

The Effects of Regional Unemployment Benefits: Evidence from Germany's Hartz IV Reform *

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Abstract

This paper studies the effects of regional unemployment benefits on location choices and job-finding rates of unemployed workers. I exploit changes to the unemployment benefit design of long-term unemployed workers in Germany's Hartz IV reform in 2005, which resulted in higher benefits in high-rent, typically urban regions, irrespective of the worker's previous employment history. I show that the reform led to higher urban concentration among long-term unemployed workers. Using dynamic (triple) difference-in-differences specifications, I provide causal evidence that the reform led to higher mobility among jobseekers at high risk of long-term unemployment, which is particularly directed towards large cities. Housing assistance, as included under the post-reform long-term unemployment benefits, is crucial for explaining the observed mobility patterns. Finally, I document that unemployed job-seekers in cities have lower re-employment rates when approaching long-term unemployment, compared to job-seekers in rural areas.

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1 Introduction

Unemployment is a spatial phenomenon. Across many developed countries, the difference in unemployment rates between low and high unemployment rate commuting zones persistently exceeds ten percentage points (Bilal, 2023; Kuhn et al., 2022). Yet, in spite of these large spatial disparities in unemployment rates, there is only limited migration from economically depressed regions towards labor markets with better employment opportunities among the unemployed, particularly for jobseekers who have experienced long periods of unemployment.

In this paper, I study how the design of unemployment benefits affects the spatial distribution of unemployment through their effects on regional mobility and spatial differences in job-finding among unemployed workers. I exploit changes to the long-term unemployment benefit structure in Germany's Hartz IV reform in 2005, which left short-term unemployment benefits (unemployment insurance (UI)) largely unaffected. Long-term unemployed workers are displaced workers who have exhausted their initial stream of unemployment benefits and therefore rely on typically less generous long-term unemployment assistance. Before the reform, the benefit height of unemployment assistance was primarily determined by labor earnings before the unemployment spell. After the reform, long-term unemployed workers received housing assistance and a lump sum payment as a function of household composition. Under the new scheme, total benefits were therefore higher in high-rent, typically urban regions, *irrespective* of the previous employment history of the claimant. I argue that paying the housing cost as part of long-term unemployment benefits insures jobseekers who are (or about to become) long-term unemployed against a loss of *real* benefits when moving to more expensive labor markets with potentially better re-employment opportunities.

Using social security records from a 2% random sample of German employment biographies, I start by documenting descriptively how the reform affected location choices of unemployed workers. I show that long-term unemployed workers became geographically more concentrated in regions with higher population density and higher rents after the reform. Around 20% of the immediate increase in urban concentration can be explained by sorting, i.e. long-term unemployed workers disproportionately relocating to urban areas. These increases in mobility can be particularly observed among unemployment insurance recipients, who later enter long-term unemployment.

To provide casual evidence on the effects of introduction regional unemployment benefits on location choices, I study the mobility of workers during unemployment insurance, i.e. before potentially entering long-term unemployment. This choice is guided by legal barriers, which limited the possibility to move for jobseekers claiming long-term unemployment benefits after the reform, whereas jobseekers were free to chose their residency location during unemployment insurance. Since Hartz IV primarily affected long-term unemployment benefits, the reform should have especially affected unemployment spells at high risk of entering long-term unemployment, with only small effects on workers who were unlikely exhausting UI benefits. To capture an individual's exposure to the reform, I predict the risk of entering long-term unemployment based on *pre-reform* data, exploiting the rich set of worker and employment biography information available in the SIAB. I then use this predicted exposure measure in a dynamic difference-in-difference design, comparing mobility during unemployment insurance among spells with high long-term unemployment risk to those with low risk of exhausting UI, before and after the reform. I find large mobility in-

creases after the implementation of Hartz IV, which persist up to 2.5 years after the reform. Relative to the pre-reform mobility rates among jobseekers at high risk of long-term unemployment, the effects are largest for moves towards urban areas.

To provide evidence that eligibility for housing assistance is crucial for explaining the documented effects on mobility, I exploit legal barriers in mobility for jobseekers below 25 years. In particular, job centers could deny jobseekers below 25 years access to housing assistance when long-term unemployed if the jobseekers had moved to a new flat before claiming long-term unemployment benefits (§22 SGB II, Paragraph 5). I exploit this age cut-off using a triple difference-in-differences design. I first compare differences in mobility between UI spells that enter long-term unemployment to UI spells that end in re-employment at least three months before exhaustion, similar to the time series evidence at the beginning. As the third difference, I additionally distinguish between claimants below 25 and above, restricting the estimation sample to claimants between 21 and 28 years of age. The triple DiD-findings suggest that eligibility for housing is crucial for explaining the observed mobility response, in particular with respect to mobility directed towards urban areas.

In the second part of my analysis, I study how the reform affected job finding rates differentially across space. Specifically, I track re-employment rates of short-term unemployed workers when approaching benefit exhaustion, before and after the reform and between unemployed workers in rural and urban areas. Following [Price \(2019\)](#), my empirical strategy exploits variation in the timing of short-term benefit exhaustion between unemployment cohorts (before/after), and within cohorts across workers, who are differently close to benefit exhaustion as a consequence of small age or labor market experience differences. In line with previous findings, I find that lower long-term unemployment benefits led to higher job-finding rates especially when workers were close to benefit exhaustion. Next, I examine whether these effects vary across residency locations among claimants. Consistent with smaller (larger) reform-induced benefit cuts in urban (rural) regions, I find a smaller (larger) increase in job-finding among claimants in cities (rural areas), compared to before the reform. I rule out differences in job supply for jobseekers close to benefit exhaustion between cities and rural areas as an alternative explanation for my finding.

This paper makes two contributions. First, I provide novel evidence on how *regional* long-term unemployment benefits affect unemployed workers. Previous work in the literature on (optimal) unemployment benefit design has studied the effects of general, i.e. non-regional specific, short-term benefit cuts on job-finding ([Kroft and Notowidigdo, 2016](#)) and mobility ([Fernandez-Navia, 2019](#)), and whether these effects vary over the business cycle ([Piqueras, 2023](#); [Schmieder et al., 2012](#)).¹ A recent paper by [Jung et al. \(2023\)](#) studies the effects of regional unemployment benefits in a spatial search and matching model. I add to this literature by showing that in the context of regionally varying benefits (or benefit cuts), unemployed workers respond by higher job-finding rates in local labor markets with lower benefits (larger benefit cuts), and by moving towards areas with higher benefits (smaller benefit cuts). In contrast to [Jung et al. \(2023\)](#), I provide a causal estimate for the migration elasticity with respect to benefit height, which they calibrate to match the mobility rate in West German districts.

Second, I add to the literature that studies the effects of the Hartz IV reform. Several papers have attributed the decline in the aggregate unemployment rate in Germany after the reform to the benefit cuts from Hartz IV ([Krause and Uhlig, 2012](#)). [Price \(2019\)](#) shows that

¹For a review on the effects of unemployment insurance benefits, see [Schmieder and von Wachter \(2016\)](#).

the reform increased job-finding rates among UI recipients, who are approaching a transition into (lower) long-term unemployment assistance. I first document that the decline in (long-term) unemployment rates after the reform varies across space, with larger (smaller) reductions in local labor markets with higher (smaller) reform-induced benefit cuts. Compared to [Price \(2019\)](#), I show that the effects of Hartz IV on job-finding rates also vary substantially across locations, with larger effects in rural counties compared to urban ones. Lastly, I provide novel evidence on how the reform affected geographic mobility, and particularly mobility from low- to high-benefit regions among long-term unemployed workers.

This paper is organized as follows. Section [2](#) describes the institutional context. In section [3](#), I introduce the administrative data and provide preliminary motivating evidence. Section [4](#) studies the effects of Hartz IV on the geographic allocation and mobility of long-term unemployed workers. Section [5](#) examines its effects on job-finding across space. Section ?? concludes.

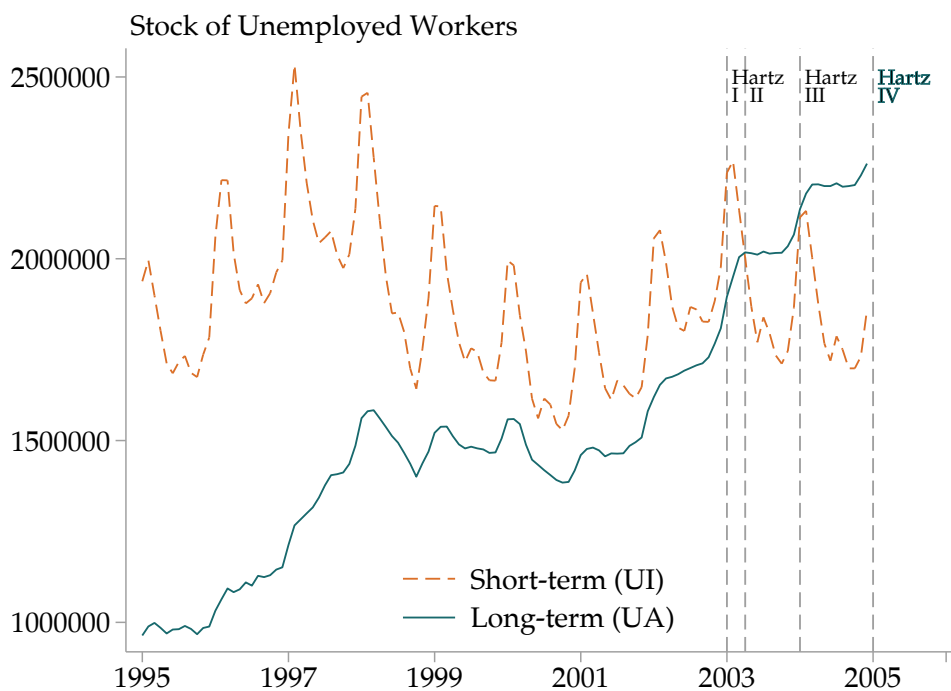
2 Institutional Context - Germany's Hartz IV Reform

I begin by describing the general context of the Hartz IV reform, before providing details on how long-term unemployment benefits were changed due to Hartz IV. Since geographic mobility will be a key outcome of my empirical analysis, I finally outline which mobility restrictions unemployed workers face, depending on the unemployment benefits they receive.

