics of employment following displacement in more detail. I hypothesize that the initial drop in employment reflects initial adjustment costs, as they require time to navigate the new local labor market. To test this hypothesis, I repeat the matching exercise but distinguish between two types of moves. First, I examine all relocations across commuting zones, regardless of whether the destination is urban. Second, I consider local moves within counties, identifiable for the matched subset of displaced workers in the PASS survey data. These moves are intended to capture relocations that clearly occur within the same labor market.[36] In the admin-survey data, I am able to match 371 local movers to observationally similar stayers.

Figure B.14 in the Appendix shows the matched event study by type of move when looking at employment. While moves across commuting zones exhibit a similar dip in the immediate years after displacement, local movers show no significant decline in re-employment rates when relocating

[36] I could in principle also detect moves across counties within commuting zones in the main administrative dataset. However, as I want to focus on very local moves, I use the matched survey dataset for this exercise.

Figure 13: Means and Matched Event Study Estimates for Employment



Notes: This figure shows raw means and matched event study estimates from Equation (28) for employment. Employment is defined as having a job at the 30th of June in a given year. The treated group consists of movers, who relocate to a commuting zone in the top tercile of rents during unemployment insurance. The control group consists of matched stayers, who remain in a rural area after displacement. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals.

within the same location. Despite the wide confidence intervals due to the small local-mover sample, the point estimates are close to zero and suggest no adverse employment effects from relocating locally during unemployment. Taken together, these findings suggest that movers across commuting zones require more time to integrate into their new labor market, which helps explain the pronounced dip in re-employment among movers to cities.

8 Calibration of Welfare Effects

In this section, I combine the estimated sufficient statistics from Sections 5, 6, and 7 with the theoretical framework to assess the welfare implications of redistributing benefits across space. I begin by considering marginal reforms to Germany's long-term unemployment benefit system after Hartz IV. I then show how my framework and estimates can be used to compute the (globally) optimal design of unemployment benefits across space.

8.1 The Welfare Effects of Marginal Reforms

I first examine whether redistributing benefits toward the city in a budget-neutral way increases welfare. Following Proposition 1, there would be welfare gains if and only if the following inequality holds:

$$\left(\frac{b_c/p_c}{b_r/p_r}\right)^{-\rho} \cdot \left(\frac{p_c}{p_r}\right)^{-1} > \underbrace{\frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}}_{\text{Duration Responses}} + \underbrace{\frac{\epsilon_{N_c,b_c}}{(1+\epsilon_{1-s_r,b_r})N_r}}_{\text{Migration Response}} \cdot \underbrace{\frac{(1-s_c)b_c-(1-s_r)b_r}{(1-s_c)b_c}}_{\text{Fiscal Effect of Movers}_{r\to c}} \tag{29}$$

To compute both sides of this inequality, I use my empirical estimates for the right-hand side together with data on benefit and price differences during the first years after Hartz IV. Following the migra-

Table 5: Parameters for Marginal Benefit Reforms

Description	Parameter	Value (SE)	Source
Panel A: Estimates			
Unemployment Duration Response in City	ϵ_{1-s_c,b_c}	0.64 (0.24)	Table 1
Unemployment Duration Response in Rural	ϵ_{1-s_r,b_r}	0.58 (0.15)	Table 1
Migration Response to City	ϵ_{N_c,b_c}	0.264 (0.128)	Table 3
Fiscal Effect of Movers	$\Delta\text{Fiscal}_{r\to c}$	0.115 (0.046)	Figure 12
Panel B: Data (2005-2010)			
Price Index in City	p_c	1.057	Section 3.1
Price Index in Rural	p_r	0.951	Section 3.1
Population Share in Rural	N_r	0.49	SIAB
Panel C: Other Parameters			
Coefficient of Relative Risk Aversion	ρ	3	Literature

Notes: This table summarizes the estimates, moments in the data and externally calibrated parameters that I use to compute Equations (29) and (32).

tion elasticity results, I choose a coefficient of relative risk aversion equal to the midpoint ($\rho = 3$) of the range of estimates that are regularly used in the literature (Landais and Spinnewijn, 2021). Table 5 summarizes all inputs used to compute Equation (29).

Consumption smoothing benefits. By evaluating the left-hand side of Equation (29) at the post–Hartz IV benefit schedule, I implicitly assume that marginal utilities of consumption in unemployment are approximately equalized between urban and rural jobseekers. This assumption may be violated if unemployed workers can smooth consumption differently across locations through channels not captured by the model, such as spousal labor supply or asset holdings.

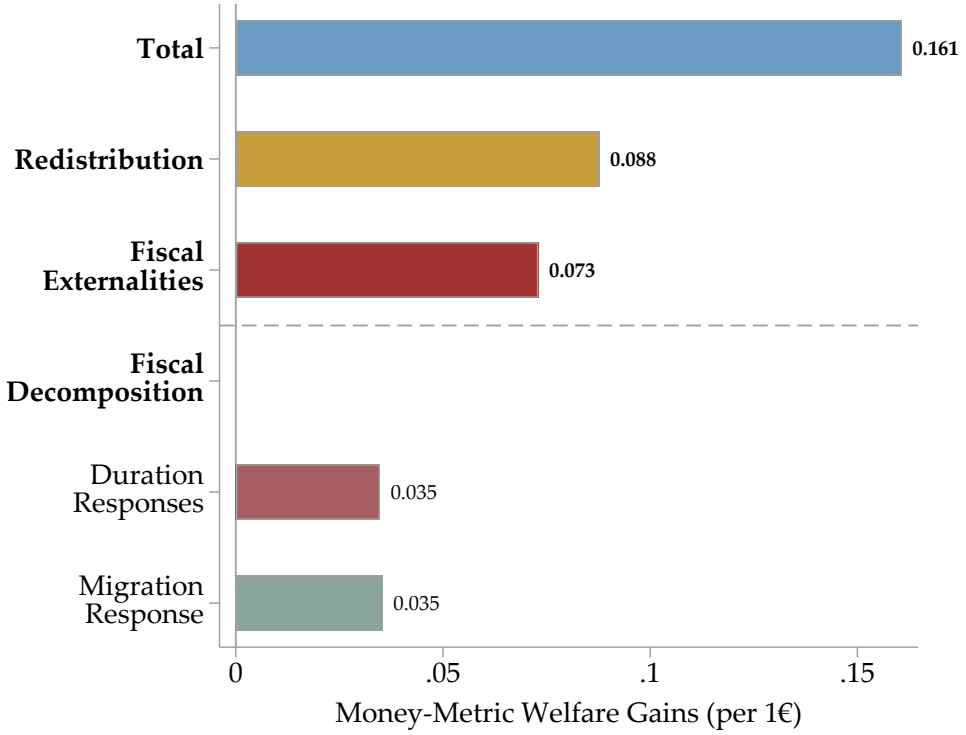
I empirically assess this assumption by testing whether marginal utilities of consumption differ between unemployed workers in urban and rural areas. Appendix I.1 shows that the ratio of marginal utilities can be approximated using the following estimable objects:

$$\frac{U'(c_c^U)}{U'(c_r^U)} \approx \left(1+\gamma\frac{c_r^E-c_c^E}{c_r^E}\right)\cdot\frac{1+\gamma\frac{c_c^E-c_c^U}{c_c^E}}{1+\gamma\frac{c_r^E-c_r^U}{c_r^E}} \tag{30}$$

The first term captures differences in consumption levels prior to entering long-term unemployment, while the second captures differences in consumption drops at entry into long-term unemployment. Appendix I describes in detail the estimation of each of the two terms. The results in Appendices I.2 and I.3 show that neither term differs significantly between urban and rural jobseekers entering long-term unemployment, and that the corresponding point estimates are very similar. This evidence suggests that marginal utilities of consumption are approximately equalized across the two groups, consistent with the strong price indexation of benefits. Accordingly, I use the post-reform benefit schedule reported in Table 5 in the subsequent welfare analysis.

Welfare effects of local policy reforms. Equation (31) presents one key result of the paper. Indexing unemployment benefits to local prices, as introduced after the Hartz IV reform, leads to excessively generous benefits in high-price locations. Consequently, further increasing benefits in cities entails

Figure 14: Decomposition of Welfare Impact



Notes: This figure shows the money-metric welfare impact of redistributing benefits from urban to rural regions, per €1 transferred to the unemployed in rural regions. Welfare gains are computed using Equation (32 and are measured relative to the marginal utility of the unemployed in urban locations. The welfare effects of each channel are obtained as the difference between the estimated welfare gains and welfare gains in a counterfactual, in which this component does not impact welfare. See text for details.

welfare *losses*.

$$\underbrace{\left(\frac{b_c/p_c}{b_r/p_r}\right)^{-\rho}\cdot\left(\frac{p_c}{p_r}\right)^{-1}}_{\approx 0.92} < \underbrace{\frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}+\frac{\epsilon_{N_c,b_c}}{(1+\epsilon_{1-s_r,b_r})N_r}\cdot\frac{(1-s_c)b_c-(1-s_r)b_r}{(1-s_c)b_c}}_{\approx 1.08} \tag{31}$$

Instead, reforms should redistribute benefits toward low-price locations. Specifically, the money-metric welfare gains per €1 transferred to unemployed workers in low-price regions are given by:

$$\frac{dW}{(1-s_r)N_r\cdot db}\Big/\frac{\left(\frac{b_c}{p_c}\right)^{-\rho}}{p_c} = \underbrace{\left(\frac{b_c/p_c}{b_r/p_r}\right)^{\rho}\cdot\left(\frac{p_r}{p_c}\right)^{-1}}_{\approx 1.088} - \underbrace{\left[\frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}+\frac{\epsilon_{N_c,b_c}}{(1+\epsilon_{1-s_r,b_r})N_r}\cdot\left(1-\frac{(1-s_r)b_r}{(1-s_c)b_c}\right)\right]^{-1}}_{\approx 0.927}$$
$$\approx 0.161 \tag{32}$$

For each euro transferred to the unemployed in cheaper locations, the social gains are approximately 16 cents. To decompose this effect, I compute the change in welfare gains when each component is introduced separately. Specifically, I calculate the difference between computed welfare gains and a counterfactual, in which this component is not welfare-relevant.[37] Figure 14 presents the results of this decomposition.

Approximately 55% of the welfare gains come from redistribution (€0.088). The marginal utilities

[37] For example, migration would not be welfare-relevant if either the migration elasticity is zero, if movers do not impose additional fiscal costs or both.

of consumption after Hartz IV were almost equalized across space due to the strong indexation of benefits to local prices. If marginal utilities are roughly equal, but one euro of benefits buy more units of consumption in rural locations ($1/p_j > 1/p_c$), there are welfare gains from redistributing benefits towards cheaper regions. While the magnitude of redistributive gains depends on the assumed degree of risk aversion, I show in Appendix Figure B.15 that redistribution yields welfare gains even for high levels of risk aversion. The remaining welfare gains (€0.073) arise from lower fiscal externalities following the redistribution of benefits from urban to rural regions. On the one hand, unemployment durations decline more in cities than they rise in rural areas when benefits are reallocated. On the other hand, lower benefits in cities reduce the fiscal externalities associated with urban migration.

8.2 Optimal Regional Unemployment Benefits

I now show how my framework and estimates can be used to solve for the (globally) optimal design of unemployment benefits across space. While the estimated fiscal externalities are informative for assessing local reforms around the observed benefit design, their magnitudes depend on the prevailing benefit level and therefore adjust under alternative allocations. This dependence is most evident for the fiscal effect of movers, but it also applies to the duration and migration elasticities. I now demonstrate how the model from Section 2 can account for changes in elasticities under alternative benefit allocations.

Specifically, the first-order conditions from the planner's problem are given by:

$$\frac{U'\left(\frac{b_j}{p_j}\right)}{p_j} = \lambda \left[(1 - s_j(b_j)) b_j \cdot (1 + \epsilon_{1-s_j, b_j}(b_j)) + \epsilon_{N_j, b_j}(\boldsymbol{b}) \cdot \frac{(1 - s_j(b_j)) - \overline{B}}{1 - N_j(\boldsymbol{b})} \right] \quad \text{for all } j \tag{33}$$

where λ is the shadow value of taxpayers' funds, i.e. the Lagrange parameter on the federal budget constraint. The first-order conditions in Equation (33) can be rewritten as:

$$\frac{(1 - s_j(b_j)) \cdot b_j}{\sigma} + \frac{1 + z_j^2 \cdot \frac{U'\left(\frac{b_j}{p_j}\right)/p_j}{\psi''(e_j(b_j))} \cdot \frac{b_j}{(1 - s_j(b_j))}}{U'\left(\frac{b_j}{p_j}\right)/p_j} = \frac{1}{\lambda} + \frac{\overline{B}}{\sigma} \quad \text{for all } j \tag{34}$$

For J locations, these first-order conditions and the budget constraint define a nonlinear system of $J + 1$ equations in as many unknowns ($\boldsymbol{b}$ and λ). To solve these equations numerically for $\boldsymbol{b}$ and λ, I require estimates of the structural parameters governing migration (σ), the utility function (ρ) and search effort cost function (ϕ, k). Table 6 summarizes the estimated parameter values, as well as other data inputs used to solve for the global optimum.

In the next step of this analysis, I will use the first-order conditions from Equation (34), together with the parameters reported in Table 6, to solve for the optimal allocation of long-term unemployment benefits across German commuting zones.

9 Conclusion

In this paper, I study the effects of making unemployment benefits more generous in high-price locations, and more generally, how local economic conditions affect the optimal design of unemployment benefits. I show theoretically that price differences, local duration and migration responses shape the optimal allocation of unemployment benefits across space. I then estimate the fiscal costs of behav-

Table 6: Parameters for Optimal Benefit Design

Description	Parameter	Value (SE)	Source
Panel A: Estimates			
Migration responsiveness	σ	4.55 (2.11)	Table 3
Search effort elasticity	ξ	0.261 (0.074)	Table H.1
Panel B: Data (2005-2010)			
Price index	p_j		Section 3.1
Reemployment wages	w_j		SIAB
Search efficiency	z_j		PASS-ADIAB
Benefits per recipient	$\overline{B}$	€650	IAB (2017)
Panel C: Internally Calibrated			
Search cost shifter	k	0.03	Match average effort
Panel D: Other Parameters			
Coefficient of relative risk aversion	ρ	3	Literature

Notes: This table summarizes the estimates, moments in the data as well as internally and externally calibrated parameters that I use to solve for the global optimum in Equation (34). Benefits and wages refer to monthly amounts, and are in 2015 (national) euros.

ioral distortions in durations and location choice when tying unemployment benefits to local prices, using quasi-experimental variation from Germany's Hartz IV reform.

I find that indexing unemployment benefits to local prices leads to overly generous benefits in high-price locations. Instead, narrowing benefit differences between locations with different prices improves social welfare, which is driven by both gains from redistribution and lower fiscal externalities of unemployment benefits.

This paper leaves several important questions for future research. First, I study long-term unemployed workers, for whom I find only limited differences in job-finding opportunities across space. For jobseekers with higher human capital or at earlier stages of their unemployment spell, cities might offer significantly better reemployment opportunities, as suggested by evidence from Moretti and Yi (2024) for the US. Quantifying duration and migration responses for these groups could therefore be informative about how space matters for benefit systems like unemployment insurance—in comparison to long-term unemployment benefits. Second, a large literature finds no positive employment effects for *parents* who move to high-opportunity locations, but substantial gains for their children (e.g. Chetty et al., 2016). Examining the early career trajectories of children whose parents relocated to high-cost commuting zones because of the reform would therefore be a promising direction for future research. Third, this paper abstracts from the effects of regional unemployment benefits on location choices *within* commuting zones. Yet, benefits that are uniformly set at the commuting zone might also affect sorting decisions across neighborhoods. Since previous research has shown that local neighborhood characteristics can strongly shape labor market trajectories (e.g. Bayer et al., 2008), future work should examine how regional unemployment benefits affect household sorting and, in turn, labor market outcomes within commuting zones.