2.1 Reform Context

Due to the increasing stock of workers relying on long-term unemployment assistance (see Figure 1), the German government initiated a comprehensive set of labor market reforms - the Hartz reforms - in the early 2000s. Hartz I to III deregulated the temporary employment sector, broadened tax advantages for mini-jobs and reorganized the Federal Employment Agency. Hartz IV, arguably the centerpiece of the reform package and "Germany's most important labour-market reform since the war" ([The Economist, 2004](#)), changed the structure of long-term unemployment benefits. It was passed by the lower house of parliament (*Bundestag*) in December 2003, confirmed by the upper house (*Bundesrat*) in July 2004 and became effective on January, 1, 2005.

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

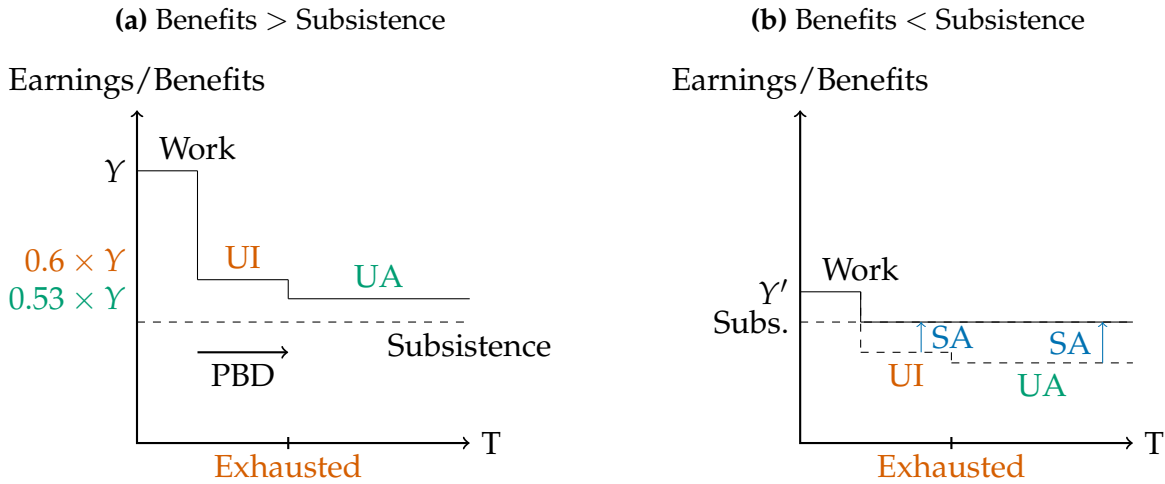


Notes: Vertical dashed lines indicate the month when each reform pillar of the Hartz reforms became effective. The data come from caseload statistics published by the German Federal Employment Agency (*Bundesagentur für Arbeit*).

2.2 Changes to Long-Term Unemployment Benefits

Benefits before Hartz IV Prior to the reform, Germany's unemployment benefit system consisted of three tiers. Unemployed workers received time-limited unemployment insurance (UI, *Arbeitslosengeld*) and would transition into unlimited, means-tested unemployment assistance (UA, *Arbeitslosenhilfe*) if they had not returned to work before exhausting UI (see Figure 2a). Whenever benefits were not enough to cover the claimant's assessed need (subsistence), benefits could be topped up by supplementary social assistance (SA, *Sozialhilfe*) (see Figure 2b).

Figure 2: Unemployment Benefits before the Reform



Workers on UI benefits would receive 60% (67% if they have dependent children) of their previous after-tax labor earnings. Their potential benefit duration (PBD) depended on age at UI entry and on months worked in the previous seven years before the UI spell.² After exhausting their initial UI stream, long-term unemployed workers would receive 53% (57%) of their previous after-tax labor earnings, subject to meeting the means-testing criteria. In principle, UA benefits could last indefinitely, subject to annual means testing. If benefits in UI or UA (or both) fell below the subsistence level, the difference between their assessed needs and their household income net of means testing would be covered by SA. Specifically, SA recipients would receive

$$SA = \max(0, \text{assessed need} - \text{household income net of means testing}) \quad (2)$$

where the assessed need covered individual allowances for household members of the claimant as well as housing and heating expenditures, as long as they were locally "appropriate".

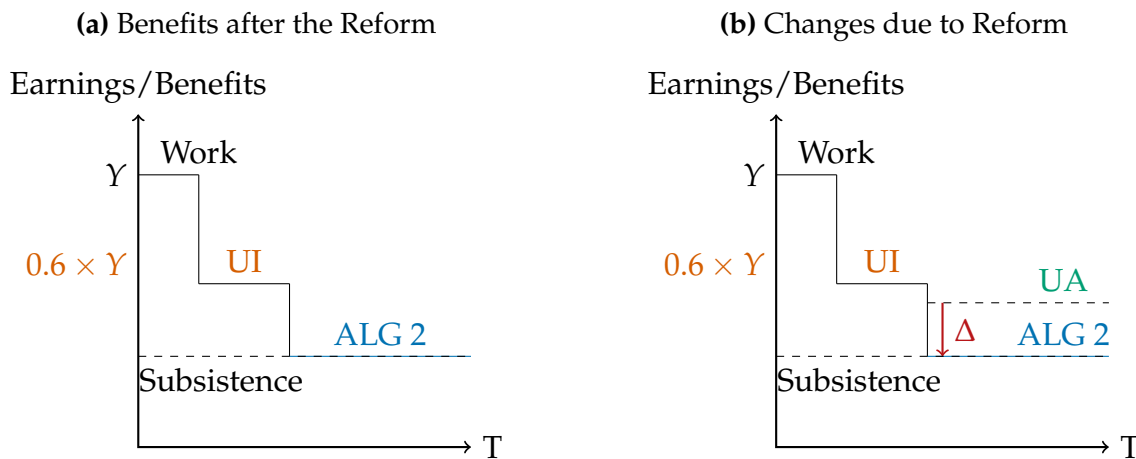
²The formula for PBD in months is:

$$PBD = \min[\bar{P}(a), 2 \cdot \text{floor}(m/4)] \quad (1)$$

where $\bar{P}(a)$ is an age-specific maximum duration, and m represents months of working before unemployment. $\bar{P}(a)$ 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.

Benefits after Hartz IV Hartz IV merged UA and SA together into a single system of subsistence benefits, *Arbeitslosengeld 2* (henceforth ALG 2), which no longer depended on labor earnings before the unemployment spell. Instead, benefits were computed similarly to SA before the reform (see Equation (2)). Long-term unemployed workers were therefore eligible for the sum of individual allowances (€345 in West, €331 in East Germany in 2005)³ in addition to their monthly rent and heating expenditures, as long as the latter were again considered to be locally appropriate.

Figure 3: Unemployment Benefits after the Reform (Benefits > Subsistence)



Notes: Panel (a) depicts the structure of unemployment benefits after the reform for workers, 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 a worker with benefits below subsistence. Abbreviations used: unemployment insurance (UI), unemployment assistance (UA), Arbeitslosengeld 2 (ALG 2), social assistance (SA).

In Figure 3, I show the benefit structure after the reform (Panel (a)) and the reform-implied changes (Panel (b)) for workers, whose UI and counterfactual UA benefits would have been above subsistence. Importantly, all long-term unemployed workers would receive benefits at the subsistence level under the post-reform scheme, and not only unemployed workers with low benefits as before (see Figure 2b). To mitigate the reform-induced benefit cuts (Δ) for the first UI-ALG 2 entry cohorts, Hartz IV featured supplementary benefits, which could cover up to two thirds of the benefit drop from UI to ALG 2 during the first year after exhaustion, and one third in the second year, before the recipient would transition to ALG 2 benefits only (Steffen, 2012).⁴ Hartung et al. (2022) show that while 13% of ALG 2 claimants in 2005 received these supplementary benefits, the number of recipients declined substantially in the subsequent years.

Apart from new entrants into long-term unemployment, the existing stock of 2.2mio. UA recipients would also be subject to the new ALG 2 benefit scheme starting on January 1st 2005, conditional on remaining eligible.⁵ Hartz IV had significant effects on these two

³The €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.

⁴The supplementary benefits were computed as:

$$\text{Supplementary Benefits} = \min[2/3 \cdot (\text{UI} - \text{ALG 2}), X_{h(i)} + C \cdot 60] \quad (3)$$

with $X_{h(i)}$ being equal to €160 or €320 for singles or couples, respectively. C denotes the number of dependent children.

⁵Since 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

groups of unemployed workers, who lost on average 24% of long-term unemployment cash benefits, corresponding to around 4.4% of net household income. Even among new entrants into long-term unemployment, who were temporarily eligible for mitigating benefits, 75% experienced a drop in long-term unemployment benefits, compared to counterfactual benefits in the absence of Hartz IV (Price, 2019).

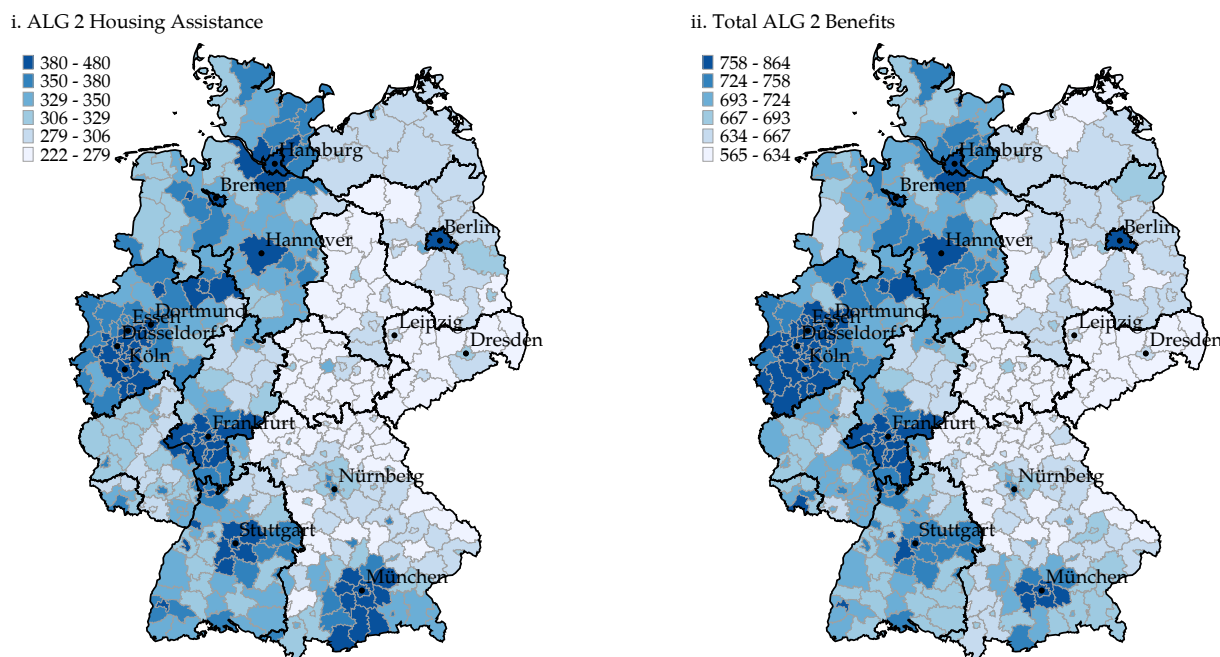
Earlier work has primarily used variation in previous earnings across workers to study the effects of Hartz IV (Hartung et al., 2022; Price, 2019). In the next section, I argue that by “indexing” ALG 2 benefits to local rents, benefit cuts due to Hartz IV might vary across locations and could therefore affect the geographic distribution of long-term unemployed workers.

2.3 Geographic Implications of Long-Term Benefits after Hartz IV

Long-term unemployment benefits under ALG 2 cover the actual rent of the claimant, as long as it is “locally appropriate” (§22 SGB II). Appropriateness is defined by the household size of the claimant, and the average rent for housing of adequate size (given household size) in the *residency municipality* of the claimant. In practice, local reference rent indices and similar sources are used by the local employment agency to determine the appropriateness of rents (Malottki et al., 2017, Section 3.2). If the actual rent exceeds the local reference rent, the employment agency paid the actual rent for six months, and the “appropriate” rent from then onward.

Figure 4 shows that the housing component of ALG 2 (Panel i.) generated sizeable variation in benefits across space in 2015, with higher benefits mostly in and around metropolitan areas (Panel ii.).

Figure 4: Average Housing Assistance and Total Benefit Height under ALG 2 by Residency County of Claimant as of 2015

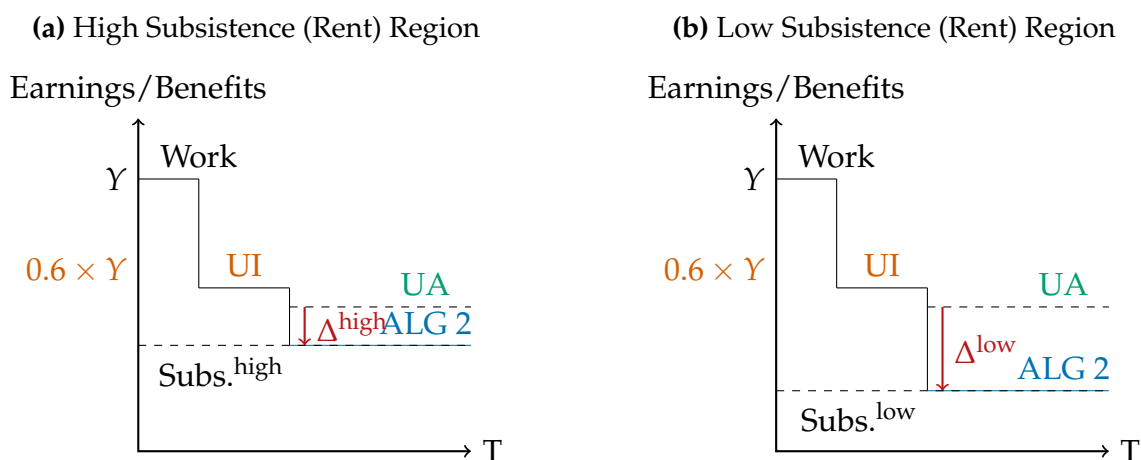


without children with one partner being employed lost eligibility. Empirically, this largely affected women, and particularly women in East Germany.

While missing values in official benefit numbers from the Federal Employment Agency during earlier years prevent me from showing the corresponding map in the year of introduction, Appendix Figure B3 shows that ALG 2 housing and total benefits varied considerably across space from 2005 onward.

As a consequence of these geographic differences in ALG 2 benefits, workers with identical labor earnings before unemployment would not necessarily receive equal benefits after Hartz IV. Figure 5 shows a stylized example of two workers with identical earnings, who exhaust in regions with different subsistence levels due to differences in rents.⁶ In this example, the benefit drop from UI to UA before the reform did not vary across locations, whereas it is lower (larger) in regions with high (low) rents under ALG 2.

Figure 5: Benefit Changes in High vs. Low Subsistence Regions



One potential implication of the post-reform benefit structure is that job seekers might have faced different incentives to avoid long-term unemployment, depending on their residency location. I will test this hypothesis empirically in Section 5. Another possible consequence of the reform relates to the location choices of (long-term) unemployed workers. Figure 5 suggests that jobseekers could increase their long-term unemployment benefits by moving to a location with higher subsistence benefits, typically high-rent, urban locations. To prevent long-term unemployed workers from manipulating their benefits through moving, any change in residency among ALG 2 claimants needed to be permitted by the responsible job center, and occurred only in the presence of necessary motives (e.g. family reunions, see §22 Paragraph 4, SGB II). However, UI recipients, if older than 24 years, could freely choose where to move without any consequences on their short- or subsequent long-term unemployment benefits. Only unemployed jobseekers below 25 years would not be eligible for housing assistance in their destination flat until they turn 25, if they had moved there before entering long-term unemployment (§22 Paragraph 5, SGB II). I provide further information on mobility restrictions in Appendix C. In Section 4, I study the effects on Hartz IV on location choices of jobseekers, distinguishing between new entrants and individuals already receiving long-term unemployment benefits.

⁶An alternative interpretation of this graph is that the same worker would face different benefit drops, depending on her residency location at exhaustion.

3 Data

3.1 Data Sources

I use a 2% random sample of German employment biographies (SIAB), provided by the Institute for Employment Research (IAB).⁷ The dataset records the exact start and end date 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. As geographic identifiers, the SIAB contains data on the residency and workplace county (N = 401) of each worker, the former starting in 1999. In some parts of my analysis, I aggregate counties to local labor markets based on commuting flows following [Kosfeld and Werner \(2012\)](#). To determine the urbanity of a worker's residency location, I use the Thünen urban classification as used in other work using German Social Security Data [Meister et al. \(2019\)](#). This classification assigns counties to three urban/rural bins, primarily based on their population density.

Limitations of the Data While my data allows me to observe wages, UI and UA benefits, I have no information on the height of ALG 2 benefits. For now, I can therefore not compute individual-specific benefit cuts due to the introduction of Hartz IV. In the future, I plan to use annual aggregate benefit height numbers at the county-household type-level from the Federal Employment Agency, which would allow me to capture regional differences in benefit changes due to the reform.

Furthermore, the SIAB does not contain high-frequency information on residency locations. In particular, residency locations are only updated at the beginning of the spell for most unemployment spells, and at the end of the year for most employment spells. While this recording procedure prevents me from analyzing the exact timing of moves (e.g. month before benefit exhaustion), I can nonetheless capture whether an unemployed worker moved during a spell or not, e.g. between entering short-term unemployment and long-term unemployment. However, when studying workers without any change in their employment status, for instance workers who remain long-term unemployed, I will have to use data at annual frequency, similar to previous work on location choices using German social security data (e.g. [Jayachandran et al., 2023](#)).

Finally, some administrative units (*Träger*) had problems with reporting their ALG 2 recipients to the Federal Employment Agency in 2005, and for a smaller subset (*zugelassene kommunale Träger*) up to 2006. In the raw data, a subset of *de facto* ALG 2 claimants are hence not recorded as recipients, but often only as unemployed jobseekers or workers undergoing active labor market policy programs (without benefits). For the administrative units with problems, I impute ALG 2 recipience if (i) the corresponding worker had exhausted his short-term benefits by 2005 (2006) and is recorded as an ALG 2 claimant in 2006 (2007), i.e. by the time when all units reported ALG 2 recipience consistently. Given that I use predicted long-term unemployment risk based on pre-reform data in my main empirical results, these reporting errors should not affect my main results.⁸

⁷For details on the dataset, see [Frodermann et al. \(2021\)](#).

⁸For the results using long-term unemployment take-up, I plan to run additional robustness checks, where I change the assumptions of my imputation.

3.2 Constructing the Estimation Samples

Based on the SIAB, I construct two estimation samples. My main sample consists of all unemployment spells at monthly frequency, that started between 2000 and 2009, i.e. five years before and after the Hartz IV reform. Since residency locations are only recorded from 1999 onward and still show implausible variation throughout 1999 and early 2000, my estimation sample often starts in the third quarter of 2000. In my analysis on job-finding rates, I restrict the unemployment cohorts to even narrower windows around the reform, only considering UI entries between 2000 and 2006, similar to [Price \(2019\)](#), whose methodology I closely follow. I restrict the sample to workers aged 16-54 at unemployment entry to abstain from early retirement decisions following previous work ([Schmieder et al., 2023](#); [Price, 2019](#)). I further impose observing the previous job before the separation, which allows me to measure key variables such as previous job characteristics or education of the worker.

For the stock of long-term unemployed, I further require that they enter long-term unemployment after having exhausted their initial stream of UI benefits. This ensures that I measure long-term unemployment consistently over the sample period. In particular, with the implementation of Hartz IV, all non-employed who are able to work (*erwerbsfähige Leistungsberechtigte*) had to register officially as unemployed, which caused a large spike in the stock of unemployed in January 2005, primarily coming from the rise in ALG 2 recipients (see e.g. [Hartung et al., 2022](#)). By imposing that I observe the exhaustion of UI benefits to be considered in my sample of long-term unemployed workers, I ensure that none of my results are merely driven by differences in the sample selection of long-term unemployed workers due to different formal registration requirements after the reform.

For my analysis on the mobility of 'stayers' in long-term unemployment (as opposed to new entrants), I use an annual version of the SIAB, where I consider the main spell of each worker on June, 30th (following the guidelines by [Dauth and Eppelsheimer, 2020](#)). This choice is guided by the infrequent, typically only annual, updating of the residency location variable among long-term unemployed workers before the reform.