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I Appendix

A Additional Tables

Table A.1: RKD Results for Different Duration Outcomes

	Unemployment Duration (Weeks)			
Censored	$\leq$ 2 Years	$\leq$ 3 Years	$\leq$ 4 Years	Uncensored
Panel A: Reduced Form				
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}$	-0.042***	-0.054***	-0.063***	-0.087***
	(0.010)	(0.014)	(0.017)	(0.027)
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}\cdot\mathbf{1}\{\text{City}\}$	0.008	0.010	0.011	0.021
	(0.015)	(0.020)	(0.025)	(0.039)
Panel B: IV				
b	0.056***	0.070***	0.080***	0.111***
	(0.013)	(0.018)	(0.022)	(0.035)
$b\cdot\mathbf{1}\{\text{City}\}$	-0.003	-0.001	0.000	-0.004
	(0.023)	(0.031)	(0.038)	(0.061)
Elasticities				
ϵ_{city}	0.59	0.64	0.68	0.74
	(0.21)	(0.24)	(0.27)	(0.36)
ϵ_{rural}	0.55	0.58	0.60	0.69
	(0.13)	(0.15)	(0.17)	(0.22)
Ceiling FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Mean Duration	41.0	48.5	53.8	64.9
Kernel	uniform	uniform	uniform	uniform
Observations	89,661	89,661	89,661	89,661

Notes: This table shows heterogeneous effects of benefit changes on unemployment duration in weeks, obtained from estimating Equation (15). Panel A shows the reduced-form estimates, whereas Panel B shows IV results. All duration outcomes record the number of weeks on long-term unemployment benefits, but are right-censored at different intervals. Benefits correspond to monthly benefits that jobseekers receive from the seventh month onward of their long-term unemployment spell. The elasticities are obtained by multiplying the IV-estimate with the ratio of average benefits and unemployment duration, separately for the city and rural location. Standard errors are clustered at the ceiling level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: RKD Results by Bandwidth around Local Ceiling

	Unemployment Duration (Weeks)							
Bandwidth	25	50	75	100	125	150	175	200
Panel A: Reduced Form								
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}$	-0.079	-0.049	-0.034	-0.047***	-0.054***	-0.051***	-0.044***	-0.043***
	(0.115)	(0.042)	(0.025)	(0.017)	(0.014)	(0.011)	(0.009)	(0.008)
$(r-c)\cdot\mathbf{1}\{r-c\geq 0\}\cdot\mathbf{1}\{\text{City}\}$	-0.048	0.069	0.005	0.008	0.012	0.008	-0.011	-0.013
	(0.213)	(0.071)	(0.039)	(0.025)	(0.020)	(0.016)	(0.014)	(0.011)
Panel B: IV								
b	0.404	0.060	0.043	0.063**	0.070***	0.071***	0.069***	0.073***
	(1.053)	(0.089)	(0.030)	(0.025)	(0.018)	(0.016)	(0.015)	(0.014)
$b\cdot\mathbf{1}\{\text{City}\}$	-0.932	-0.106	0.015	0.005	-0.001	-0.010	0.008	0.017
	(2.223)	(0.200)	(0.071)	(0.043)	(0.031)	(0.025)	(0.025)	(0.023)
Elasticities								
ϵ_{city}	-4.86	-0.43	0.53	0.63	0.58	0.59	0.57	0.86
	(-11.90)	(1.85)	(0.63)	(0.35)	(0.24)	(0.20)	(0.19)	(0.18)
ϵ_{rural}	3.30	0.49	0.35	0.51	0.64	0.58	0.72	0.60
	(8.62)	(0.72)	(0.32)	(0.21)	(0.15)	(0.13)	(0.12)	(0.12)
Ceiling FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Kernel	uniform	uniform	uniform	uniform	uniform	uniform	uniform	uniform
Observations	23,538	43,478	60,831	76,224	89,661	100,099	108,631	115,604

Notes: This table shows heterogeneous effects of benefit changes on unemployment duration in weeks, obtained from estimating Equation (15). Different columns correspond to different bandwidths that are used for the estimation samples. Panel A shows the reduced-form estimates, whereas Panel B shows IV results. All duration outcomes record the number of weeks on long-term unemployment benefits, but are right-censored at different intervals. Benefits correspond to monthly benefits that jobseekers receive from the seventh month onward of their long-term unemployment spell. The elasticities are obtained by multiplying the IV-estimate with the ratio of average benefits and unemployment duration, separately for the city and rural location. Standard errors are clustered at the ceiling level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3: General Migration DiD Results with Worker FE

	Moves	
across	County	Commuting Zone
$\mathbf{1}\{\text{Post}\}$	0.002	0.001
	(0.002)	(0.002)
$\mathbf{1}\{\text{High Risk}\} \cdot \mathbf{1}\{\text{Post}\}$	0.014***	0.011***
	(0.004)	(0.004)
Worker FE	✓	✓
Observations	52,189	52,189

Notes: This table shows static difference-in-difference effects from estimating Equation (19) among the sample of displaced jobseekers that enter unemployment before and after the reform, and who are either low- or high-risk in both periods. Standard errors are clustered at the jobseeker level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4: Effects of Moving on Selected Housing Characteristics, Heterogeneous by Age

	Δ Rent	Δ Rent per Sqm	Δ Sqm
	(1)	(2)	(3)
$\mathbf{1}\{\text{Age} \geq 25\}$	-0.006	0.021	-0.019*
	(0.016)	(0.016)	(0.011)
$\mathbf{1}\{\text{Moved}\}$	0.006	0.019	-0.005
	(0.039)	(0.028)	(0.034)
$\mathbf{1}\{\text{Age} \geq 25\} \cdot \mathbf{1}\{\text{Moved}\}$	0.120**	0.267***	-0.142**
	(0.056)	(0.100)	(0.060)
Controls	✓	✓	✓
Year FE	✓	✓	✓
Mean Outcome	0.04	0.02	0.01
Observations	2,459	2,459	2,459

Notes: This table shows the estimated coefficients from Equation (27). The treated group consists of jobseekers who enter long-term unemployment and are aged 25 to 29, whereas the control group encompasses ages 20 to 24. Controls include indicators for changes in the household composition and year fixed effects. Standard errors are clustered at the jobseeker level.

Table A.5: Migration Elasticity Estimates with Log Utility

	Log Number of Jobseekers		
	(1)	(2)	(3)
Panel A: IV Estimates			
$(1-s)\cdot\log\left(\frac{b}{p}\right)$	3.547***	2.661***	1.940*
	(1.266)	(0.644)	(1.011)
Migration Elasticity			
ϵ_{city}	0.259	0.194	0.141
	(0.131)	(0.047)	(0.074)
Commuting Zone-Year FE			✓
Effort Control		✓	✓
Kleibergen-Paap F-stat.	10.3	25.0	8.6
Observations	3,068	3,068	3,068

Notes: This table shows the estimated coefficients from Equation (24), assuming log utility. I instrument expected utility from real benefits with the interaction $\mathbf{1}\{g = \text{High Risk}\} \times \mathbf{1}\{t \geq 2005\} \times \text{Baseline Rent}_r$. Effort is proxied based on the first-order condition in Equation (H.5). The city migration elasticity ϵ_{city} is computed following Equation (20). Standard errors are clustered at the risk group$\times$ commuting zone level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

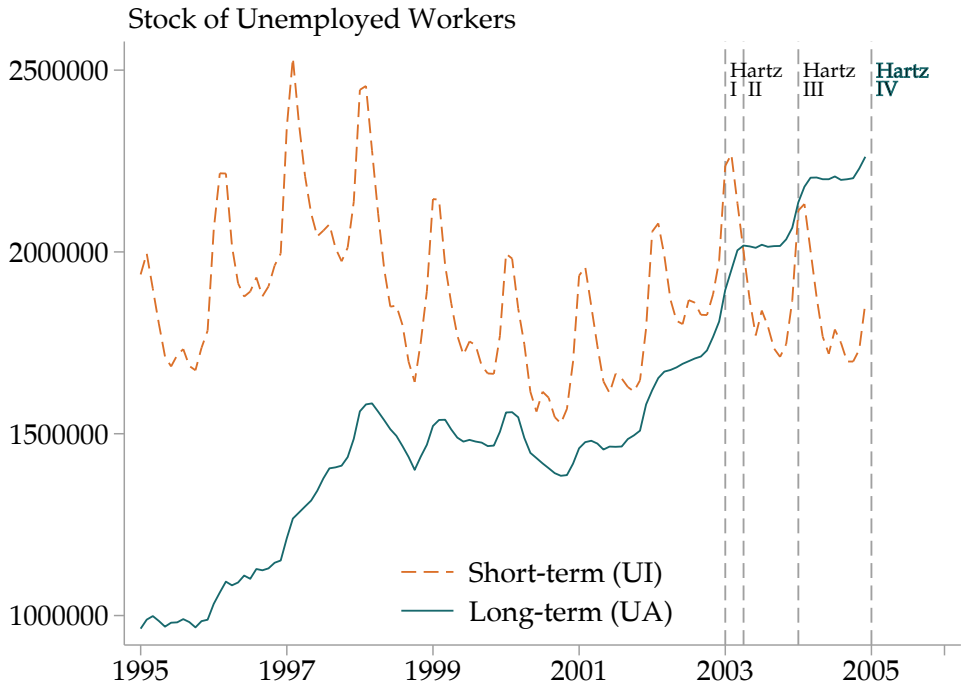
Table A.6: Characteristics of Matched Movers and Stayers in PASS-Dataset

Variable	(1) Mover	(2) Stayer	(3) Difference (1 - 2)	(4) SE Difference
A. Household Composition				
Household Size	2.53	2.60	-0.06	0.22
Number of Children	0.670	0.739	-0.069	0.158
B. Household Income				
Net Income	1870	1981	-111	172
Net Equivalence Income	1168	1213	-45	84
Net Income Other Members	429	478	-49	150
No Savings	0.293	0.250	0.043	0.066
Positive Debt	0.644	0.714	-0.070	0.070
C. Housing				
Tenant	0.849	0.878	-0.029	0.052
Rent (incl. Bills)	483	512	-29	28
Floor Space	75.5	75.4	0.1	5.4
Rent per Sqm	8.68	8.89	-0.21	0.90
D. Transfers				
Transfers	292	382	-89	63
Receives Housing Subsidy	0.022	0.054	-0.032	0.028
Receives Child Subsidy	0.250	0.293	-0.043	0.066
Receives Care Subsidy	0.110	0.187	-0.077	0.052
Number of Workers	92	92		

Notes: This table shows mean characteristics of displaced workers in the matched mover-stayer sample for displacement cohorts 2008 to 2021 in the PASS dataset. Movers refer to displaced workers, who change commuting zones after displacement. Stayers refer to displaced workers, who stay in their location after displacement. For details on the matching algorithm, see text. Income, rents and transfers correspond to monthly quantities in Euros in 2015 prices. Housing subsidy refers to *Wohngeld*, child subsidy to *Kindergeld* and care subsidy to *Pflegegeld*.

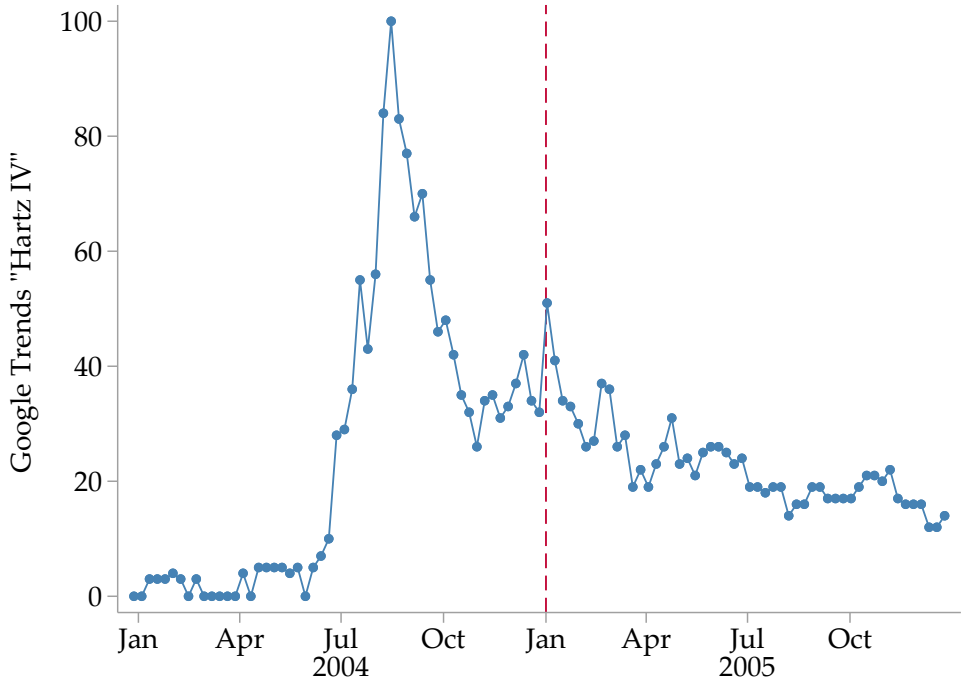
B Additional Figures

Figure B.1: Aggregate Stock of Unemployed Workers Receiving Unemployment Insurance (UI) and Unemployment Assistance (UA)



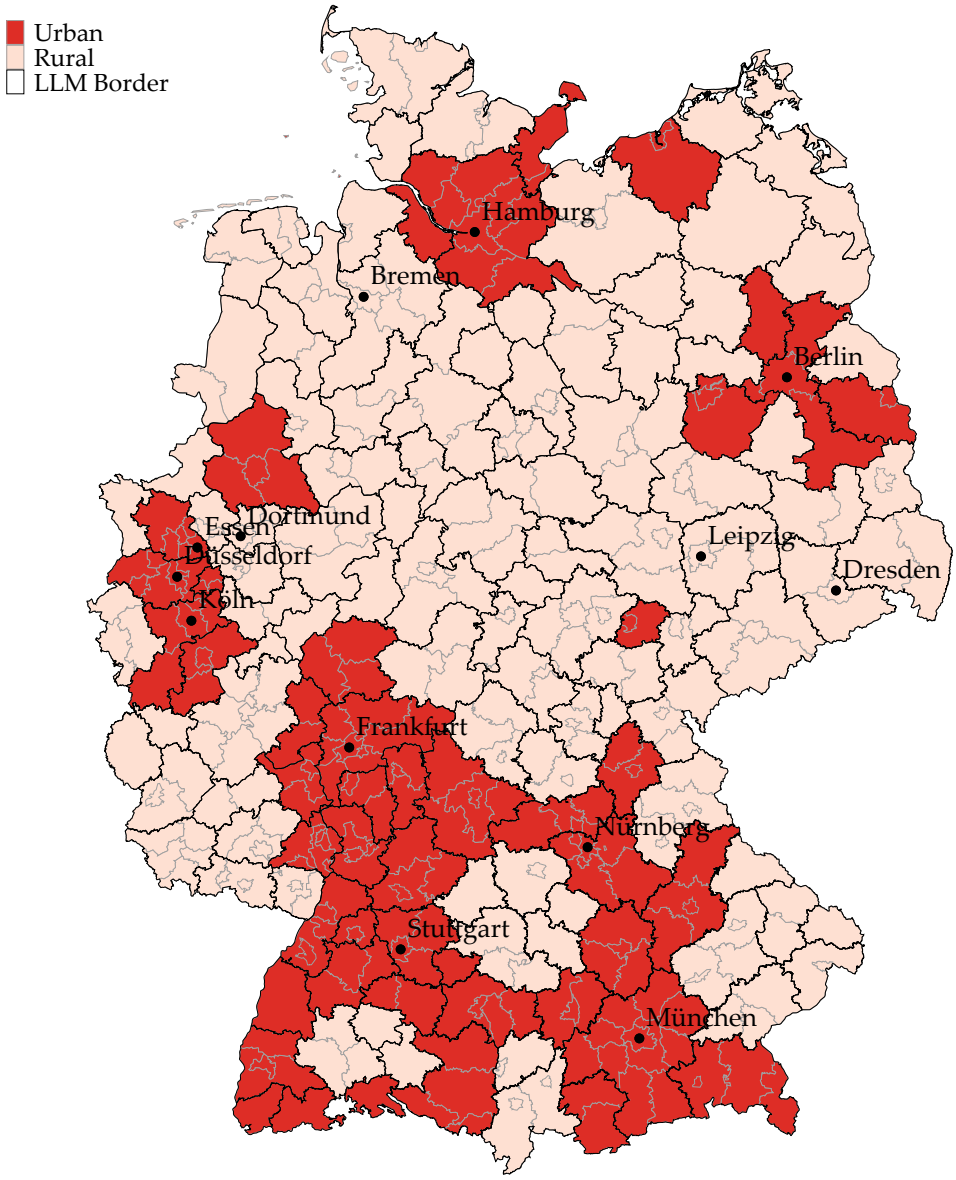
Notes: Vertical dashed lines indicate the month when each reform pillar of the Hartz reforms became effective. The data come from caseload statistics published by the German Federal Employment Agency (*Bundesagentur für Arbeit*).