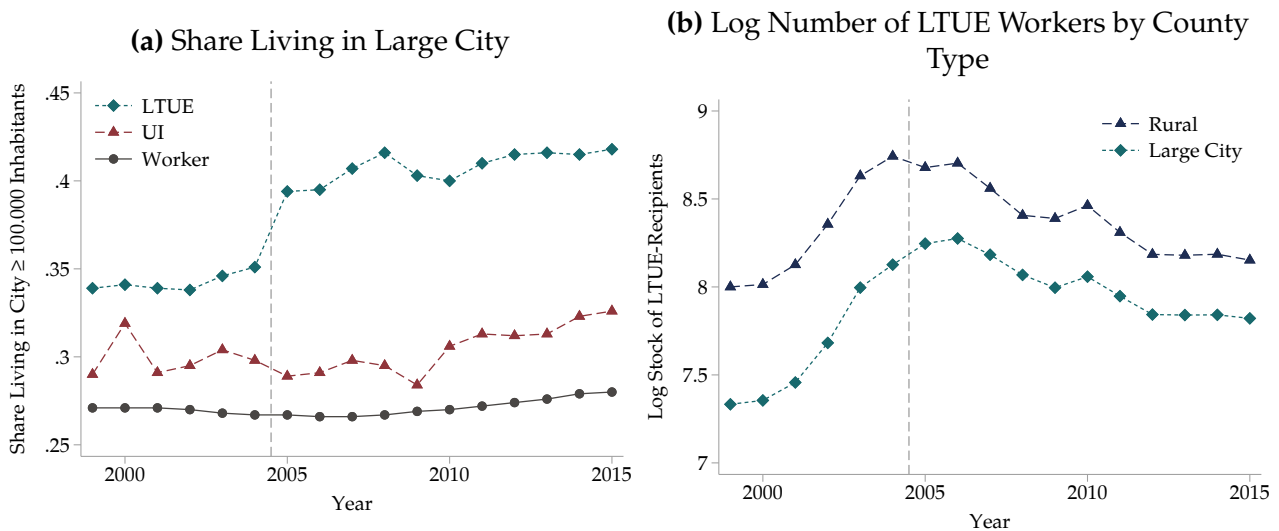
4 Location Choices

Given that post-reform benefits were higher in locations with higher rent, typically urban labor markets, let me now examine how the reform affected the geographic distribution and location choices of long-term unemployed workers.

4.1 Motivating Time Series

I start by showing descriptively how the geographic distribution changed among long-term unemployed workers as defined in Subsection 3.2 ("treated"). I compare their annual time series to short-term unemployed workers, who did not enter long-term unemployment ("control") during their unemployment spell, and a random sample of workers in June of every year. Given that ALG 2 benefits were higher in densely populated areas, Figure 6 plots how urban concentration evolved in the years around the reform among long-term and short-term unemployed workers.

Figure 6: Location Shares and Number by Worker Groups



Notes: Panel (a) shows the fraction of workers for each group whose residency county is urban according to the Thünen classification (Meister et al., 2019). Abbreviations used: long-term unemployed workers (LTUE), unemployment insurance recipients (UI).

Figure 6a depicts two observations. First, the share of long-term unemployment benefit recipients went up discontinuously from the year before the reform (2004) to the year after (2005). Six months after the reform, the share of long-term unemployed workers who live in a city with at least 100,000 inhabitants reached 39%, compared to 35% six months before the reform. This increase cannot be observed among short-term unemployed workers. Second, urban concentration further increased in the subsequent years among long-term unemployed workers with 42% having an urban residency location by the end of the time series. While short-term unemployed workers also became more likely to live in urban areas from 2010 onward, the rise in urban concentration in the immediate years after the reform uniquely applied to long-term unemployed workers. Panel 6b shows that the initial increase after the reform came from a disproportional increase in the stock of long-term unemployed workers in urban counties. Moreover, the subsequent increase in the share in Figure 6a came from a less pronounced decrease in long-term unemployment in cities, compared to rural areas.

The sudden increase of urban concentration in 2005 could have been driven by various factors. First, around 7% of previous long-term unemployed workers lost eligibility overnight with the introduction of ALG 2, which tended to be concentrated in East German and thus more rural regions. Second, the reform could have affected location choices of new and previous long-term unemployed jobseekers. Third, UI claimants might have had different incentives to avoid entering long-term unemployment, depending on their residency location. I now turn to testing the second hypothesis, before studying the effects of Hartz IV on job-finding among UI claimants in Section 5.

4.2 Mobility

Ex-ante, one could expect higher incentives to move among long-term unemployed workers. First, since benefit height could vary across locations for the same worker (see Figure 5), long-term unemployed workers might have had an incentive to move to locations with higher benefits, typically high-rent, urban locations. Second, the new long-term benefit structure could have reduced migration frictions to move to local labor markets with better job prospects, which usually have higher rents.⁹ In particular, jobseekers before the reform might have been discouraged to move to better labor markets because of the loss in *real* benefits. Since benefits under ALG 2 (approximately) equalized real benefits across space by construction, this migration barrier could have been abolished.

4.2.1 Mobility During Unemployment Insurance

I examine the effects of the Hartz IV reform on mobility of long-term unemployed workers using a dynamic difference-in-differences (DiD) design. I first focus on mobility during unemployment insurance as jobseekers faced no restrictions in their location choices during this stages of unemployment, whereas upon receiving ALG 2 benefits, job centers strongly encouraged recipients to ask for approval of their intended moves before relocating in order to remain eligible for housing assistance as part of their long-term unemployment benefits. If jobseekers were forward-looking, mobility might have already responded in the months leading up to potential long-term unemployment, similar to the documented increase in re-employment arounds before UI exhaustion (Price, 2019). In the first set of descriptive results, I use actual long-term unemployment take-up to assign spells to the treatment or control group. However, to avoid contamination of my results due to differences in the selection into long-term unemployment between before and after the reform, I then move on to use the predicted long-term unemployment risk based on pre-reform data to assign treatment status.

Time Series I start by showing average moving rates during UI recipience in the years around the Hartz IV reform. I use my sample of unemployment spells in the years around the reform as described in Subsection 3.2 and assign spells to the semester t in which they ended receiving unemployment insurance (either through finding a job or exhausting benefits). Mobility rates are shown at semi-annual (semesterly) frequency, which allows me to

⁹The bivariate correlation between job-finding rates among all unemployed workers and log rents at the county-level for $t \in [2007, 2019]$ is

$$\text{corr}(\text{job-finding rate}_{ct}, \log \text{rent}_{ct}) \approx 0.15 \quad (p < 0.01)$$

Among long-term unemployed workers (continuously unemployed for at least 12 months), the corresponding correlation is weaker, but still positive ($\text{corr} \approx 0.02$).

zoom in well on the dynamics in the months around reform announcement and implementation while having sufficiently large sample sizes to construct both time series. I measure geographic mobility as differences in the residency location of the jobseeker between entering unemployment insurance and the end of receiving UI benefits. Since the Hartz IV reform primarily affected jobseekers who would enter (or were at risk of entering) long-term unemployment, I show mobility rates among unemployment spells that terminated in long-term unemployment ("treated"), and compare them to mobility rates among spells which terminated UI at least three months before exhaustion through finding a job ("control").¹⁰ This choice of the control group is motivated by the argument that behavioral responses (e.g. job search, location choices) to long-term unemployment benefits are limited if jobseekers are still sufficiently far away from relying on long-term unemployment assistance (Price, 2019).

Figure 7 shows the corresponding time series for four types of moves: moves across counties¹¹, moves across local labor markets, moves (across counties) towards urban areas and moves towards rural areas. The latter two are defined according to the Thünen classification as outlined in Subsection 3.1.

Across all moving outcomes, spells that entered long-term unemployment show significantly higher mobility rates after the reform with large spikes in the immediate semesters after reform implementation. Moreover, while mobility rates among spells that transitioned into long-term unemployment decline slightly towards the end of the sample period, they still remain above mobility rates of spells which ended in re-employment at least three months before UI exhaustion, contrary to *lower* mobility rates before the reform. While moves towards urban and rural areas seem to have increased after the reform (Figures 7c and 7d), the relative increase was larger for moves towards cities, given that mobility towards cities among jobseekers who would enter long-term unemployment was very low before the reform.

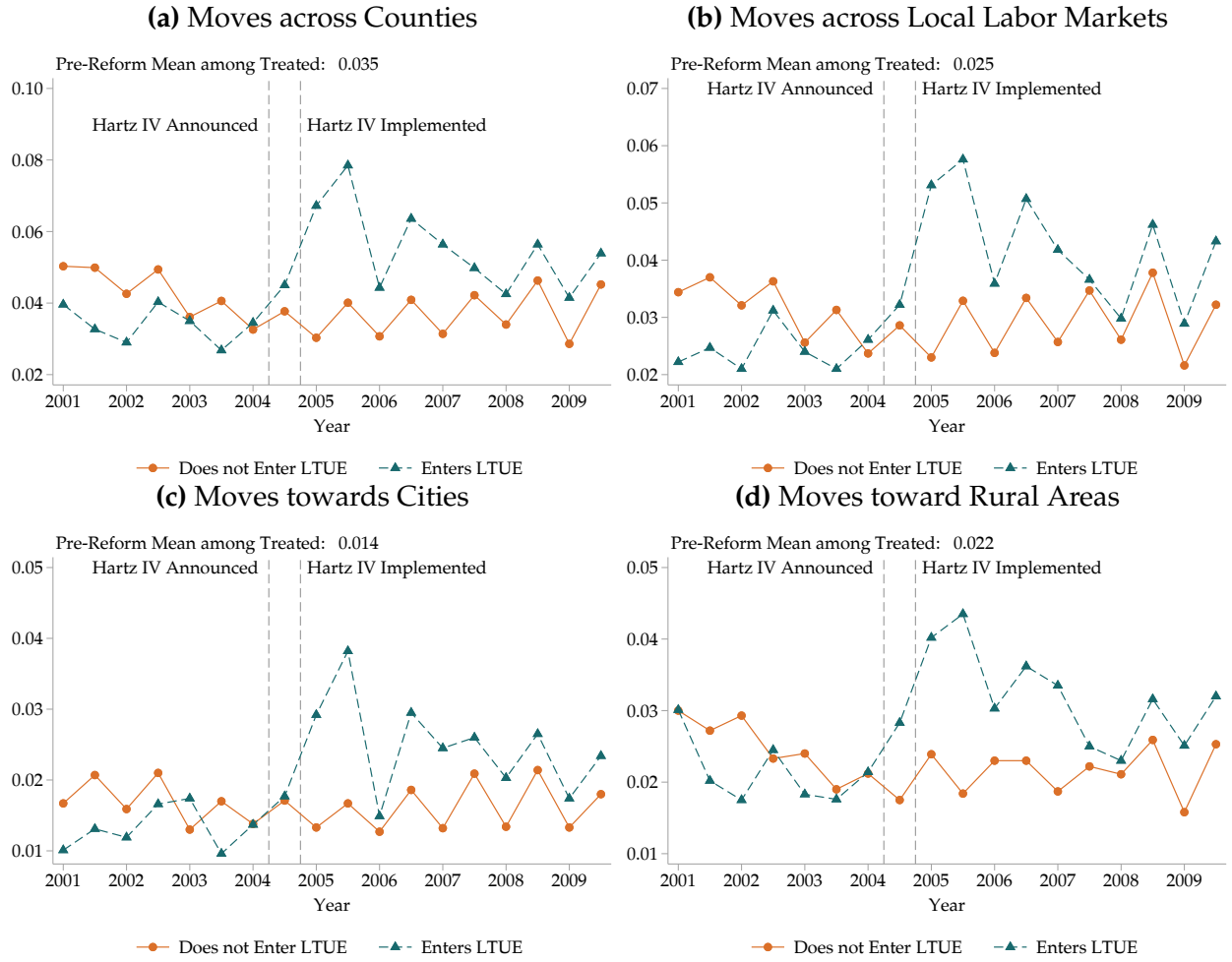
While the raw time series suggests large increases in mobility of jobseekers before entering long-term unemployment after the Hartz IV reform, which are not apparent for spells entering in re-employment, one might still be worried about a causal interpretation of these different dynamics. One particular concern could be about different selection into long-term unemployment due to the reform, which drives the observed changes in mobility. Therefore, instead of assigning treatment status based on an endogenous outcome, i.e. entering long-term unemployment, I construct the treatment using *predicted* long-term unemployment risk based on *pre-reform* data. Specifically, I use a training sample of unemployment spells in the pre-period (unemployment spells which 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 set of sociodemographic worker characteristics, information on the previous job and data on the worker's employment biography preceding unemployment. In Appendix D, I enumerate the full set of variables and show the predictive performance of my most saturated model. I then collapse the continuous long-term unemployment risk measure into a binary variable as follows:

$$\text{Treated}_{it}^{\text{Risk}} = \begin{cases} 1 & \text{if spell } i \text{ is in the top } P \text{ percentile of LTUE risk} \\ & \text{and terminates UI recipience in semester } t \\ 0 & \text{if spell } i \text{ is in the bottom } P \text{ percentile of LTUE risk} \\ & \text{and terminates UI recipience in semester } t \end{cases} \quad (4)$$

¹⁰Results when using being at least six months away from exhaustion resemble those with three months closely.

¹¹Counties are the most fine-grained geographic units for which I can detect moves, of which there are 401 in Germany.

Figure 7: Time Series of Moving Rates for Unemployment Insurance Spells Entering Long-Term Unemployment and Not Entering Long-Term Unemployment



Notes: This figure shows the estimated δ_k coefficients from Equation (5). The treatment group in quarter t consists of workers, who start claiming LTUE benefits in quarter t , after having exhausted their initial UI stream. The control group consists of UI recipients at the beginning of their spell in quarter t , who won't exhaust benefits in the current unemployment spell. Capped spikes denote 95 percent confidence intervals.

In my baseline results using predicted long-term unemployment risk, I set P approximately to 16.6%, which corresponds to the fraction of spells that enter long-term unemployment during the pre-reform period. I later vary P to examine how results change when expanding or restricting the share of spells in the treated group. 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.

Baseline Difference-in-Differences Design Formally, my baseline dynamic difference-in-difference estimation (DiD) equation is:

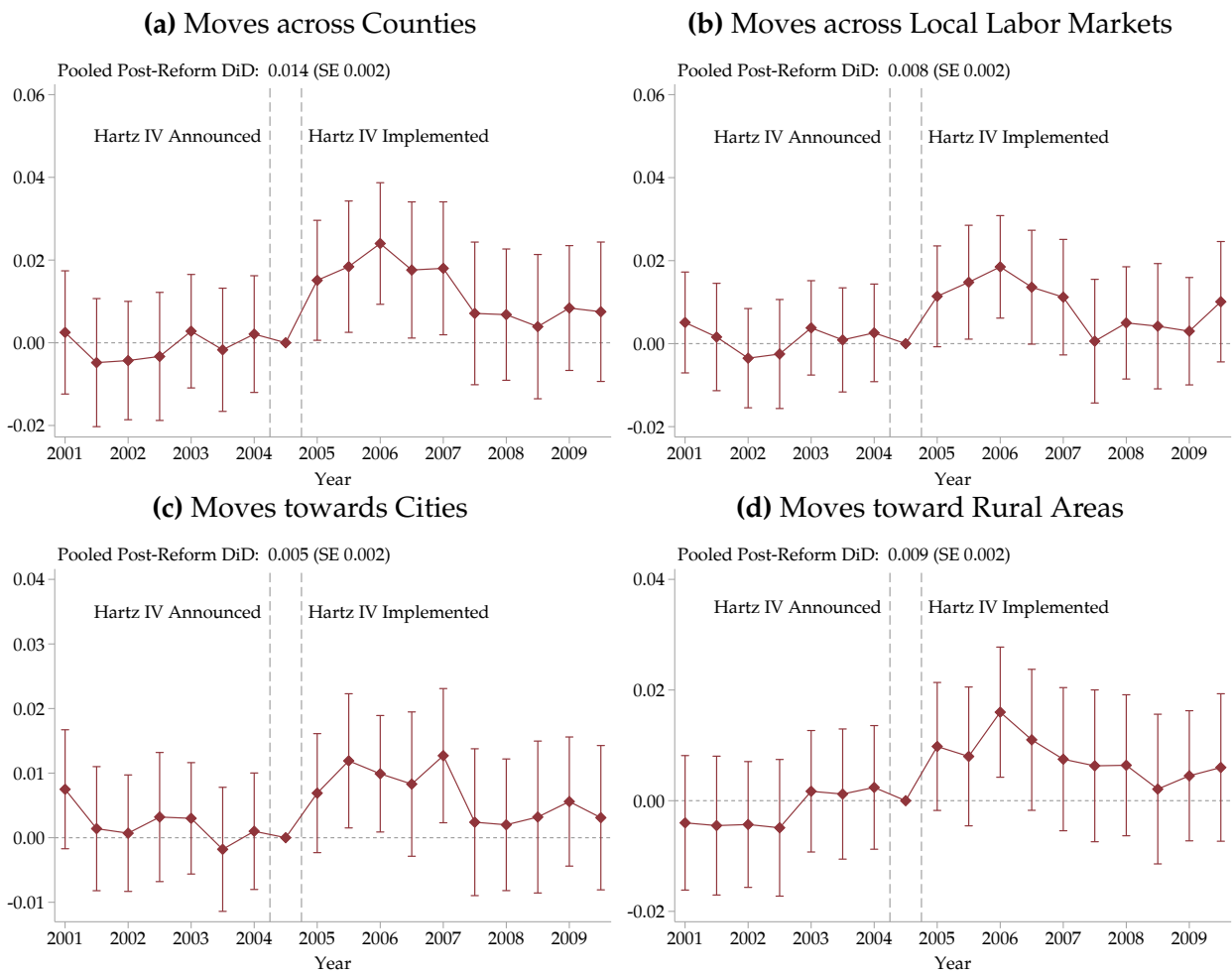
$$y_{it} = \beta_0 + \sum_{\substack{s=s1 \ 2001 \\ s \neq s2 \ 2004}}^{s2 \ 2009} \delta_s \cdot (\text{Treated}_{it}^{\text{Risk}} \times \mathbf{1}\{s = t\}) + \beta_1 \text{Treated}_{it}^{\text{Risk}} + \tau_t + \epsilon_{it}, \quad (5)$$

where t refers to semesters (January to June, July to December) and i indexes unemployment spells. The choice of semesters is guided by the trade-off between zooming in on treatment dynamics in the immediate months around reform announcement and implementation and having sufficiently large treatment and control groups for each t . δ_s identifies the difference

in outcomes between spells with high and low long-term unemployment risk, relative to the second semester (July to December) of 2004. Since the reform was publicly announced in July 2004 (see 2.1), any anticipation effects before reform implementation should result in changes in mobility from the first to the second half of 2004. Standard errors are clustered at the worker level.

Mobility Results Using Predicted Unemployment Risk Figure 8 shows the estimated δ_s coefficients from Equation (5) for my four main moving outcomes. Each panel also includes the estimated pooled post-reform DiD effect, where I consider all spells with long-term unemployment risk above P (here $P = 16.6$) from January 2005 onward as treated, using the same estimation sample as in (5).