Figure B.2: Google Trends in Keyword ″Hartz IV″



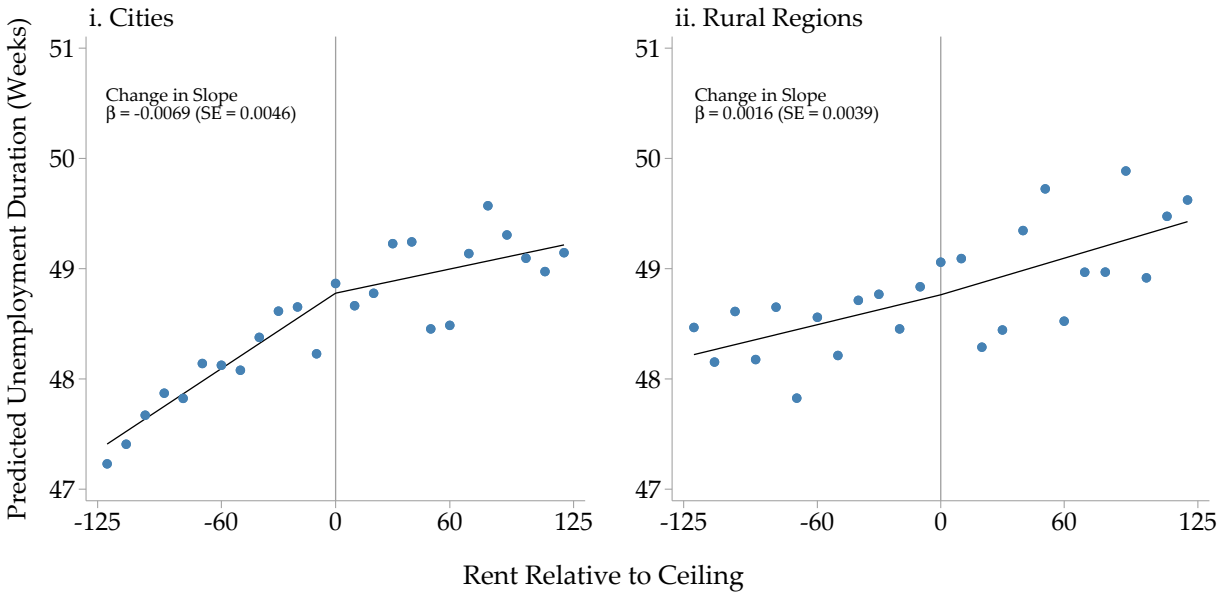
Notes: This figure shows google trends in the keyword ″Hartz IV″.

Figure B.3: City Definition



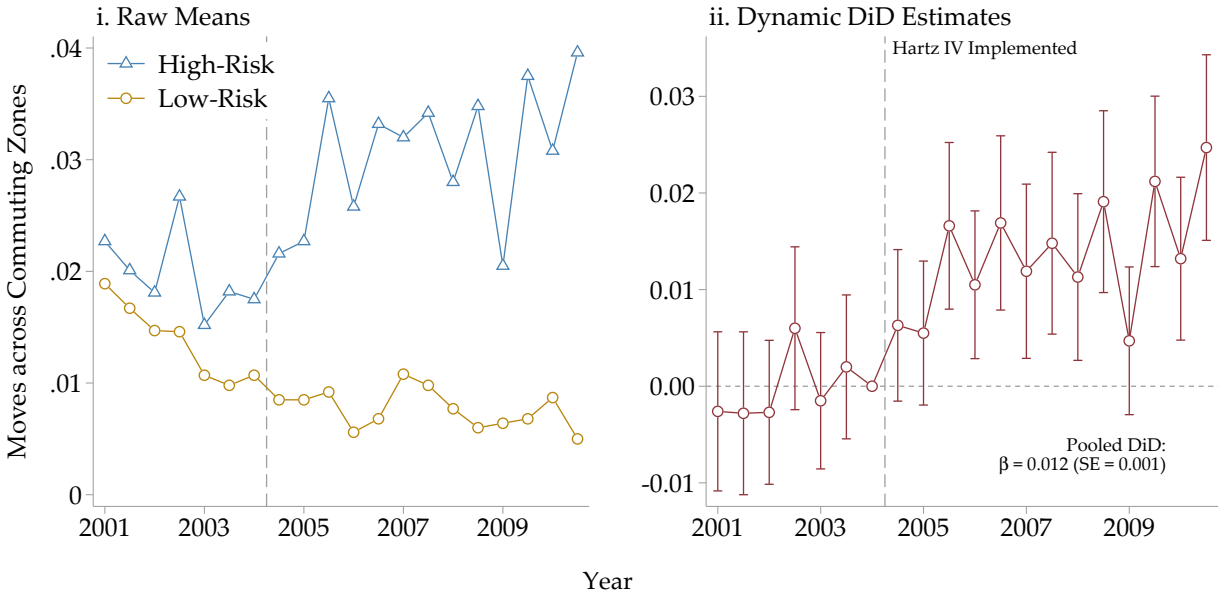
Notes: This figure shows the definition of cities and rural regions in my empirical analysis. Cities are defined as commuting zones in the top tercile of the rent distribution. Dark lines indicate commuting zone borders, according to the definition by Kosfeld and Werner (2012). Gray lines indicate county borders.

Figure B.4: Predicted Unemployment Duration by Location



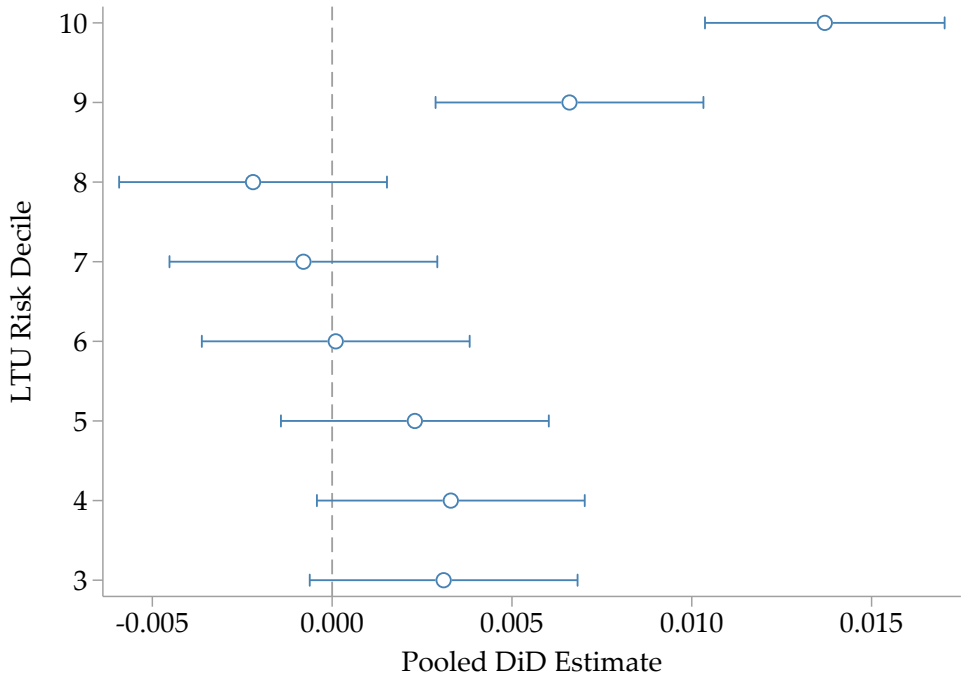
Notes: This figure shows predicted unemployment duration around the cutoff, separately between jobseekers in cities and rural regions. Predicted unemployment duration is obtained from a regression of duration on jobseekers characteristics at entry into long-term unemployment, namely age, sex, education, available income and previous wages and employment, and ceiling fixed effects. Unemployment duration is measured in weeks, and right-censored at three years. The running variable is the difference between the jobseekers rent and the local rent ceiling.

Figure B.5: Means and Dynamic DiD Estimates for Moves across Commuting Zones at Biannual Frequency



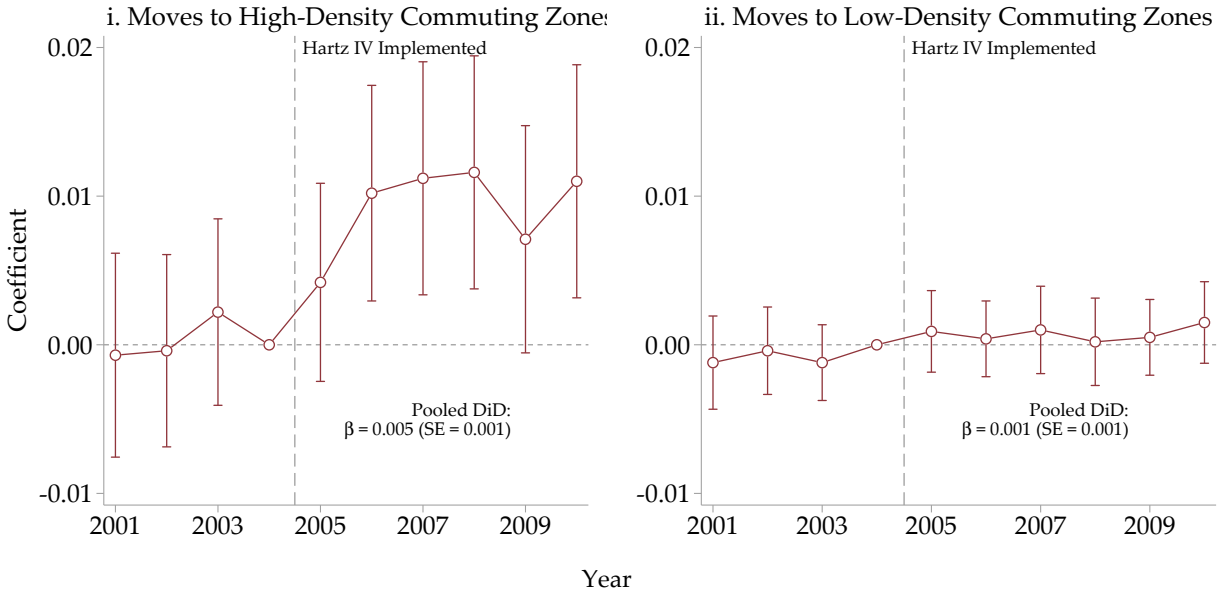
Notes: This figure shows means and dynamic difference in difference (DiD) estimates for moves across commuting zones at biannual frequency Panel i. shows raw means, and Panel ii. shows the estimated δ_k coefficients from Equation (19). The treatment group in semester t consists of unemployment spells, which are in the top sixtile ($\approx$ 16.6%) of the long-term unemployment risk distribution and terminate UI recipience in semester t. The control group consists of unemployment spells, which are at in the bottom sixtile ($\approx$ 16.6%) of the long-term unemployment risk distribution and terminate UI recipience in semester t. Capped spikes denote 95 percent confidence intervals.

Figure B.6: Static DiD Effects for Moves across Commuting Zones by Deciles of Predicted Risk



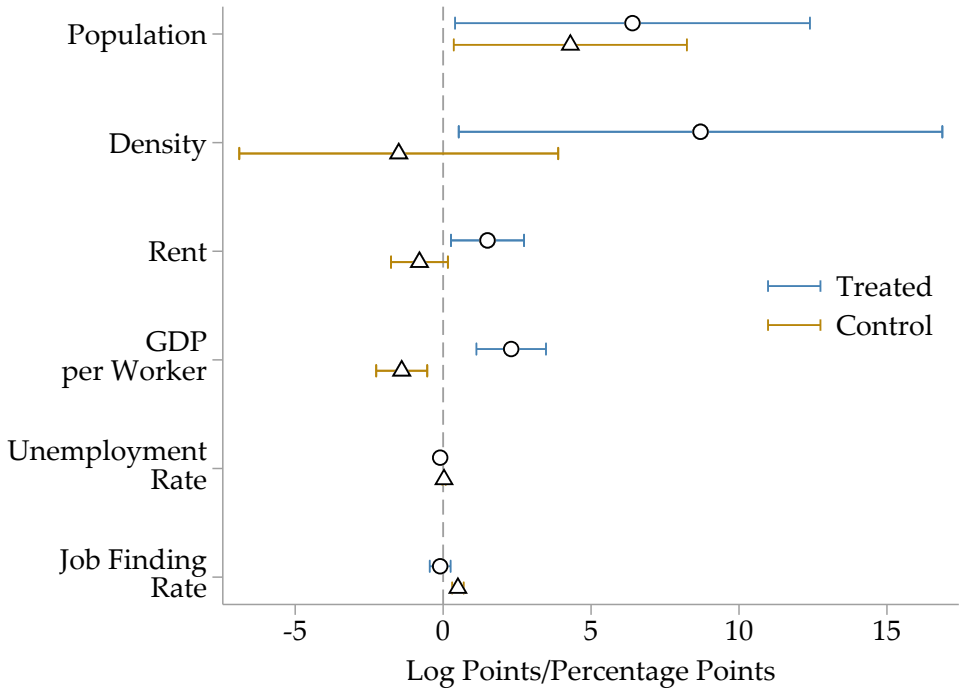
Notes: This figure shows the difference-in-differences (DiD) estimate from estimating the static analogue of Equation (19 for different treatment groups. LTU risk deciles refer to the predicted risk decile that is considered treated in the DiD regression. The control group remains constant across different treatment groups and consists of the bottom 16.6 percent of the predicted risk distribution. Capped spikes denote 95 percent confidence intervals.

Figure B.7: Dynamic DiD Estimates for Directed Moves by Commuting-Zone Population Density



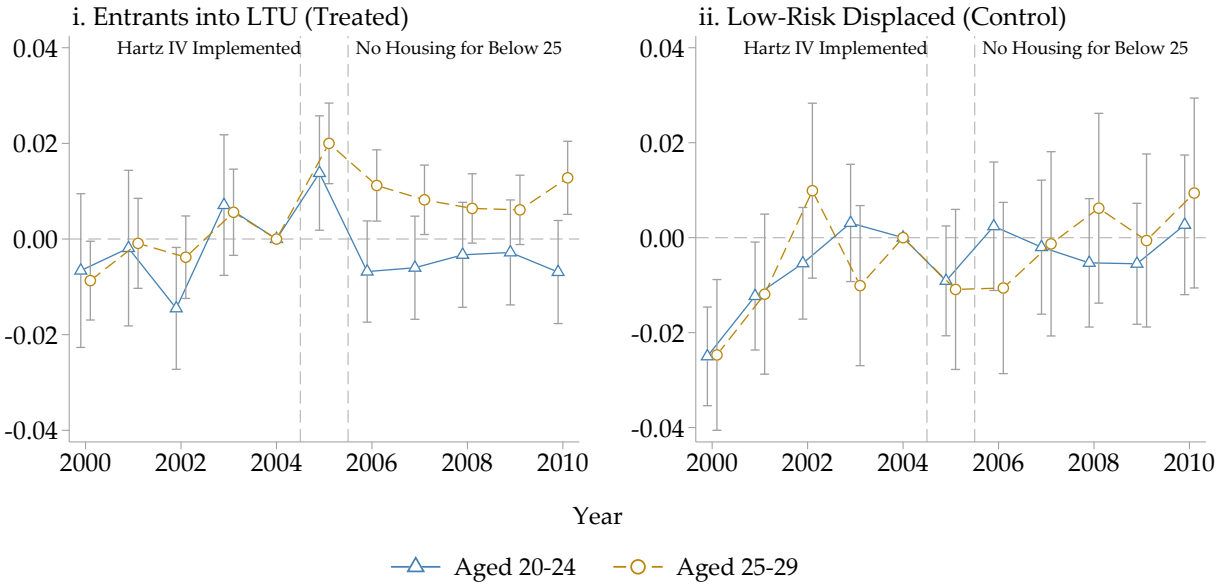
Notes: This figure shows the dynamic difference-in-difference estimates from Equation (19). Panel i. shows moves from commuting zones in the bottom tercile of population density toward commuting zones in the top tercile of population densitys. Panel ii. display estimates for moves from commuting zones in the top tercile of population densitys toward commuting zones in the bottom tercile of population densitys. Jobseekers in the treated group belong to the top 16.6% of predicted long-term unemployment risk, whereas jobseekers in the control group belong to the bottom 16.6%. Capped spikes denote 95 percent confidence intervals.