Figure 8: Dynamic DiD Effects for Long-Term Unemployment Entrants



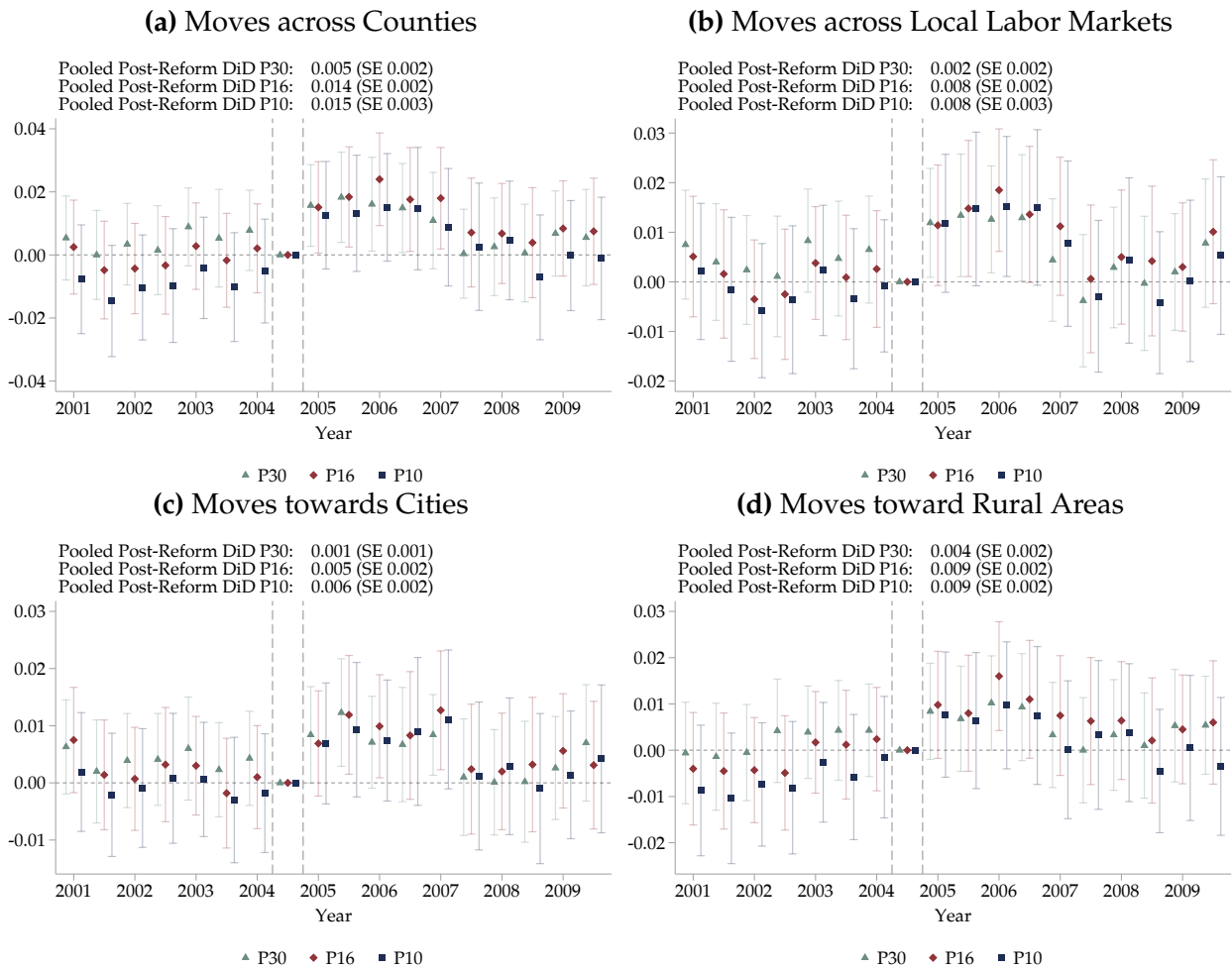
Notes: This figure shows the estimated δ_s coefficients from Equation (5). The treatment group in semester t consists of unemployment spells, which are in the top sextile ($\approx 16.6\%$) of the long-term unemployment risk distribution and terminate UI recipience in semester t . The control group consists of unemployment spells, which are at in the bottom sextile ($\approx 16.6\%$) of the long-term unemployment risk distribution and terminate UI recipience in semester t . Capped spikes denote 95 percent confidence intervals.

For all moving outcomes, there is an immediate and significant increase among spells with high long-term unemployment risk, relative to spells with a low predicted probability of transitioning into long-term unemployment. The dynamic DiD-estimates remain positive throughout the sample period, but are not statistically distinguishable from zero (mostly) from the second half of 2007 onward, which lines up with the dynamics depicted in Figure 7. The effect magnitudes are sizeable, corresponding to a 32-55% increase relative to

the pre-reform moving rates in the treatment group according to the pooled DiD estimates. Although these differences are not statistically significant, the relative increase is largest for moves towards cities (55% in the pooled DiD estimate, see Figure 8c).

Robustness Using Different Percentiles of Unemployment Risk For robustness, I also vary the percentile cutoff P , which assigns spells to the treatment or control group according to Equation (4).

Figure 9: Dynamic DiD Effects for Different Percentiles of Unemployment Risk

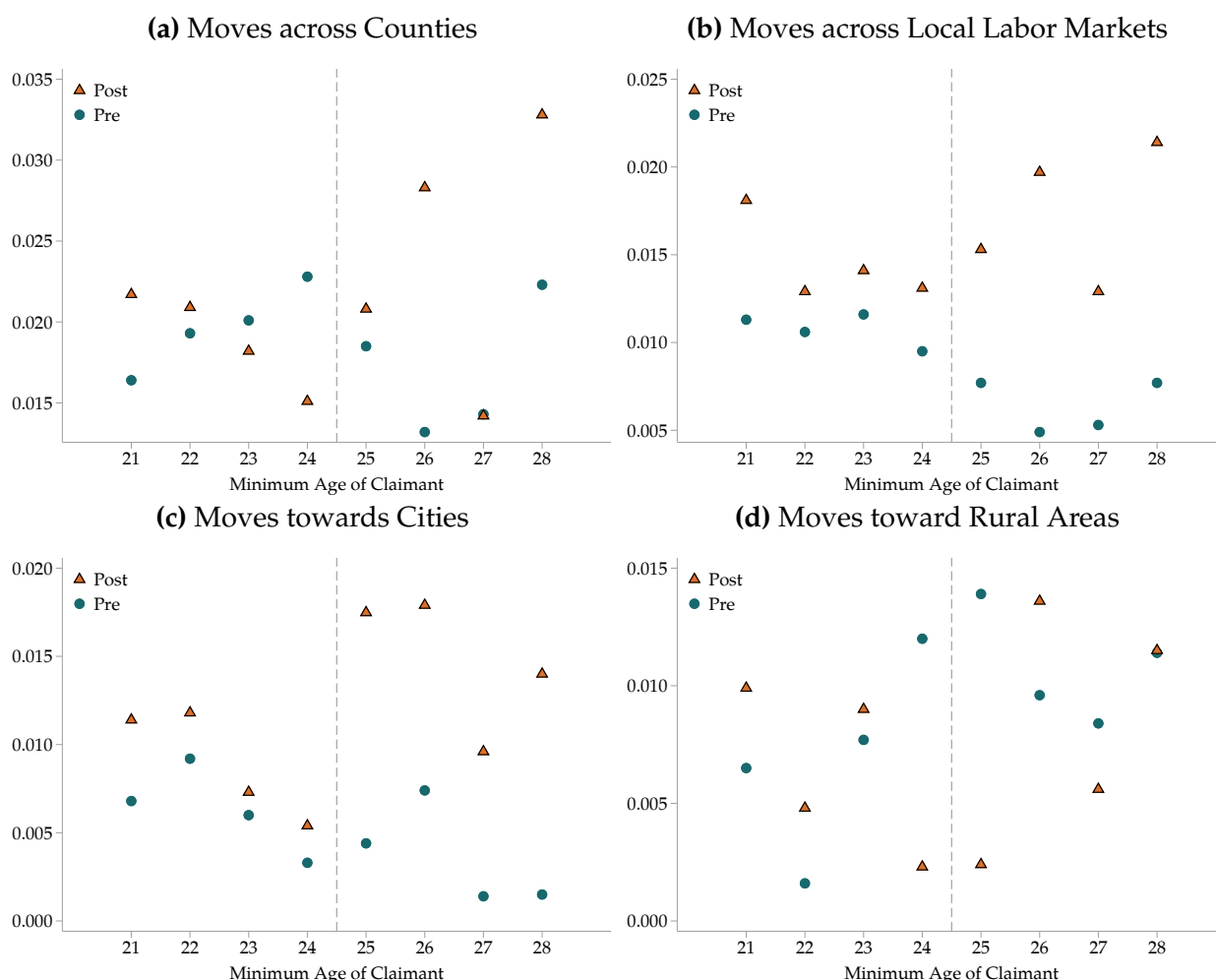


Notes: This figure shows the estimated δ_s coefficients from Equation (5). The treatment group in semester t consists of unemployment spells, which are in the top 30, 16 or 10th percentile of the long-term unemployment risk distribution and terminate UI recipience in semester t . The control group consists of unemployment spells, which are at in the bottom 30, 16 or 10th percentile of the long-term unemployment risk distribution and terminate UI recipience in semester t . Capped spikes denote 95 percent confidence intervals.

Figure 9 shows the corresponding results when considering the top 30th, 16th or 10th percentile of long-term unemployment risk as treated. Across the four moving outcomes, the dynamics around the reform evolve similar for the three cutoffs. However, the pooled DiD-estimates indicate that the reform's effect on worker mobility during UI becomes more mitigated once I include additional spells with lower long-term unemployment risk than in the original cutoff ($P^{\text{orig}} = 16.6$).

Triple DiD with Age Cutoff for Young Workers To provide further evidence in favor of Hartz IV affecting the mobility of new entrants into long-term unemployment, I exploit an institutional age cutoff, which discouraged jobseekers below 25 years to move before entering ALG 2. As outlined briefly in Section 2.3, jobseekers below 25, who moved to a flat outside of their parental home, would not be eligible for housing assistance until they turn 25, even if that move occurred *before* claiming long-term unemployment benefits (§22 Paragraph 5, SGB II). 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. Figure 10 shows moving rates of entrants by age of the claimant, separately for pooled entry cohorts before and after the reform. Since I only observe year of birth, a subset of claimants labeled as 24 might have already turned 25 when claiming long-term unemployment benefits.

Figure 10: Average Moving Rates among Entrants into Long-Term Unemployment by Age of the Claimant



Notes: This figure shows average moving rates among LTUE entrants by age of the claimant, separately for claimants who entered before (2001-2004) or after (2005-2009) the Hartz IV reform.

Across the different mobility outcomes except of moves towards rural areas, mobility among new entrants is only significantly higher among claimants aged at least 25. Most strikingly, there is a clear discontinuity in the probability to move towards cities between entering claimants aged 24 and 25, consistent with the regulatory limits on mobility of claimants below 25.¹² Overall, Figure 10 provides graphical evidence in favor of eligibility for hous-

¹²Several reasons could explain, why entering claimants below 25 have positive mobility rates as well, in

ing assistance being crucial in determining whether new long-term unemployment benefit claimants moved. I will now exploit this institutional age cutoff in a triple DiD design. I first compare differences in mobility between UI spells that enter long-term unemployment to UI spells that end in re-employment at least three months before exhaustion, similar to the time series in Figure 7, before and after the reform. As the third difference, I additionally distinguish between claimants below 25 and above. For my estimates to be biased in the third difference, there would need to be different selection into long-term unemployment after the reform between claimants below and above 25 years. I restrict my estimation sample to the age groups 21 to 28, thereby mimicking the comparison in Figure 10.