Figure B.8: Difference between Destination and Origin Characteristics among Movers after the Reform



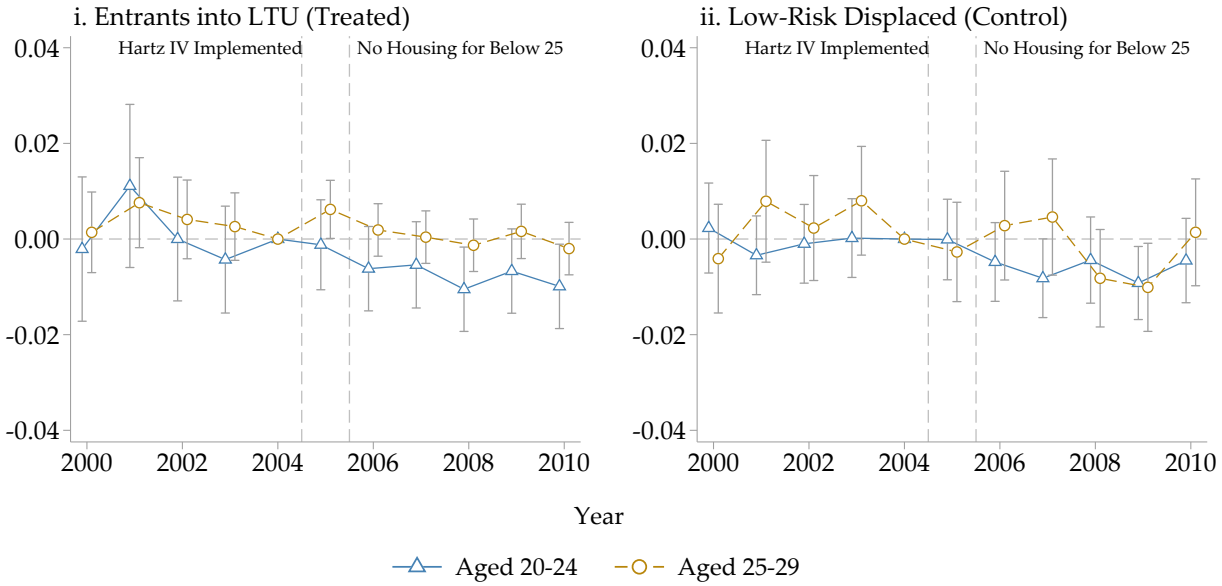
Notes: This figure shows the difference in the average characteristic between destination and origin for jobseekers that moved after the reform. Jobseekers in the treated group belong to the top 16.6% of predicted long-term unemployment risk, whereas jobseekers in the control group belong to the bottom 16.6%. Capped spikes denote 95 percent confidence intervals.

Figure B.9: Estimated Profiles for Moves towards High-Rent Regions by Treatment and Age Groups



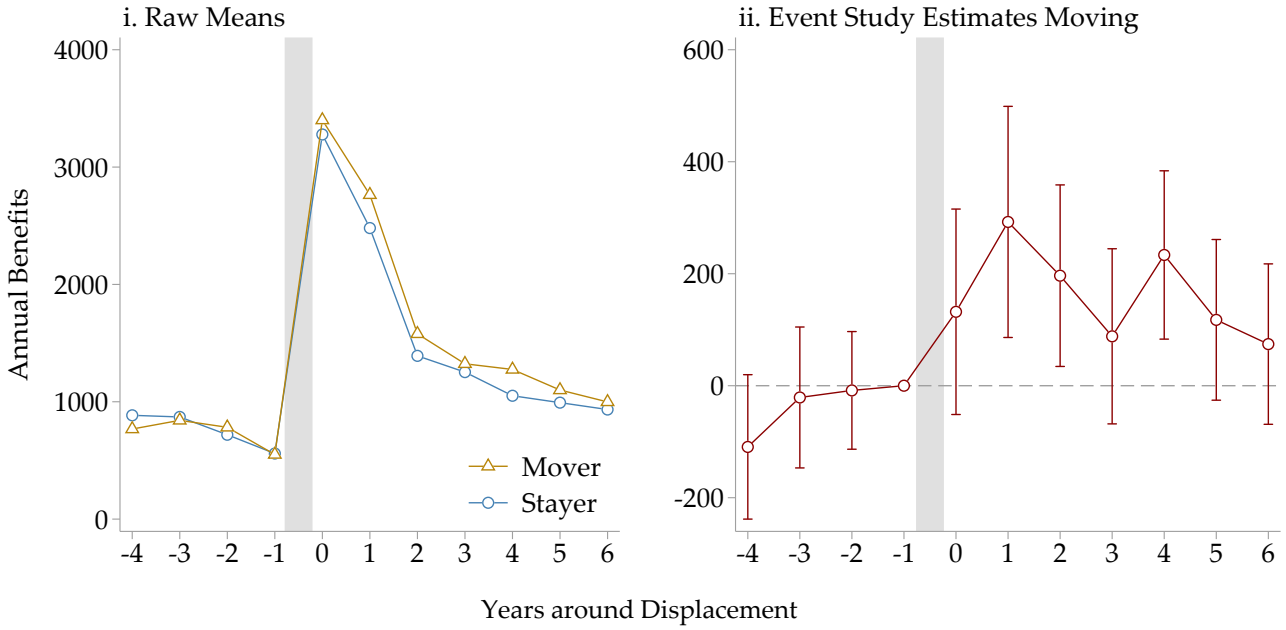
Notes: This figure shows the implied profiles for moves towards destinations in the top tercile of the rent distribution from estimating Equation (26). Panel i. shows the estimated profile for UI claimants that enter long-term unemployment. Panel ii. depicts the corresponding profile for UI claimants with low predicted risk of entering long-term unemployment (bottom 50% of predicted risk score). Capped spikes denote 95 percent confidence intervals.

Figure B.10: Estimated Profiles for Moves towards Low-Rent Regions by Treatment and Age Groups



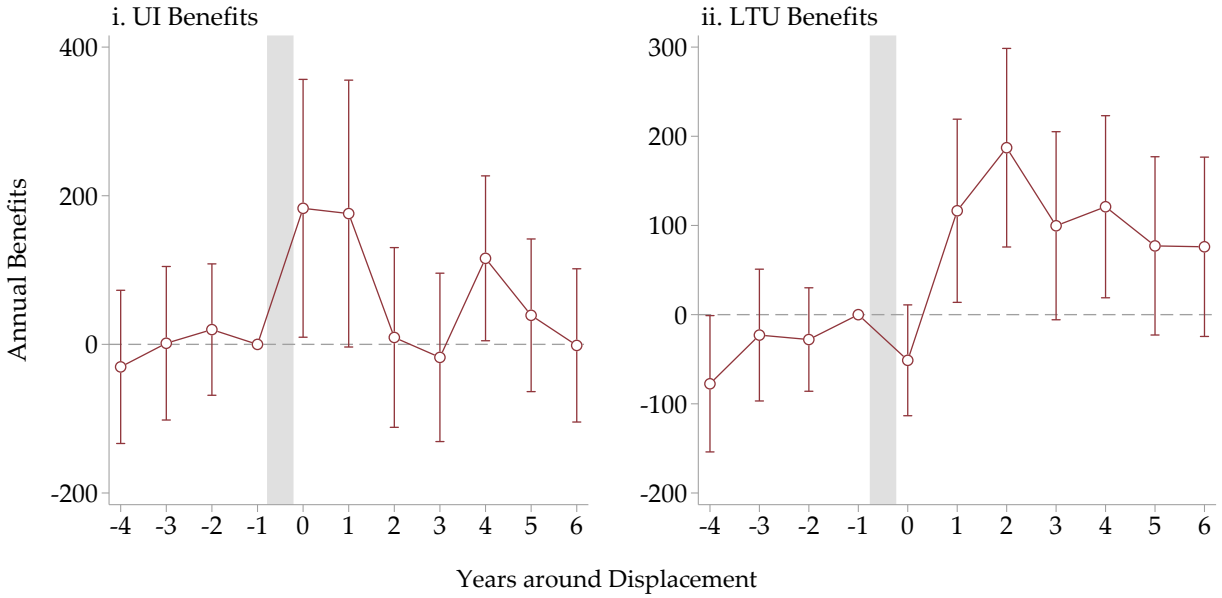
Notes: This figure shows the implied profiles for moves towards destinations in the bottom tercile of the rent distribution from estimating Equation (26). Panel i. shows the estimated profile for UI claimants that enter long-term unemployment. Panel ii. depicts the corresponding profile for UI claimants with low predicted risk of entering long-term unemployment (bottom 50% of predicted risk score). Capped spikes denote 95 percent confidence intervals.

Figure B.11: Means and Matched Event Study Estimates for Annual Unemployment Benefits



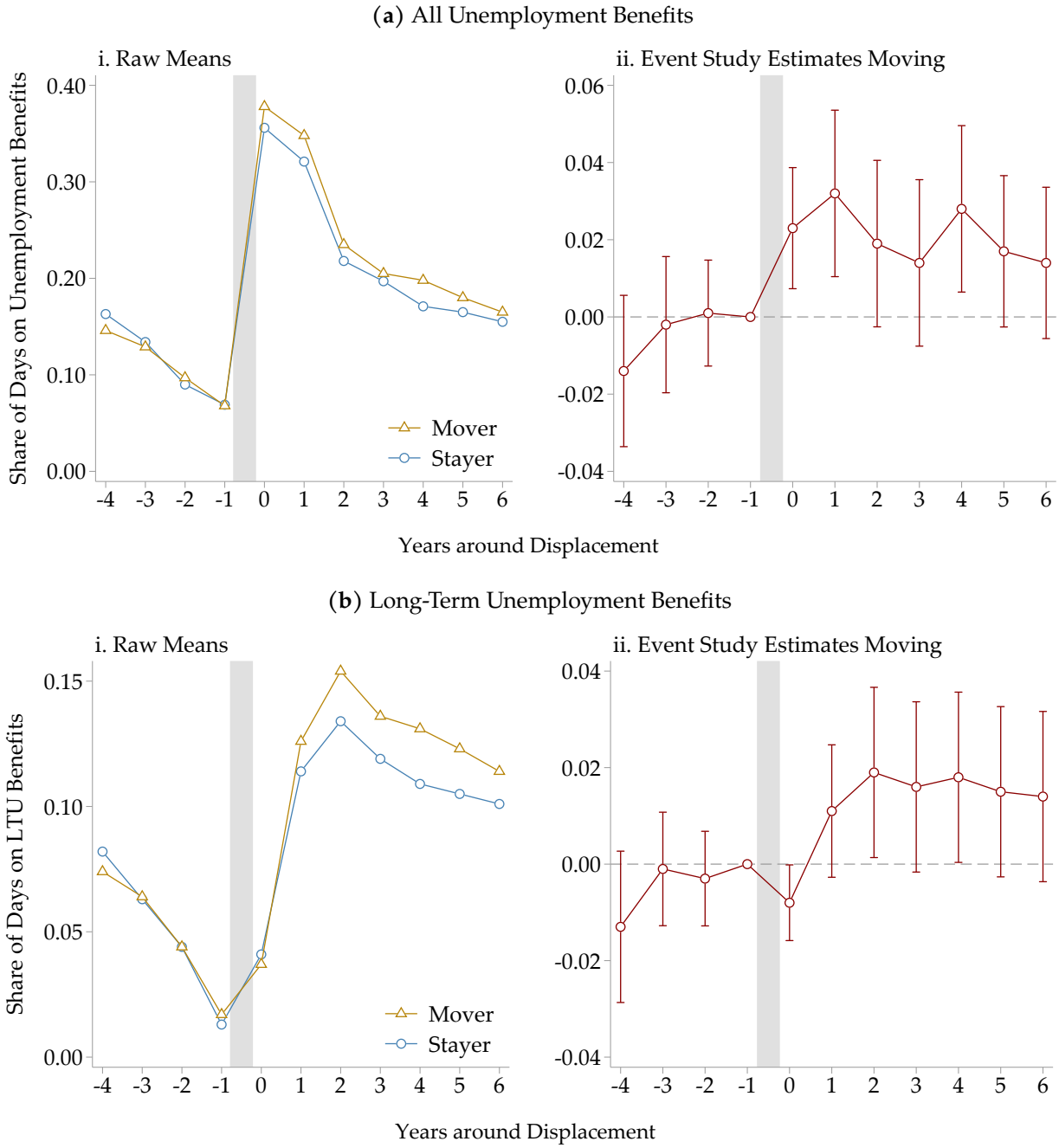
Notes: This figure shows raw means and matched event study estimates from Equation 28 for annual unemployment benefits received. Benefits are computed by multiplying the number of days on unemployment benefits (UI and LTU), multiplied with the daily benefit rate. The treated group consists of movers, who relocate to a commuting zone in the top tercile of rents during unemployment insurance. The control group consists of matched stayers, who remain in a rural area after displacement. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals.

Figure B.12: Matched Event Study Estimates for Annual UI and LTU Benefits



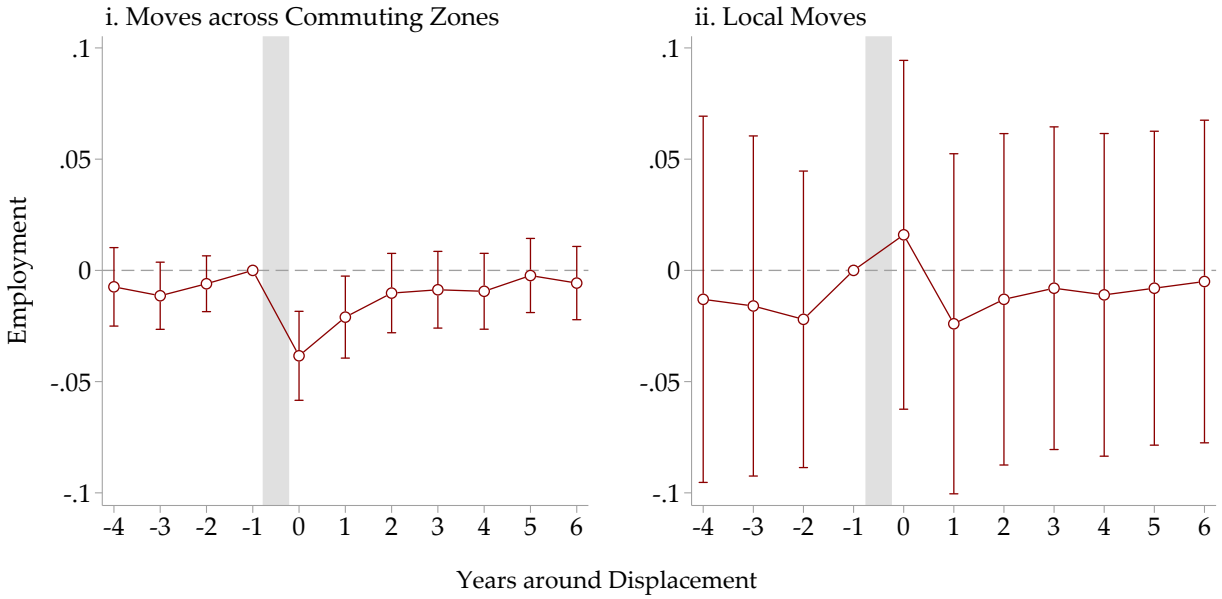
Notes: This figure shows matched event study estimates from Equation 28 for annual unemployment benefits received, separately for UI benefits and LTU benefits. Benefits are computed by multiplying the number of days on unemployment benefits (UI and LTU), multiplied with the daily benefit rate. The treated group consists of movers, who relocate to a commuting zone in the top tercile of rents during unemployment insurance. The control group consists of matched stayers, who remain in a rural area after displacement. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals.