Table 1: Triple DiD Results

	Moves			
	(1) across Counties	(2) across LLMs	(3) to Cities	(4) to Rural Areas
LTUE Entrants	0.004*	-0.001	0.001	-0.004**
	(0.002)	(0.001)	(0.001)	(0.002)
Post	-0.002**	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
LTUE Entrants \times Post	-0.005*	-0.001	0.000	0.000
	(0.003)	(0.000)	(0.002)	(0.003)
LTUE Entrants \times Post \times Age \geq 25	0.012***	0.010***	0.009***	0.001
	(0.004)	(0.003)	(0.003)	(0.004)
Mean DV (Treated, Pre-Period)	0.019	0.009	0.006	0.008
Controls	Yes	Yes	Yes	Yes
Observations	202861	202842	130696	72190
Treated Observations	19897	19897	11808	8001
Year Range	2001-2009	2001-2009	2001-2008	2001-2009

Notes: The table reports results of a triple DiD specification. In addition to comparing LTUE entrants to UI claimants, before and after the reform (see Equation (5)), I add the distinction between claimants below and above 25 years. The estimation sample is restricted to jobseekers aged 21 to 28. Standard errors are clustered at the worker level. Abbreviations used: long-term unemployed (LTUE), unemployment insurance (UI). Asterisks indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1 reports results for the static triple DiD specification. Except for moves towards rural areas, long-term unemployed entrants of at least 25 years show significant increases in mobility after the reform, compared to the corresponding mobility among UI claimants in the control group. The effect magnitudes are sizeable, corresponding to around 60 to 150% of the pre-reform mean among long-term unemployment entrants. Moreover, there is no increase in mobility among entrants younger than 25 years, consistent with institutional restrictions to mobility in this group.

spite of potentially not being eligible for housing assistance. First, my moving variables do not allow me to distinguish moves (back) into the parental home, which claimants below 25 were encouraged to do, from other moves. Second, in the presence of serious moving motives (e.g. domestic violence), housing assistance was also granted to claimants below 25.

4.2.2 Mobility During Long-Term Unemployment

For completeness, I also examine mobility responses of long-term unemployed jobseekers after entry, i.e. *during* long-term unemployment. Since the residency variable is usually only updated once a year for most UA recipients before the reform, I conduct this empirical exercise using data at annual frequency. Specifically, I estimate

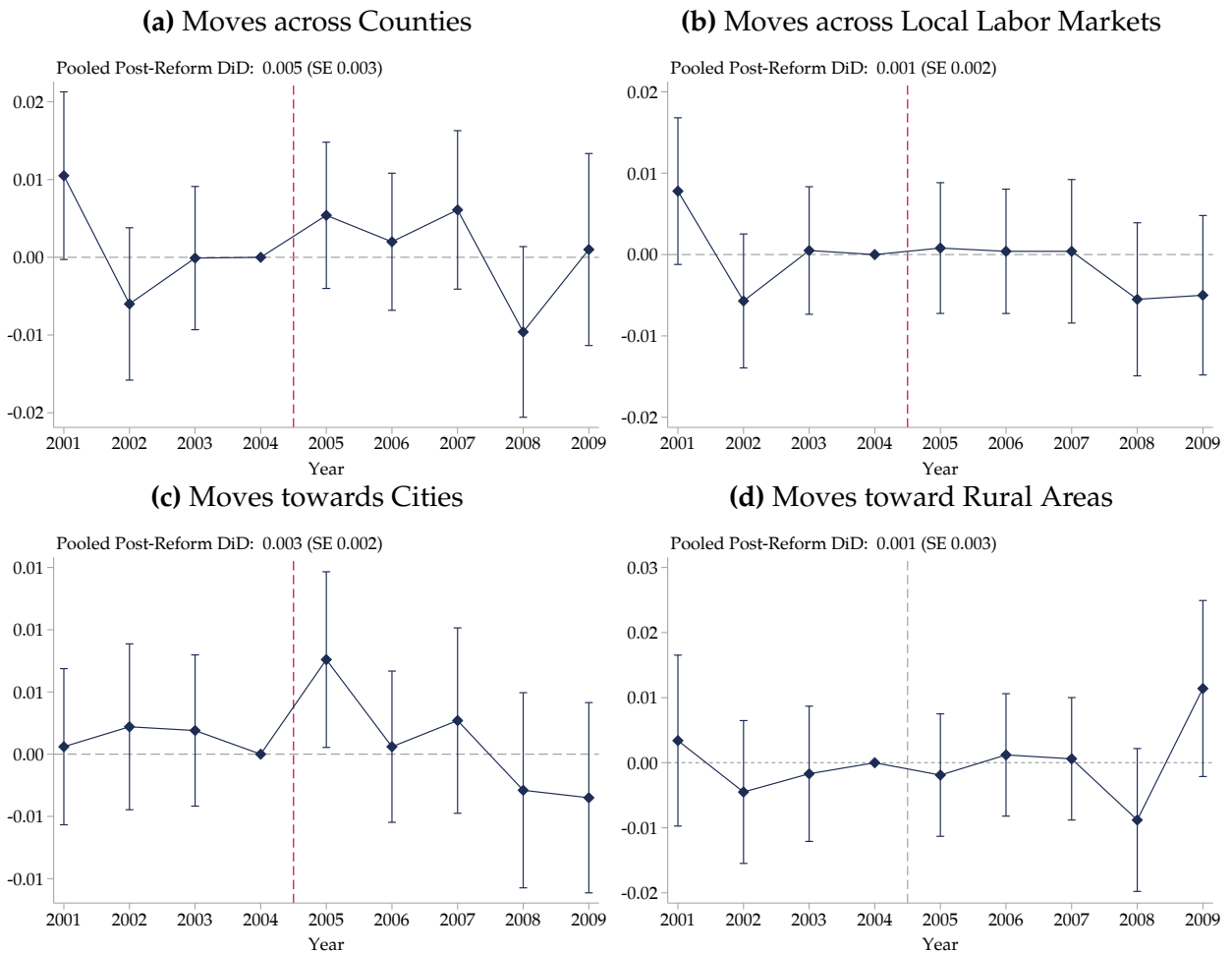
$$y_{it} = \beta_0 + \sum_{\substack{k=2001 \\ k \neq 2004}}^{2009} \delta_k (\text{Treated}_{it} \times \mathbf{1}\{k = t\}) + \beta_1 \text{Treated}_{it} + X'_{it} \gamma + \tau_t + \epsilon_{it} \quad (6)$$

where now treatment is defined as:

$$\text{Treated}_{it} = \begin{cases} 1 & \text{if worker } i \text{ is long-term unemployed in year } t \text{ and } t - 1 \\ 0 & \text{if worker } i \text{ entered UI in year } t - 1 \text{ and terminated UI} \\ & \text{at least three months before exhaustion through re-employment} \end{cases} \quad (7)$$

Figure 11 shows the corresponding event study graphs.

Figure 11: Dynamic DiD Effects for Long-Term Unemployment ‘Stayers’



Notes: The figure shows δ_k coefficients from Equation (6). The treatment group consists of long-term unemployed workers, who were already long-term unemployed in the year before (‘stayers’). The control group consists of UI recipients at the beginning of their spell in quarter t , who won’t exhaust benefits in the current unemployment spell. Capped spikes denote 95 percent confidence intervals.

Among ‘stayers’ in long-term unemployment, the reform does not appear to have increased mobility. Only moves towards cities show an increase in the months around reform imple-

mentation. The absence of an effect on mobility among non-entering long-term unemployed jobseekers can be rationalized by the strict conditions that ALG 2-receiving movers needed to satisfy to remain eligible for housing assistance. Together with my triple DiD results on young jobseekers, these findings suggest that eligibility for housing assistance in the destination region seemed to be crucial for jobseekers to decide to move.

5 Job-Finding Rates

I next turn to study the effects of regional long-term unemployment benefits on job-finding. I begin by examining whether the lower long-term unemployment benefits affected job-finding rates among unemployed workers in general, and whether this effect varies across regions as reform-induced benefit cuts vary across regions.

5.1 Effects of Hartz IV on Job-Finding

Price (2019) finds that the reform affected job-finding rates of unemployed workers, particularly in the months around UI benefit exhaustion. I closely follow his identification strategy, but use a slightly different dataset. Nevertheless, this subsection is mostly replicating findings from Price (2019), before examining a novel heterogeneity dimension of the reform - whether the effects vary by residency location of the claimant.

Identification Idea The new ALG 2 benefits applied to all UI exhaustees after January 2005 and to long-term unemployed workers, who had exhausted before January 2005 and consequently received UA. I can therefore not exploit quasi-random variation in the timing of exhausting UI benefits in a small window around January 2005. Instead, following Price (2019), my identification strategy uses two sources of variation. First, I compare unemployed workers, who would exhaust before January 2005 ("pre-cohorts") with those who would exhaust from January 2005 onward ("post-cohorts"). Given that ALG 2 benefits were on average lower than UA benefits under the pre-reform scheme, I would expect higher job-finding rates for the post-cohorts. Note that the pre-cohorts include unemployed workers, who would exhaust under the old scheme, but in months at which the reform had already been announced.¹³ To the extent that these workers in the "control" cohorts already increase their job-finding in anticipation of the subsequent reform, my estimates will capture a lower bound of the true effect.

However, the comparison between potential exhaustees under the old versus the new scheme is likely confounded by differences in labor market conditions as I am effectively comparing job-finding rates of workers across different years. Hence, I additionally compare workers within cohorts (pre, post), who are differently close to UI benefit exhaustion. In line with a standard job-search model, one would expect the reform-induced benefits cuts to matter particularly when workers approach UI benefit exhaustion. To ensure that this comparison is not only driven by differences in completed unemployment duration, I control non-parametrically for unemployment duration. The residual variation in months to UI exhaustion is therefore coming from differences in age and previous years worked across workers, who have been unemployed for the same time. Appendix Figure B4 illustrates my sources of variation.

Formal Set-Up I use my sample of unemployment entry cohorts between 2000 and 2006 at monthly frequency, as described in Section 3.2. Whereas all unemployed workers in cohorts 2000 and 2001 would exhaust under the old rules, the share of potential post-reform exhaustees increases for cohorts 2002 to 2004 and eventually reaches one for the cohorts 2005 and 2006, i.e. where all unemployed workers would certainly exhaust into ALG 2.

¹³For example, a potential exhaustee in October 2004 would have known that after two months of UA benefits, she would transfer to the less generous ALG 2 scheme.