Figure B.13: Means and Matched Event Study Estimates for Share of Days on Unemployment Benefits



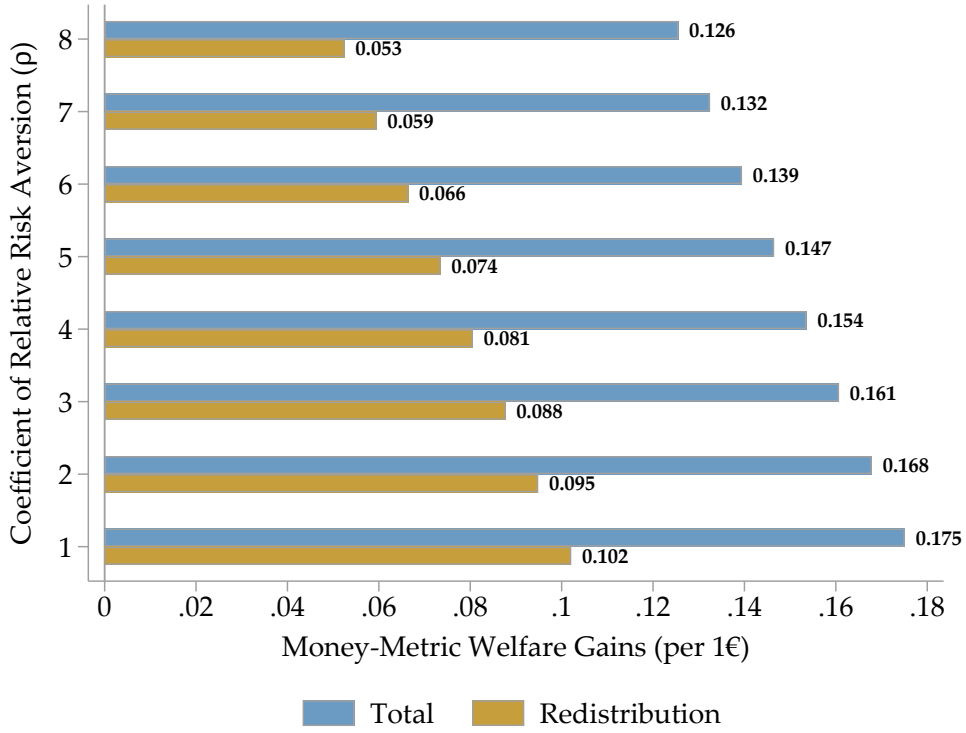
Notes: This figure shows raw means and matched event study estimates from Equation 28 for the share of days on unemployment benefits. Panel (a) considers all unemployment benefits (UI and LTU), whereas panel (b) only considers days with LTU recipience. The share of days on unemployment benefits is computed by summing over all days with benefit recipience (UI and LTU), divided by all days recorded with social security (employment, unemployment, job search). The treated group consists of movers, who relocate to a commuting zone in the top tercile of rents during unemployment insurance. The control group consists of matched stayers, who remain in a rural area after displacement. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals.

Figure B.14: Matched Event Study Estimates for Employment by Proximity of Move



Notes: This figure shows matched event study estimates from Equation (28) for employment, by proximity of the move. Employment is defined as having a job at the 30th of June in a given year. Panel i. shows the estimates for the treatment group consisting of displaced workers, who move across commuting zones during UI. Panel ii. shows the corresponding estimates for jobseekers, who move within the same county after displacement. Panel i. uses all displaced spells in the administrative data (SIAB), whereas Panel ii. uses local moves as identified in the PASS-ADIAB matched survey data. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals.

Figure B.15: Welfare Impact of Redistributing Benefits towards Rural Regions by Degree of Risk Aversion



Notes: This figure shows the money-metric welfare impact of redistributing benefits from urban to rural regions, per €1 transferred to the unemployed in rural regions. The blue bars show the total welfare impact, whereas the yellow bars show the welfare effect of redistribution. Welfare gains are measured relative to the marginal utility of the unemployed in urban locations.

C Model Appendix

C.1 Proposition 1

Proof of Proposition 1. A reallocation of benefits between two locations c and r is budget-neutral if and only if

$$N_c(\boldsymbol{b})\left((1-s_c)b_c\cdot(1+\epsilon_{1-s_c,b_c})+\frac{(1-s_c)b_c-\overline{B}(\boldsymbol{b})}{1-N_c}\cdot\epsilon_{N_c,b_c}\right)d\ln b_c \tag{C.1}$$

$$=-N_r(\boldsymbol{b})\left((1-s_r)b_r\cdot(1+\epsilon_{1-s_r,b_r})+\frac{(1-s_r)b_r-\overline{B}(\boldsymbol{b})}{1-N_r}\cdot\epsilon_{N_r,b_r}\right)d\ln b_r \tag{C.2}$$

$$\Leftrightarrow\quad d\ln b_c=-\frac{N_r(\boldsymbol{b})}{N_c(\boldsymbol{b})}\cdot\frac{\left((1-s_r)b_r\cdot(1+\epsilon_{1-s_r,b_r})+\frac{(1-s_r)b_r-\overline{B}(\boldsymbol{b})}{1-N_r}\cdot\epsilon_{N_r,b_r}\right)}{\left((1-s_c)b_c\cdot(1+\epsilon_{1-s_c,b_c})+\frac{(1-s_c)b_c-\overline{B}(\boldsymbol{b})}{1-N_c}\cdot\epsilon_{N_c,b_c}\right)}\cdot d\ln b_r \tag{C.3}$$

A reform is welfare improving if and only if:

$$N_c(\boldsymbol{b})\frac{\partial v_c}{\partial\ln b_c}d\ln b_c+N_r(\boldsymbol{b})\frac{\partial v_r}{\partial\ln b_r}d\ln b_r>0 \tag{C.4}$$

Hence, a balanced-budget reallocation $\partial\ln b_c>0>\partial\ln b_r$ increases jobseekers' welfare if and only if:

$$\frac{\frac{\partial v_c}{\partial\ln b_c}}{\frac{\partial v_r}{\partial\ln b_r}}>\frac{(1-s_c)b_c\cdot(1+\epsilon_{1-s_c,b_c})+\frac{(1-s_c)b_c-\overline{B}(\boldsymbol{b})}{1-N_c}\cdot\epsilon_{N_c,b_c}}{(1-s_r)b_r\cdot(1+\epsilon_{1-s_r,b_r})+\frac{(1-s_r)b_r-\overline{B}(\boldsymbol{b})}{1-N_r}\cdot\epsilon_{N_r,b_r}} \tag{C.5}$$

Recall that migration mirrors jobseekers' welfare changes across locations:

$$\epsilon_{N_j,b_j}=\frac{1-N_j(\boldsymbol{b})}{\sigma}\cdot\frac{\partial v_c}{\partial\ln b_j}\quad\text{for all } j \tag{C.6}$$

Plugging (C.6) into (C.5), dividing by $\partial v_c/\partial\ln b_c$, multiplying by $\partial v_r/\partial\ln b_r$ and rearranging yields:

$$1>\frac{(1-s_c)b_c\cdot\frac{1+\epsilon_{1-s_c,b_c}}{\partial v_c/\partial\ln b_c}+\sigma[(1-s_c)b_c-\overline{B}(\boldsymbol{b})]}{(1-s_r)b_r\cdot\frac{1+\epsilon_{1-s_r,b_r}}{\partial v_r/\partial\ln b_r}+\sigma[(1-s_r)b_r-\overline{B}(\boldsymbol{b})]} \tag{C.7}$$

$$(1-s_r)b_r\cdot\frac{1+\epsilon_{1-s_r,b_r}}{\partial v_r/\partial\ln b_r}+\sigma[(1-s_r)b_r-\overline{B}(\boldsymbol{b})]>(1-s_c)b_c\cdot\frac{1+\epsilon_{1-s_c,b_c}}{\partial v_c/\partial\ln b_c}+\sigma[(1-s_c)b_c-\overline{B}(\boldsymbol{b})] \tag{C.8}$$

$$\frac{1+\epsilon_{1-s_r,b_r}}{U'\left(\frac{b_r}{p_r}\right)/p_r}+\sigma\cdot(1-s_r)b_r>\frac{1+\epsilon_{1-s_c,b_c}}{U'\left(\frac{b_c}{p_c}\right)/p_c}+\sigma\cdot(1-s_c)b_c \tag{C.9}$$

where the last line follows from

$$\frac{\partial v_j}{\partial\ln b_j}=(1-s_j)b_j\cdot\frac{U'\left(\frac{b_j}{p_j}\right)}{p_j} \tag{C.10}$$

□

C.2 Corollary 1

Proof of Corollary 1. I start from the expression in Proposition 1:

$$\frac{1+\epsilon_{1-s_r,b_r}}{U'\left(\frac{b_r}{p_r}\right)/p_r}+\sigma(1-s_r)b_r > \frac{1+\epsilon_{1-s_c,b_c}}{U'\left(\frac{b_c}{p_c}\right)/p_c}+\sigma(1-s_c)b_c \tag{C.11}$$

$$\frac{U'\left(\frac{b_c}{p_c}\right)/p_c}{U'\left(\frac{b_r}{p_r}\right)/p_r} > \frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}+\frac{U'\left(\frac{b_c}{p_c}\right)/p_c}{1+\epsilon_{1-s_r,b_r}}\cdot\sigma[(1-s_c)b_c-(1-s_r)b_r] \tag{C.12}$$

Recall the definition of the migration elasticity in the city:

$$\epsilon_{N_c,b_c}=\sigma\cdot(1-N_c(\boldsymbol{b}))\cdot(1-s_c)b_c\cdot\frac{U'\left(\frac{b_c}{p_c}\right)}{p_c} \tag{C.13}$$

$$\Leftrightarrow \quad \frac{U'\left(\frac{b_c}{p_c}\right)}{p_c}\cdot\sigma=\frac{\epsilon_{N_c,b_c}}{(1-N_c(\boldsymbol{b}))\cdot(1-s_c)b_c} \tag{C.14}$$

Rewriting Equation (C.12) yields:

$$\frac{U'\left(\frac{b_c}{p_c}\right)/p_c}{U'\left(\frac{b_r}{p_r}\right)/p_r} > \frac{1+\epsilon_{1-s_c,b_c}}{1+\epsilon_{1-s_r,b_r}}+\frac{\epsilon_{N_c,b_c}}{(1+\epsilon_{1-s_r,b_r})(1-N_c(\boldsymbol{b}))}\cdot\frac{(1-s_c)b_c-(1-s_r)b_r}{(1-s_c)b_c} \tag{C.15}$$

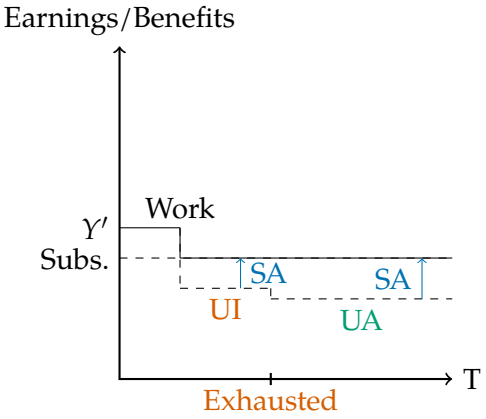
Using $N_r(\boldsymbol{b})=(1-N_c(\boldsymbol{b}))$ yields the expression in Corollary 1. □

D Additional Institutional Details

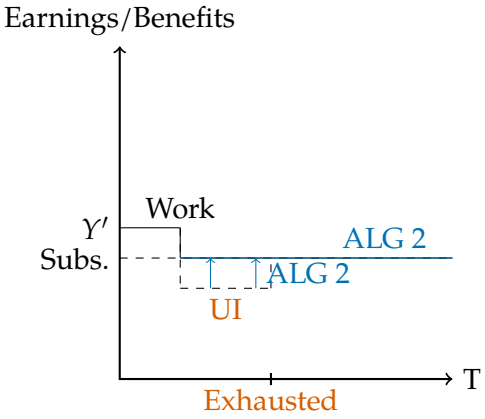
D.1 Changes in Benefits

Figure D.1: Changes in Benefits for Workers with Benefits $<$ Subsistence

(**a**) Benefits before the Reform



(**b**) Benefits after the Reform



E Simulating Welfare Benefits

E.1 Additional Simulation Figures

Figure E.1: Simulated Real Income Losses between Average Location and other Locations before the Reform

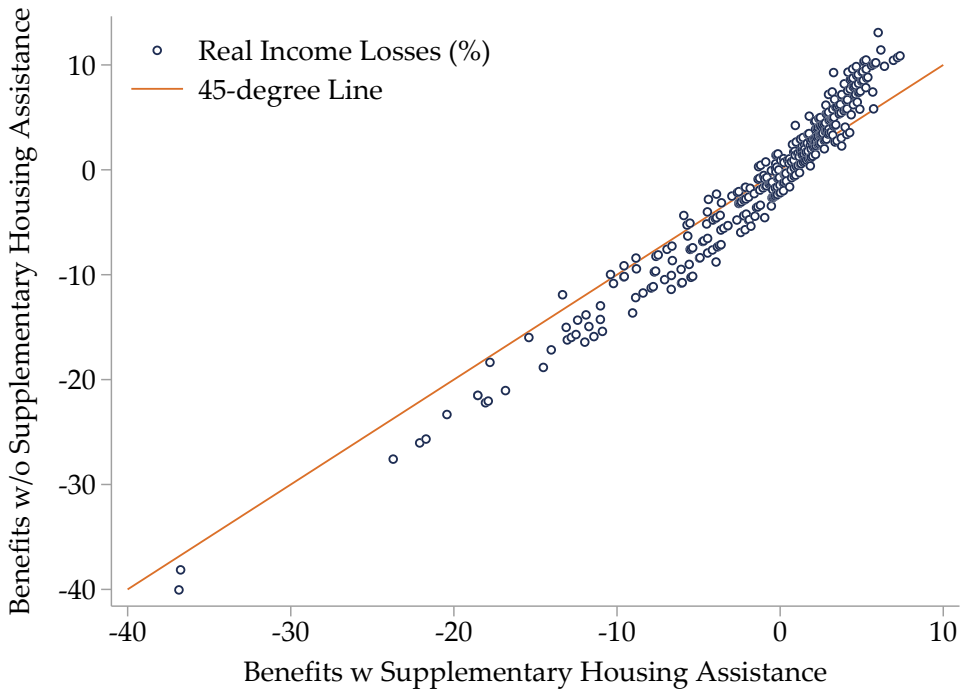
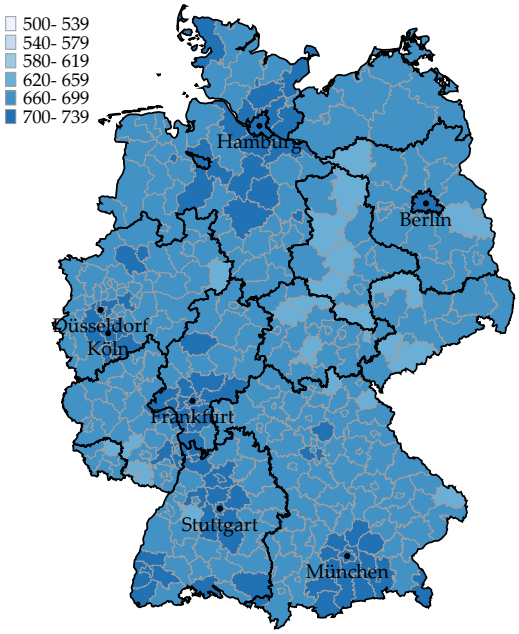
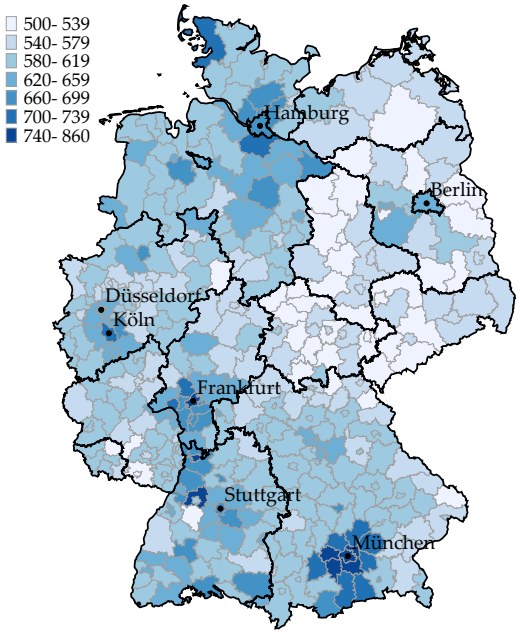


Figure E.2: Simulated Nominal Long-Term Benefits before and after the Reform

i. Pre Reform



ii. Post Reform



E.2 Wohngeld

The formula to calculate Wohngeld for household h in year t living in a municipality of rent level r (*Mietenstufe*) is:

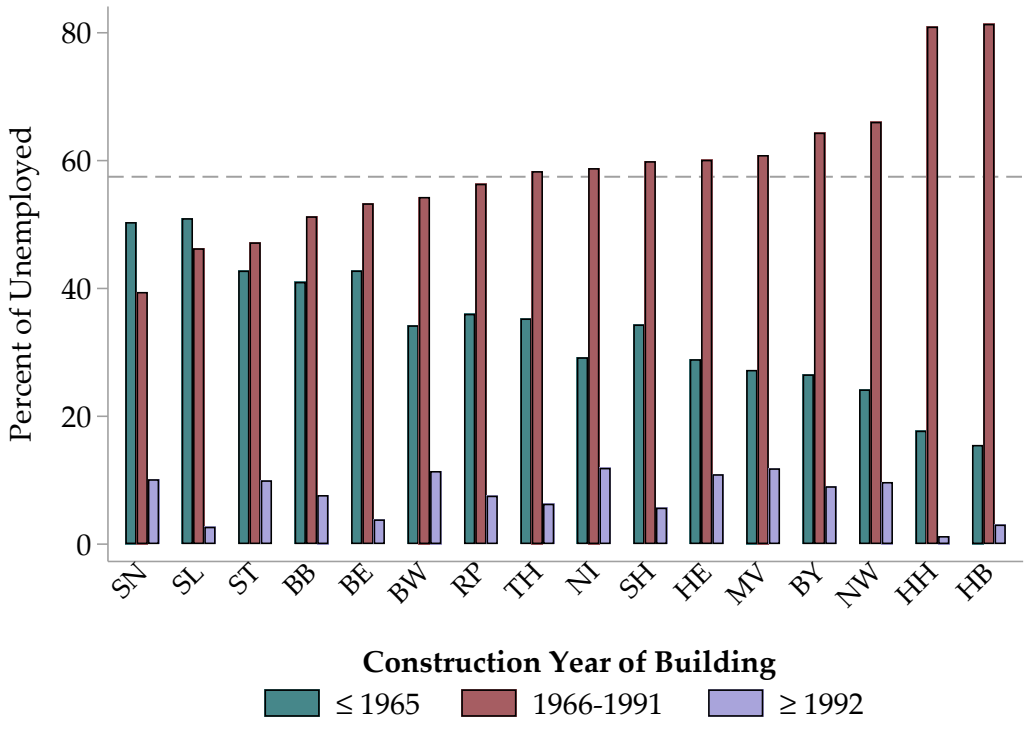
$$\text{Wohngeld}_{ht} = f_t \times [M_{ht} - (a_{ht} + b_{ht} \cdot M_{ht} + c_{ht} \cdot Y_{ht}) \cdot Y_{ht}] \tag{E.1}$$

where M_{ht} is the eligible monthly rent. The eligible rent corresponds to the actual rent of the household, subject to being below a threshold $\overline{M}_{r(ht)}$, which depends on the rent level of the municipality of the claimant and household size. Until 2009, it also depended on the building age of the housing unit.[38] If the actual rent falls below a year-specific threshold, it is replaced by that floor. Formally:

$$M_{ht} = \begin{cases} \underline{M}_t & \text{if Rent}_{ht} < \underline{M}_t \\ \text{Rent}_{ht} & \text{if } \underline{M}_t < \text{Rent}_{ht} \leq \overline{M}_{r(ht)} \\ \overline{M}_{r(ht)} & \text{otherwise} \end{cases}$$

Since I do not observe the age of the building that my sample of displaced workers reside in, I assume that their building age lies in $a \in [1966, 1991]$, which corresponds to the modal category among displaced workers during the years 2000 to 2008 in the GSOEP, both for entire Germany as well as for most states, as displaced in Figure E.3.

Figure E.3: Distribution of Building Construction Year Cohorts across German States



[38] In 2002, building age a was summarized in three categories: $a \leq 1965, a \in [1966, 1991], a \geq 1992$.

The ceilings $\overline{M}_{r(ht)}$ are updated and I assign households to their ceiling as a function of household size, year and rent level (see below).

Y_{ht} corresponds to net monthly household income, subject to being above a household size and year-specific floor $\underline{Y}_{ht}$. If Y_{ht} falls below that threshold, it is replaced by $\underline{Y}_{ht}$. a, b and c are parameters which vary depending on household size and year. f_t is a factor that was introduced in 2009. I obtain the parameter values and income thresholds from GETTSIM (2022).[39]

Rent levels during the time period of my sample vary from 1 (low) to 6 (high). Municipalities are assigned to rent levels depending on the deviation of their rent per sqm price from the national average for observationally similar units (see Table E.1). I obtain the rent level of all municipalities from the corresponding reports from the Federal Ministery of Transport, Building and Housing (Bundesministerium für Verkehr, Bau- und Wohnungswesen, 2002). As I only observe the residency county (and not municipality), I assume that workers live in the municipality with the highest rent level in their corresponding county.

Table E.1: Assignment of Municipalities to Rent Levels

Rent Level (*Mietenstufe*)	Rent Relative to National Average
I	$< 85\%$
II	$[85\%, 95\%)$
III	$[95\%, 105\%)$
IV	$[105\%, 115\%)$
V	$[115\%, 95\%)$
VI	$\geq 125\%$

[39] The law text can be accessed from the Bundesgesetzblatt (2002) (accessed 05.04.2025).

F Predicting Long-Term Unemployment Risk

I closely follow the selection of predictors in Mueller and Spinnewijn (2023) to predict long-term unemployment risk. I deviate from their approach in three dimensions. First, I use an indicator for whether a spell claims long-term unemployment benefits after UI exhaustion as the outcome to predict, instead of an indicator for not having found a job within six months as in Mueller and Spinnewijn (2023). This is motivated by differences in potential unemployment benefit duration across spells, which implies that some spells are still far from long-term unemployment benefit recipience even after six months of UI recipience (the maximum eligibility corresponds to 31 months). Second, I have so far only used a linear probability or logit model, instead of using modern machine learning techniques for the prediction. I will leave the latter for the near future of this project. Third, I do not use any geographic information (apart from whether the claimant resides in East or West Germany at UI entry) to avoid that my risk measure is largely driven by the location of the claimant, which could potentially contaminate my results on mobility.

To predict long-term unemployment risk, I use a random 90% sample of unemployment spells that entered unemployment insurance between 1999 and 2003 (both included) to run the following linear probability model (or logistic regression):

$$\begin{aligned}\text{Spell Enters LTUE}_{it} = \beta_0 &+ \text{Worker Characteristics}_{it}'\gamma \\ &+ \text{Previous Job Characteristics}_{it}'\delta \\ &+ \text{Previous Working History}_{it}'\eta + \epsilon_{it} \quad \text{(F.1)}\end{aligned}$$

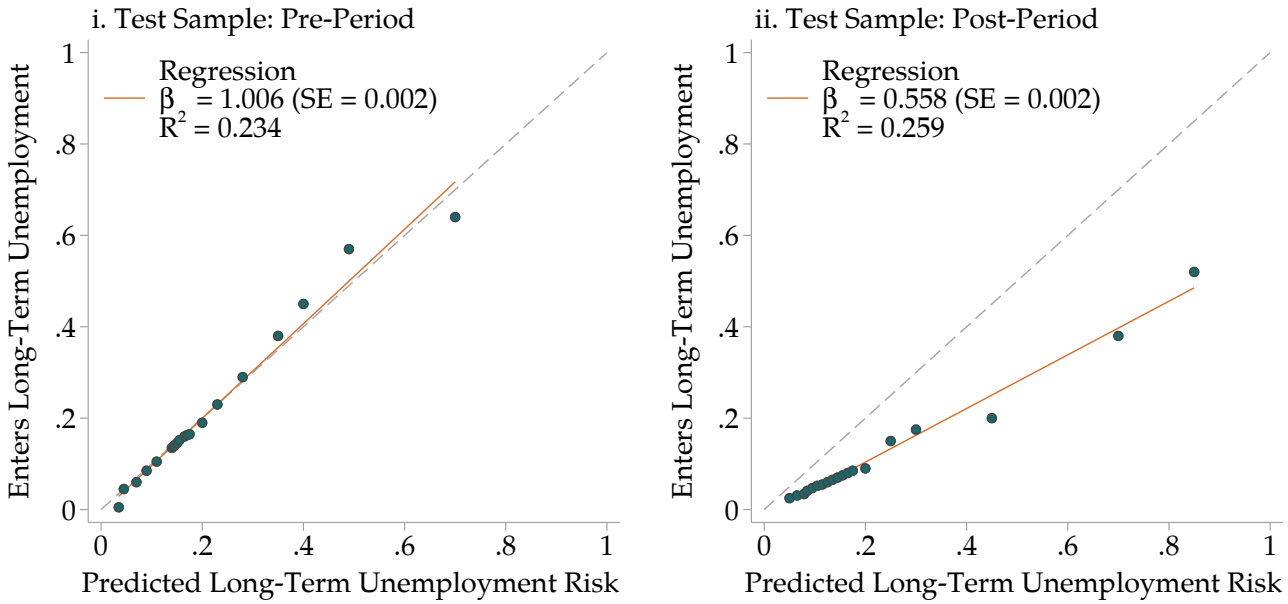
In practice, I step-wise add worker characteristics, previous job information and finally characteristics of the previous working history. Similar to Mueller and Spinnewijn (2023), information on the previous working history comes with the largest predictive gains, compared to the other set of predictors. Table F.1 shows the full set of predictors that I use.

Table F.1: Set of Predictors Used for Predicting LTUE Risk

Worker Characteristics	**Previous Job Characteristics**	**Employment History (last 5 years)**
Age	Wage	Number of unemployment pells
Education	Full-time/part-time	Number of jobs
Migrant	Main job/mini job	Days on UI
Marital status	Tenure	Days on LTUE
Number of kids	3-digit industry code	Potential benefit duration
Sex	Firm size	
Lives in East Germany		

Based on my results using the training sample, I predict long-term unemployment risk for the hold-out test sample from the pre-reform years, and for all spells starting in 2004 or later. Figure F.1 shows the predictive performance for the pre-reform 10% test sample, and the post-reform test sample, using the logit specification.

Figure F.1: Binned Scatter Plots of Predictive Performance



As can be seen from the panels in Figure F.1, my most saturated specification obtains a reasonable fit for the test sample in the pre-period, explaining similar amounts of variation in long-term unemployment as in Mueller and Spinnewijn (2023). However, while I explain a similar share of variation as indicated by the R^2-value, the slope is significantly below one. This suggests that my prediction yields higher long-term unemployment risk than can be de facto observed in the data during the post period. Yet, this is likely (also) a consequence of the reform, which on average lowered incentives to enter long-term unemployment.

G Unemployment Duration Responses: Additional Material

G.1 Constructing Local Rent Ceilings

I obtain information on rent ceilings from Jobcenter guidelines, information material for jobseekers and local government documents. The administrative data contain information on total housing costs per month, the residency county and household size. Jobcenters occasionally apply different ceilings within county×household size×year cells, for instance depending on heating type, or the residency municipality instead of county. In this section, I discuss how I map these more specific ceilings to jobseekers in the administrative data.

First, there is variation across Jobcenters and over time in the type of rent, to which the ceiling directly applies: (i) rent including utilities and heating, (ii) rent including utilities but without heating, and (iii) rent excluding all bills. Figure G.1 depicts two examples for ceiling types (i) and (ii). Panel G.1a shows Berlin's ceiling from 2005, which was directly applied to rent including all bills (*Bruttowarmmiete*). In contrast, the ceiling of Duisburg in G.1b applies to rents including utilities, but excluding heating costs (*Bruttokaltmiete*).

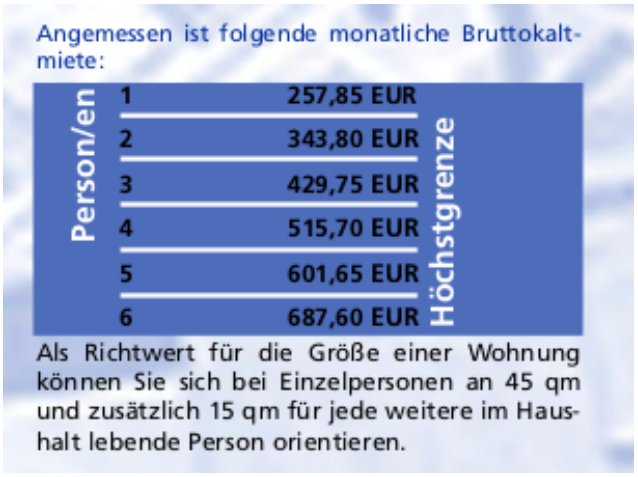
Figure G.1: Examples of Reported Ceilings

(**a**) Ceiling in Berlin as of 2005

Tabelle 1: Richtwerte für angemessene Bruttowarmmieten in Berlin gemäß AV-Wohnen und Berechung Nettokaltmiete/qm

Haushaltsgröße	Bruttowarmmiete	durchschnittliche Wohnungsgröße	durchschnittliche Nettokaltmiete
Eine Person	360 Euro	50 qm	5,00 Euro/qm
Zwei Personen	444 Euro	60 qm	5,20 Euro/qm
Drei Personen	542 Euro	75 qm	5,00 Euro/qm
Vier Personen	619 Euro	85 qm	5,10 Euro/qm
Fünf Personen	705 Euro	100 qm	4,80 Euro/qm

(**b**) Ceiling in Duisburg as of 2009

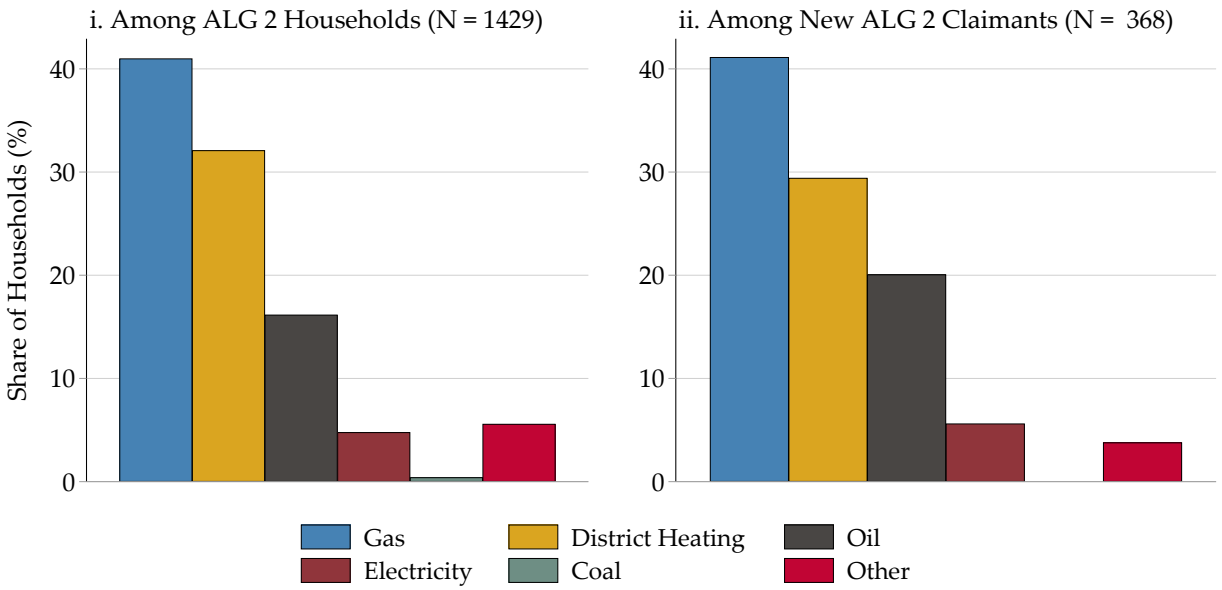


Notes: This figure shows two examples of local rent ceilings from the original documents. Panel (a) shows the ceiling in Berlin starting in 2005, whereas Panel (b) shows the ceiling in Duisburg from 2009 onwards. The ceiling in Berlin applies to rent including all bills (*Bruttowarmmiete*), whereas Duisburg applies the ceiling to rent excluding heating (*Bruttokaltmiete*) and tests separately for the appropriateness of heating costs.

As I only observe total housing costs per month including all bills in the administrative data, I need to map ceilings excluding bills (type (ii) and (iii)) to the observed housing costs in the administrative data. If utilities and heating are not directly included in the ceiling ((ii) and (iii)), Jobcenters usually test separately for the appropriateness of utilities and/or heating. To determine appropriateness, most Jobcenters rely on tables that define unusually high consumption and expenditure levels for heating (*Bundesheizkostenspiegel*) and utilities (*Nebenkostenspiegel*).[40] The former specifies annual costs of heating (per square meter) depending on consumption, heating type and building size. To construct a local heating limit, which I can then add to the ceiling on rents in order to obtain the directly comparable ceiling, I assume that Jobseekers use gas as their heating type—the model heating type among long-term unemployed Jobseekers according tabulations in the German Socio-Economic Panel (SOEP,

[40] Other Jobcenters determine their local heating and utilities ceilings based on expenditure data of local claimants. I also digitize the local values on bills and use them directly if local authorities provide them in their internal guidelines.

Figure G.2: Heating Type for Different Samples of ALG 2 Claimants



Notes: This figure shows the distribution of heating types among long-term unemployment benefit claimants (ALG 2) after the Hartz IV reform. Panel i. shows the ceiling among all ALG 2 claimants, whereas Panel ii. shows the ceiling among new claimants. *Source:* German Socio-Economic Panel (SOEP).

Figure G.2)—and live in a building with total (heating) area of 500-1000m^2 (single household) or 250-500m^2 (other household types). The latter assumption again correspond to the modal building types in which long-term unemployed jobseekers reside, according to data from the SOEP.

Both assumptions should not introduce large measurement error in the total ceiling, as variation across heating and building types is limited, in particular for single-person households, who constitute the vast majority in my estimation sample. For instance, in 2007, the appropriate heating ceilings vary from €1.075 per square meter (gas, building size $>$ 1000m^2) to €1.39 per square meter (oil, building size 100-250m^2) among the three most common heating types, which cover roughly 90% of all claimants. For single-person claimants—the vast majority in my sample ($\approx$ 70%)—the assumptions on heating and building types would at max introduce a mismeasured gap of around €13.[41] I finally multiply the obtained heating ceiling per square meter with the locally appropriate floor space, depending on household size.

A second source of measurement error in the ceiling comes from variation across municipalities *within* counties. Figure G.3 provides the example of ceilings in the Oder-Spree-Kreis from 2005 onward, where two different ceilings were applied to two separate groups of municipalities.
When ceilings vary across municipality groups within a county, I assume that jobseekers reside in the modal group based on aggregate stocks on long-term unemployment benefit claimants from the Federal Employment Agency. The population-weighted standard deviation across municipality groups—conditional on applying different ceilings within the county—is €15 for single-person households. In robustness checks, I also assign jobseekers to the minimum and maximum ceilings in their county, regardless of residency patterns in the aggregate stocks.

[41] Gas heating in a building of 500-1000m^2 is considered inappropriate starting at €1.12 per square meter.

Figure G.3: Rent Ceiling in Oder-Spree from 2005 onward

	1 Person	2 Personen	3 Personen	4 Personen	jede weitere Person
als angemessen gelten:	50 m² oder 2 Räume	65 m² oder 2 Wohnräume	80 m² oder 3 Wohnräume	90 m² oder 4 Wohnräume	+ 10 m²
Orte u Gemeinden Gruppe 1, Mietstufe III (5,07 €/m²) Fürstenwalde, Schöneiche b. Berlin, Woltersdorf, Grünheide, Hangelsberg	253, 50 €	329, 55 €	405,60 €	456, 30 €	50,70 €
Orte u Gemeinden Gruppe 2, Mietstufe II (4,76 €/m²) Beeskow, Eisenhüttenstadt, Mullrose, Storkow, Erkner und übrige Gemeinden	238, 00 €	309,40 €	380,80 €	428,40 €	47,60 €

Notes: This figure shows the ceiling by household size and municipality group for the county of Oder-Spree from 2005 onward. The first row displays values for municipality group 1 (*1. Mietstufe*), whereas the second row shows values for municipality group 2 (*2. Mietstufe*). Rent ceilings refer to rent excluding bills (*Nettokaltmiete*).

G.2 Predicting Ceiling Exemptions

I exploit the kinked benefit schedule in ALG 2 benefits that to (most) long-term unemployment spells after six months of claiming. More specifically, benefits for spell s from the seventh month onward are given by:

$$b_s = \begin{cases} n_s + r_s - y_s - s_s & \text{if } r_s \leq c_{jth(s)} | \left((r_s > c_{jth(s)}) \cap \mathbf{1}\{\text{Exempted}_s\} \right) \\ n_s + c_{jth(s)} - y_s - s_s & \text{if } (r_s > c_{jth(s)}) \cap \mathbf{1}\{\text{Not Exempted}_s\} \end{cases} \tag{G.1}$$

where n_s denote needs, r_s rent, y_s income, s_s benefit sanctions and exempted is being defined as receiving a housing subsidy above the ceiling after six months of claiming. For roughly 50% of my sample, I don't observe benefits from the seventh month onward as they exit long-term unemployment before. The only unknown in Equation (G.1) is the individual probability of exemption when exceeding the local ceiling, as I observe all other components of the benefit formula (during the months prior to exiting).

To predict their individual probability of exemption, I use a 90% training sample among jobseekers who reach month seven of their spell and estimate:

$$\mathbf{1}\{\text{Exempted}_s\} = \sum_k \beta_k \cdot \mathbf{1}\{(r_s - c_{jth(s)} \in \text{bin}_k\} + X_s'\gamma + \epsilon_s \tag{G.2}$$

where the first term captures €5-bins in the running variable, and X_s is a vector of covariates observed at entry into long-term unemployment (listed in Table F.1). β_k captures that jobseekers are not exempted below the ceiling, and that exemption practices might vary depending on by how much the rent exceeds the local ceiling. For instance, in case of small deviations from the ceiling, jobseekers might be less strict in applying the ceiling, compared to large deviations.

I then use the estimated coefficients from Equation (G.2) to predict exemption probabilities for jobseekers who exit long-term unemployment before the seventh month.

Table G.1: Set of Predictors Used for Predicting Exemption from the Local Rent Ceiling

Jobseeker Characteristics	**Recipient Status**	**Ceiling Characterisitcs**
Age (binned)	Search status	Commuting zone
Education	Parallel UI recipience	Differences in ceiling within county
Sex	Able to work	
Household composition		
Available income		

H Migration Response: Additional Material

H.1 Deriving the Migration Elasticity in the City

For ease of exposition, I ignore group and time subscripts. Based on the location choice Equation (21), and assuming that the taste shocks $\phi_j(i)$ are i.i.d. distributed across jobseekers and locations following a Gumbel$(1/\sigma)$ distribution, the probability of choosing location j can be written as:

$$\pi_j = \frac{\exp\left[\sigma \times \left(s_j \cdot U\left(\frac{w_j}{p_j}\right) + (1-s_j)U\left(\frac{b_j}{p_j}\right) - \psi(e_j)\right)\right]}{\sum_k \exp\left[\sigma \times \left(s_k \cdot U\left(\frac{w_k}{p_k}\right) + (1-s_k)U\left(\frac{b_k}{p_k}\right) - \psi(e_k)\right)\right]} \tag{H.1}$$

Let me further denote v_j as the common component of indirect utility when choosing location j:

$$v_j \equiv s_j \cdot U\left(\frac{w_j}{p_j}\right) + (1-s_j)U\left(\frac{b_j}{p_j}\right) - \psi(e_j) \tag{H.2}$$

Then, the migration elasticity in location j with respect to local benefits b_j can be written as:

$$\begin{aligned}
\epsilon_j^{\text{migr}} &\equiv \frac{d\log \pi_j}{d\log b_j} = \frac{1}{\pi_j} \cdot \frac{d\pi_j}{d\log b_j} = \frac{1}{\pi_j} \cdot \frac{d\pi_j}{db_j} \cdot b_j \\
&= \frac{1}{\pi_j} \cdot \sigma \cdot \frac{dv_j}{db_j} \cdot \pi_j \cdot (1-\pi_j) \cdot b_j && \text{(prop. of multinomial logit)} \\
&= \sigma \cdot (1-s_j) \cdot U'\left(\frac{b_j}{p_j}\right) \cdot (1-\pi_j) \cdot b_j \\
&= \sigma \cdot (1-s_j)\left(\frac{b_j}{p_j}\right)^{1-\rho} \cdot (1-\pi_j) && \text{(CRRA utility)}
\end{aligned}$$

To obtain a migration elasticity for the city—in the two-location case as in the model—I take a weighted average of $(1-s_j)$ and the real-benefit term:

$$\epsilon_{\text{City}}^{\text{migr}} = \sigma \cdot \frac{\sum_{j\in\text{City}} \pi_j \cdot (1-s_j) \cdot \left(\frac{b_j}{p_j}\right)^{1-\rho}}{\pi_{\text{City}}} \cdot (1-\pi_{\text{City}}) \tag{H.3}$$

which yields Equation (25).

H.2 Parametritzing the Utility and Search Cost Functions

Estimating the effort cost function parameters. I start by deriving the estimating equation for the search elasticity ψ. Recall the following two equations, in which I have assumed that search efficiency is time-invariant and constant across groups, and that the effort cost shifter k_g is constant across time and space.

$$\psi(e_{jgt}) = k_g \cdot \frac{e_{jgt}^{1+(1/\phi)}}{1+(1/\phi)} \qquad \text{(assumption on effort costs)}$$

$$\psi'(e_{jgt}) = z_j \cdot \left[U\left(\frac{w_{jt}}{p_{jt}}\right) - U\left(\frac{b_{jgt}}{p_{jt}}\right) \right] \qquad \text{(first-order condition on effort)}$$

Combining these two equations yields the estimating equation for the search elasticity ϕ:

$$e_{jgt}^{1/\phi} = \frac{z_j}{k_g} \cdot \left[U\left(\frac{w_{jgt}}{p_{jgt}}\right) - U\left(\frac{b_{jgt}}{p_{jgt}}\right) \right] \tag{H.4}$$

$$\log(e_{jgt}) = \log(z_j) - \log(k_g) + \phi \cdot \log\left[U\left(\frac{w_{jgt}}{p_{jt}}\right) - U\left(\frac{b_{jgt}}{p_{jt}}\right) \right] \tag{H.5}$$

I estimate Equation (H.5) using individual jobseeker information on search effort from the matched survey (PASS-ADIAB). Specifically, I estimate for jobseeker i in year t:

$$\log(e_{it}) = \beta_0 + \phi \cdot \log\left[U\left(\frac{w_{it}}{p_{jt(it)}}\right) - U\left(\frac{b_{it}}{p_{jt(it)}}\right) \right] + X_{it}'\delta + \tau_t + \eta_{j(it)} + \epsilon_{it} \tag{H.6}$$

I use the number of sent applications during the previous four weeks as my measure for effort e_{it} following Faberman et al. (2022). Since 60% of individuals in my sample did not submit any job applications in the previous four weeks despite being unemployed, I estimate a Poisson regression model, using the number of applications as the measure of search effort. For completeness, I also report results for the outcome being $\log(1 + \#\text{applications}_{it})$. The survey asks jobseekers about their expected re-employment earnings and current benefit amounts, which I use to measure w_{it} and b_{it}, respectively. I measure $p_{jt(it)}$ using the price index from Section 3.1. X_{it} includes socio-demographic characteristics as well as information on individuals' prior employment histories. τ_t and $\eta_{j(it)}$ are year fixed effects and local labor market fixed effects. The latter capture regional variation in search efficiency.

I estimate (H.6) using OLS and IV. Note that any unobserved factor influencing both search effort (through the shifter k_g which is part of the error term ϵ_{it}) and the utility difference between employment and unemployment may lead to inconsistent estimates of the search elasticity ϕ. To address this concern, I instrument the utility difference by using programmatically assigned benefit amounts instead of actual benefits as an input b_{it}, similar to Krueger and Mueller (2010).

Table H.1: Search Elasticity Estimates

	#Applications		log(1 + #Applications)		
	Poisson	Poisson IV	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)
$\log\left[U\left(\frac{w}{p}\right) - U\left(\frac{b}{p}\right)\right]$	-0.009	0.261***	-0.004	0.193**	0.191
	(0.034)	(0.074)	(0.017)	(0.083)	(0.273)
Controls	✓	✓	✓	✓	✓
Commuting Zone FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Individual FE					✓
Mean DV	4.41	4.43	0.94	0.95	0.90
Kleibergen-Paap F-stat.		105		105	16
Observations	24,051	22,566	24,051	22,566	15,833

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I Consumption Smoothing Benefits: Additional Material

I.1 Estimating the Ratio of Marginal Utilities of Consumption

To estimate the ratio of marginal utilities of consumption in the unemployed state, I follow Piqueras (2025) and approximate the ratio of marginal utilities by expressions that I can estimate from the data.

$$\frac{U'(c_c^U)}{U'(c_r^U)} \approx \frac{U'(c_c^E) \cdot \left(1 + \rho \frac{c_c^E - c_c^U}{c_c^E}\right)}{U'(c_r^E) \cdot \left(1 + \rho \frac{c_r^E - c_r^U}{c_r^E}\right)} \tag{I.1}$$

$$\approx \frac{U'(c_r^E)}{U'(c_r^E)} \cdot \left(1 + \rho \frac{c_r^E - c_c^E}{c_r^E}\right) \cdot \frac{1 + \rho \frac{c_c^E - c_c^U}{c_c^E}}{1 + \rho \frac{c_r^E - c_r^U}{c_r^E}} \tag{I.2}$$

$$= \left(1 + \rho \frac{c_r^E - c_c^E}{c_r^E}\right) \cdot \frac{1 + \rho \frac{c_c^E - c_c^U}{c_c^E}}{1 + \rho \frac{c_r^E - c_r^U}{c_r^E}} \tag{I.3}$$

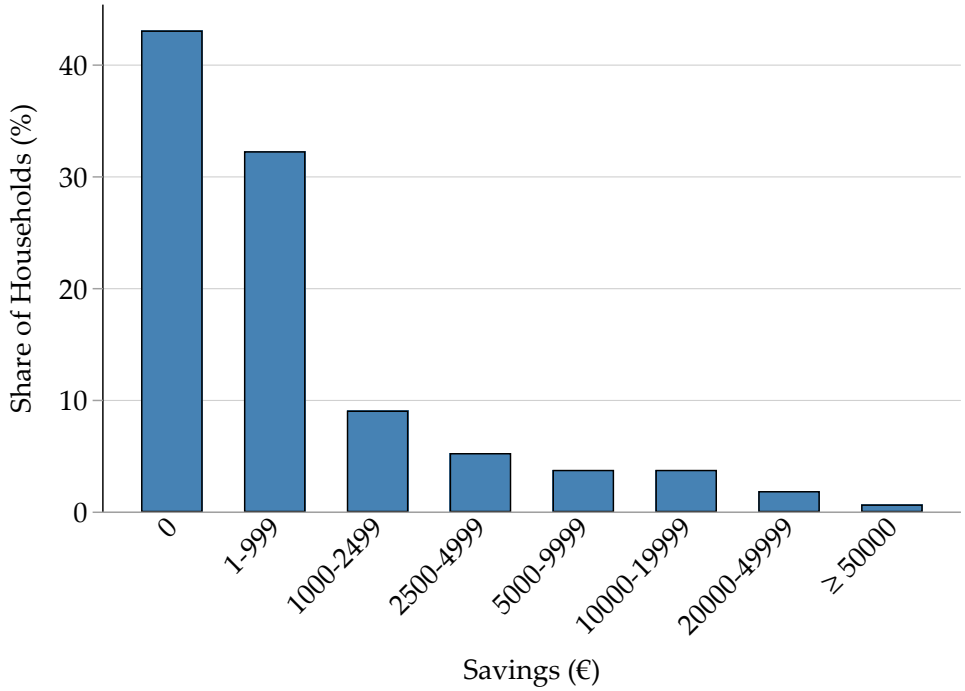
where the first line implements a Taylor approximation of $U'(c_j^U)$ around $U'(c_j^E)$ and the second line follows from another Taylor approximation of $U'(c_c^E)$ around $U'(c_r^E)$.

Equation (I.3) shows the two main components that are needed to approximate the ratio of marginal utilities: (i) differences in the consumption level before jobseekers enter long-term unemployment, and (ii) the ratio of consumption losses upon long-term unemployment entry between jobseekers in urban and rural locations. I use a sample of individuals entering long-term unemployment between 2008 and 2021, matched to household-level survey information from the PASS-ADIAB dataset. Although these data do not contain direct measures of consumption, they provide information on net household income, savings, and indicators of material deprivation across several spending categories, including durable goods such as cars, household appliances, and clothing.

Consumption measurement. I use monthly net equivalized household income as my baseline measure for household-level consumption. This assumes that households don't use (financial) assets to smooth consumption losses upon long-term unemployment entry ("hand-to-mouth"). Figure I.1 shows that around 75% of households have less than €1,000 of savings in the year before entering long-term unemployment, and 43% have no savings at all.[42] The baseline analysis treats real net household income as a proxy for household consumption. To assess robustness, I first restrict the sample to households without savings, for whom this proxy is likely to be most accurate. I further consider an alternative outcome based on an affordability index that aggregates indicators of affordability across 21 consumption items, spanning housing, durable consumption items (e.g. car, household appliances).

[42] The distribution of savings is very similar between long-term unemployment entrants in urban and rural locations, with 72.5% in urban and 77.3% and rural locations having less than €1,000 of savings, respectively.

Figure I.1: Distribution of Savings among Households before Entering Long-Term Unemployment



Notes: This figure shows the distribution of household savings among households that enter long-term unemployment in my sample.

I.2 Consumption Level Before Long-Term Unemployment

I first assess differences in consumption levels prior to entry into long-term unemployment between jobseekers in urban and rural locations. Following Piqueras (2025), I estimate the following regression using a sample of households that enter long-term unemployment and a comparison group that does not enter in the subsequent year. The sample is restricted to households observed in two consecutive survey waves, residing in location j in year t:

$$y_{ht} = \beta_0 + \beta_1 \mathbf{1}\{\text{Will Enter LTU}\}_{ht} + \beta_2(\mathbf{1}\{\text{Will Enter LTU}\}_{ht} \times \mathbf{1}\{\text{City}\}_{j(ht)}) + X'_{ht}\gamma + \tau_t + \epsilon_{ht} \quad \text{(I.4)}$$

The outcome variable y_{ht} denotes consumption proxies—log real equivalized household income and the consumption affordability index. The indicator $\mathbf{1}\{$ Will Enter LTU $\}$ equals one for households that enter long-term unemployment in the subsequent year. The vector X_{ht} includes household- and household-head-level sociodemographic characteristics, and τ_t denotes year fixed effects. I cluster standard errors at the household level, and use cross-sectional survey weights to ensure sample representativeness

Tables I.1 and I.2 show the main results for equivalized income and affordability, respectively. The results for both outcomes indicate that households entering long-term unemployment already face lower consumption opportunities in the year prior to benefit receipt. However, this pattern does not differ systematically between urban and rural locations. If anything, the small and statistically insignificant interaction estimates suggest that households who will enter long-term unemployment in urban areas exhibit slightly higher income and consumption affordability. This result holds when restricting the sample to households with less than €1,000 of savings (columns (4)).

Table I.1: Differences in Log Real Equivalized Household Income Prior to Long-Term Unemployment Entry

Sample	Full			<€1,000 Savings
	(1)	(2)	(3)	(4)
1{Will Enter LTU}	-0.601***	-0.501***	-0.491***	-0.313***
	(0.027)	(0.026)	(0.027)	(0.028)
1{Will Enter LTU} · **1**{City}	0.017	0.055	0.036	0.023
	(0.053)	(0.045)	(0.047)	(0.048)
1{City}			0.019	
			(0.016)	
Controls		✓	✓	✓
Observations	49,299	48,684	48,684	18,959
LTU Entrants	1,719	1,677	1,677	1,348

Notes: This table shows differences in log real equivalized household income among households who will claim long-term unemployment benefits in the subsequent year, relative to households who will not enter long-term unemployment. A city is defined living in a commuting zone in the top tercile of the rent distribution. Cross-sectional household weights are used. Standard errors are clustered at the household level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table I.2: Differences in the Affordability Index Prior to Long-Term Unemployment Entry

Sample	Full			<€1,000 Savings
	(1)	(2)	(3)	(4)
1{Will Enter LTU}	-3.140***	-2.906***	-2.798***	-1.841***
	(0.179)	(0.175)	(0.177)	(0.198)
1{Will Enter LTU} · **1**{City}	0.083	0.320	0.118	0.193
	(0.366)	(0.339)	(0.343)	(0.372)
1{City}			0.206***	
			(0.058)	
Controls		✓	✓	✓
Observations	67,221	66,430	66,430	23,723
LTU Entrants	1,719	1,677	1,677	1,348

Notes: This table shows differences in a consumption affordability index comprising 21 different spending items among households who will claim long-term unemployment benefits in the subsequent year, relative to households who will not enter long-term unemployment. A city is defined living in a commuting zone in the top tercile of the rent distribution. Cross-sectional household weights are used. Standard errors are clustered at the household level. Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

I.3 Consumption Drop at Long-Term Unemployment Entry

I estimate the consumption response to entry into long-term unemployment using a matched event-study design (e.g., Landais and Spinnewijn, 2021). Specifically, I implement a two-step matching procedure. First, I match households exactly on year, household composition, prior location (urban versus rural), savings (below or above €1,000), and the household head's pre-entry employment status. Second, within each matching cell, I pair each long-term unemployment entrant with the household that has the closest propensity score based on pre-entry net household income and household-head characteristics (age, migration status, and educational attainment).

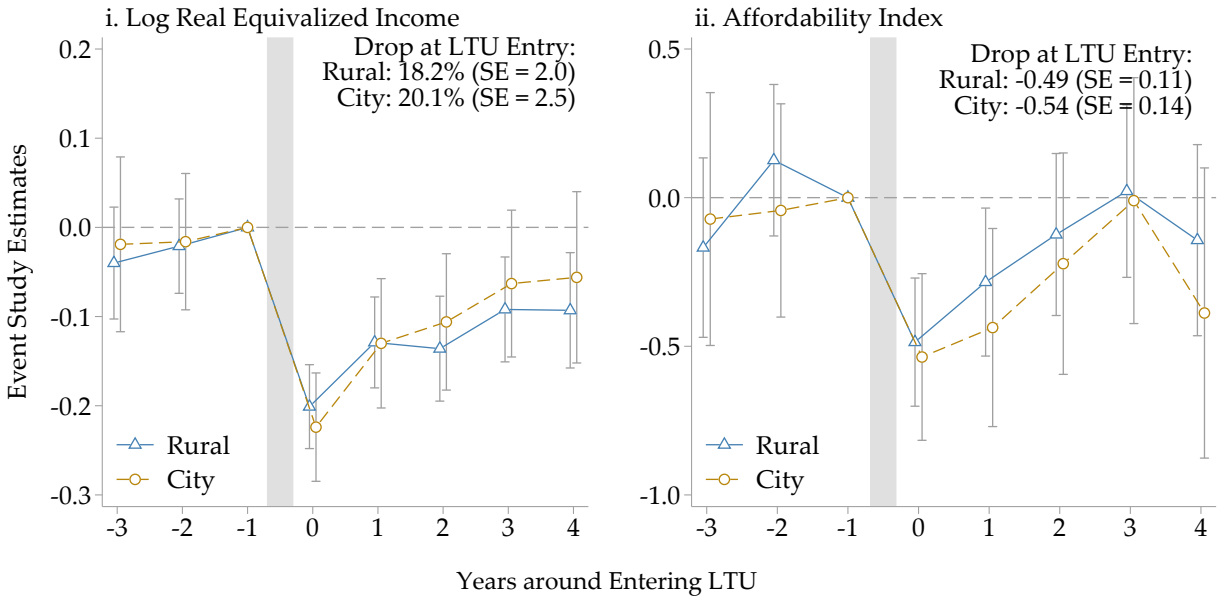
Using this matched sample, I estimate the following event-study specification separately for urban and rural entrants and their matched controls:

$$y_{hct} = \sum_{\substack{k=-3 \\ k\neq -1}}^{4} \delta_k^j \cdot (\mathbf{1}\{\text{Enters LTU}\}_h \times \mathbf{1}\{t = c + k\}) + \sum_{\substack{k=-3 \\ k\neq -1}}^{4} \gamma_k^j \times \mathbf{1}\{t = c + k\} + \pi_t^j + \alpha_h^j + X_{ht}'\beta^j + \epsilon_{hct} \tag{I.5}$$

for household h of cohort c in year t, who resides in location j (urban/rural) in event time $k = 0$. Enters LTU$_h$ is equal to one for households in the matched sample that I observe entering long-term unemployment from one survey wave to the next one. π_t^j and α_h^j are year and household fixed effects, respectively. X_{ht} controls for household composition and age of the household head.

Main results. Figure I.2 shows the estimated event study coefficients (δ_k^{city} and δ_k^{rural}) for the two main outcomes of interest: log real equivalized household income (Panel i.) and the consumption affordability index (Panel ii.). Both outcomes exhibit sizable declines upon entry into long-term unemployment. Real household income falls by about 20% in urban areas and 18% in rural areas, while the affordability index declines by roughly 0.5 points in both locations.

Figure I.2: Matched Event Study Estimates among LTU Entrants in Urban and Rural Locations



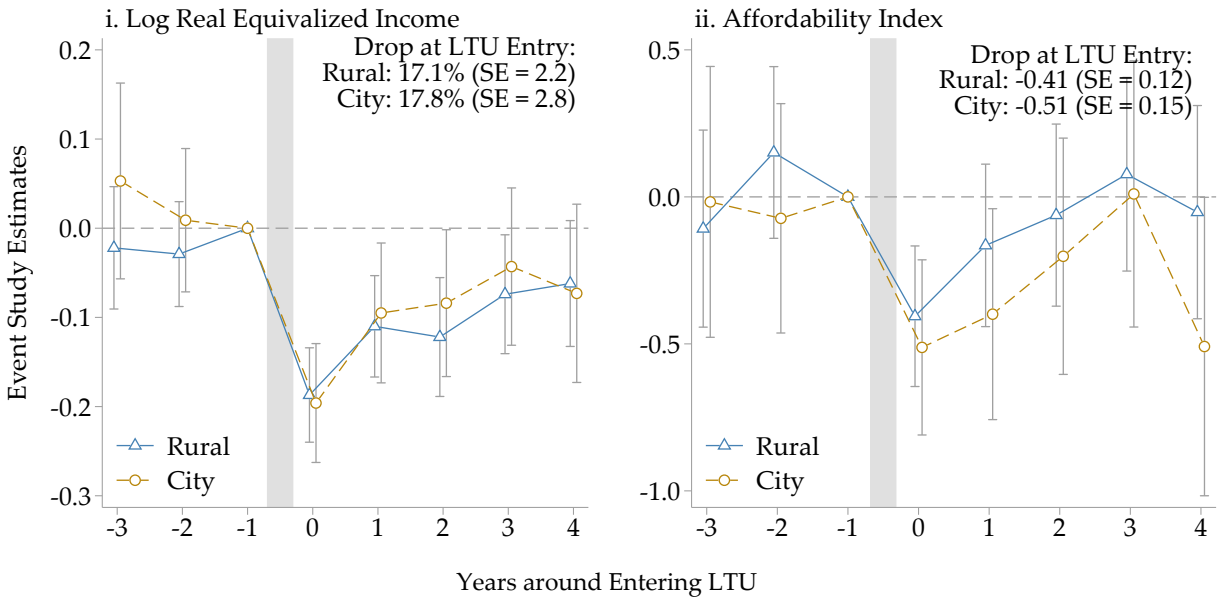
Notes: This figure shows the event-study coefficients from estimating Equation (I.5), separately for a matched sample of long-term unemployment entrants in urban and rural locations. Panel i. shows the results for log real equivalized household income and Panel ii. for a consumption affordability index comprising 21 different spending items. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals. Standard errors are clustered at the household level.

Although these estimates are large when interpreted as consumption responses to long-term unemployment—relative to estimates at entry into unemployment insurance (UI)—they closely mirror the spending declines observed among households exhausting UI in the United States (Ganong and Noel, 2019). Most importantly, these estimates are very similar across urban and rural locations, implying that the second term in Equation (I.3) is approximately equal to one.

Robustness. I estimate Equation (I.5) on a matched sample of long-term unemployment entrants with less than €1,000 in pre-entry savings, who are plausibly hand-to-mouth, to alleviate concerns that households smooth consumption losses through savings.

Figure I.3 shows that income and affordability declines in the hand-to-mouth subsample closely mirror those in the full sample. Income losses are slightly smaller, reflecting lower pre-unemployment earnings in this group and, consequently, smaller declines when benefits are set at local subsistence levels. Crucially, the income and affordability responses—used as proxies for consumption—again do not differ significantly between long-term unemployment entrants in urban and rural areas.

Figure I.3: Matched Event Study Estimates among LTU Entrants in Urban and Rural Locations with Less Than €1,000 Savings Pre-Entry



Notes: This figure shows the event-study coefficients from estimating Equation (I.5), separately for a matched sample of long-term unemployment entrants in urban and rural locations with less than €1,000 savings in the year prior to long-term unemployment. Panel i. shows the results for log real equivalized household income and Panel ii. for a consumption affordability index comprising 21 different spending items. See text for details on the matching procedure. Capped spikes denote 95 percent confidence intervals. Standard errors are clustered at the household level.