Using this sample, I estimate a discrete-time duration model at monthly frequency using the complementary log-log link, similar to [Price \(2019\)](#) and previous work on the effects of benefit cuts on job-finding ([Meyer, 1990](#)). In particular, I regress the job-finding rate of unemployed worker i with completed unemployment duration d (in months) in quarter-year t

$$\lambda_{idt} = \mathbb{P}[D_i = d | D_i > d - 1] \quad (8)$$

on

$$\lambda_{idt} = 1 - \exp(-\exp(\mathbf{x}'_{idt}\boldsymbol{\beta})) \quad (9)$$

$$\mathbf{x}'_{idt}\boldsymbol{\beta} = \alpha_d + \tau_t + \mathbf{z}'_{id}\boldsymbol{\gamma} + \sum_{k=-9}^{4+} \delta_k^Y \mathbf{1}\{\text{m to UI exhaustion}_{id} = k \cap \text{Cohort} = Y\} \quad (10)$$

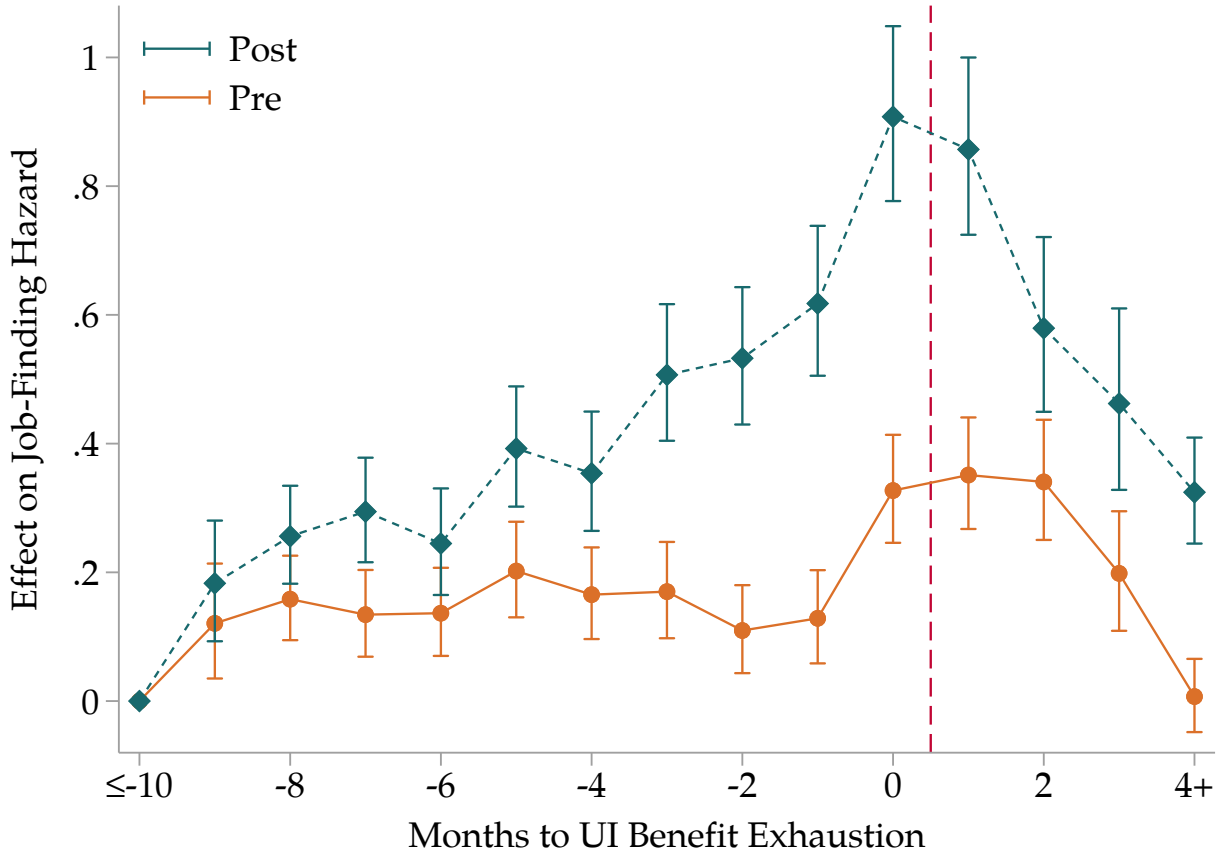
where α_d are monthly unemployment duration indicators, τ_t quarter-year fixed effects, and \mathbf{z}_{id} is a vector of worker characteristics. Jobs refer to "regular jobs", i.e. jobs that are subject to social security contributions. The coefficients of interest are δ_k^Y , which capture the effect of being $-k$ months away from UI exhaustion for a worker in cohort Y (pre, post), relative to being ten or more months away from exhaustion (the omitted category).¹⁴ As described in the previous paragraph, the identifying variation for estimating δ_k^Y comes from differences in potential unemployment duration across workers, who have been equally long unemployed and who search in the same quarter-year. To ensure that only small differences in observable differences generate the identifying variation for δ_k , I include age- and previous years worked-bins in \mathbf{z}_{id} . Effectively, most of the identifying variation is therefore coming from the comparison of workers with similar age, but who happen to be on different sides of the age- and previous working years-cutoff, which determine PBD of unemployment insurance. Additional worker controls include sex, education, migration background and whether a worker resided in East or West Germany when entering unemployment. As one set of robustness checks, I will show that none of my results is sensitive towards including the control vector \mathbf{z}_{id} .

Results Figure 12 shows the estimated job-finding profiles in the months around UI benefit exhaustion. The green line depicts the estimated δ_k coefficients for (potential) exhaustees before the reform, and the blue line for post-reform cohort. k is coded such that if workers do not find a job in month zero, they would enter long-term unemployment.

Figure 12 suggests that unemployed workers who would exhaust into UA (green profile) have a relative flat profile and only a small spike when approaching UI exhaustion. The point estimates imply that in the last month before exhaustion, the job-finding hazard is only 20% higher than when being ten or more months away from exhaustion. In contrast, potential exhaustees into ALG 2 (blue profile) show an increasing job-finding hazard towards benefit exhaustion with a clear spike in the last month before exhaustion. Both profiles decline rapidly after exhaustion, which is consistent with higher job-search effort in the immediate months around exhaustion, which subsequently falls again, as in job-search models with reference dependence ([DellaVigna et al., 2021](#)). In line with the findings from [Price \(2019\)](#), my results suggest that jobseekers who faced lower ALG 2 benefits responded

¹⁴My notation implies that δ_{-9}^{pre} captures the effect of a potential exhaustee under the pre-reform rules, who is nine months away from benefit exhaustion, relative to a (observationally similar) pre-reform exhaustee, who is still ten or more months away from exhaustion.

Figure 12: Relative Job-Finding Hazards among Pre- and Post-Reform Unemployment Cohorts



Notes: This figure shows the estimated δ_k coefficients from Equation (10). The Post coefficients refer to the job-finding profiles among cohorts, who would exhaust their UI benefits under the post-reform rules, i.e. in January 2005 or later. The pre coefficients refer to the job-finding profiles, who would exhaust before January 2005. Capped spikes denote 95 percent confidence intervals.

by finding regular jobs, particularly in the immediate months before they would have to rely on ALG 2 benefits.

5.2 Effects of Harz IV on Job-Finding by Location

Next, I investigate whether the reform affected job-finding rates differently across locations. Because of the housing assistance in ALG 2, benefit cuts in long-term unemployment benefits were likely lower in urban areas (see Figure 5). Therefore, one would expect the reform to have lower bite in urban (high-rent), relative to rural (low-rent) areas. I test this hypothesis formally by running similar duration models as in Subsection 5.1, but I allow the cohort-specific profiles to vary by the *residency location* at UI entry of the claimant. More specifically, I estimate

$$\lambda_{id} = 1 - \exp(-\exp(\mathbf{x}_{idt}'\boldsymbol{\beta}^r)) \quad (11)$$

$$\mathbf{x}_{idt}'\boldsymbol{\beta}^r = \alpha_d + \tau_t + \mathbf{z}_{idt}'\boldsymbol{\gamma} + \sum_{k=-9}^{4+} \delta_k^{Y,r} \mathbf{1}\{m \text{ to UI exhaustion}_{id} \cap \text{Cohort} = Y \cap \text{Location} = r\} \quad (12)$$

which yields separate job-finding profiles for each combination of $\{\text{pre, post}\} \times \{\text{large city, rural}\}$, and therefore enables me to assess whether the pre- to post-reform differences vary by

the residency location of the jobseeker. Importantly, the vector of worker characteristics z_{idt} includes previous wage decile fixed effect, which ensures that the differences in job-finding profiles ($\delta_k^{Y,r}$) between urban and rural areas are not capturing differences in previous wages.¹⁵

Finally, I want to rule out that differences in job-finding profiles across residency locations are driven by geographic differences in labor demand. One concern could be that firms in larger (urban) labor markets can select from a larger pool of jobseekers to fill their vacancies, which could make them less likely to hire the arguably more negatively selected set of unemployed workers at later stages of their unemployment spell. While I do control for the completed unemployment duration, Equation (12) might not capture that firms in cities have a particular distaste for hiring (close to) long-term unemployed workers. A smaller spike in the post-reform job-finding hazard in urban areas might therefore not only capture lower incentives to find a job (as I argue), but also lower labor demand from firms in cities for this particular set of unemployed workers. To address this concern, I re-estimate a discrete duration model with regressors as in Equation (12), but I now assign successful jobseekers to rural/urban based on their reemployment *workplace* location instead of their *residency* location as before. Jobseekers, who did not find a job continue to be assigned based on their residency location.¹⁶

Results by Residency Location Figure 13 shows the job-finding profiles for the pre- and post-reform cohorts by residency location.

Panels i. and ii. depict that while post-reform cohorts in urban areas have a higher re-employment hazard around benefit exhaustion than the pre-cohorts (panel i.), the difference is substantially larger for unemployed workers in rural areas (panel ii.). This contrast becomes most evident in the month immediately preceding benefit exhaustion ($k = 0$), where post-reform jobseekers in large cities have a 29 percentage point higher hazard relative to their pre-reform counterpart, compared to a 68 percentage point difference in rural areas ($p < 0.01$).

Results by Workplace Location I next turn to the results, where jobseekers are assigned to large cities or rural areas based on the location of their reemployment job.

Panels i. and ii. in Figure 14 depict the pre- to post-reform differences in job-finding profiles for jobseekers, who found jobs in very urban and rural locations, respectively.¹⁷ In contrast to the profiles based on residency location, the pre- to post-reform differences are of similar magnitude for rural and very urban workplace locations. This convergence is particularly driven by a higher job-finding hazard for jobs in large cities around UI benefit exhaustion. The results by workplace locations therefore suggest that the differences by residency locations are not merely driven by differences in labor demand. Instead, firms seem to have hired workers around benefit exhaustion similarly, regardless of whether workplaces were located in larger or smaller local labor markets.

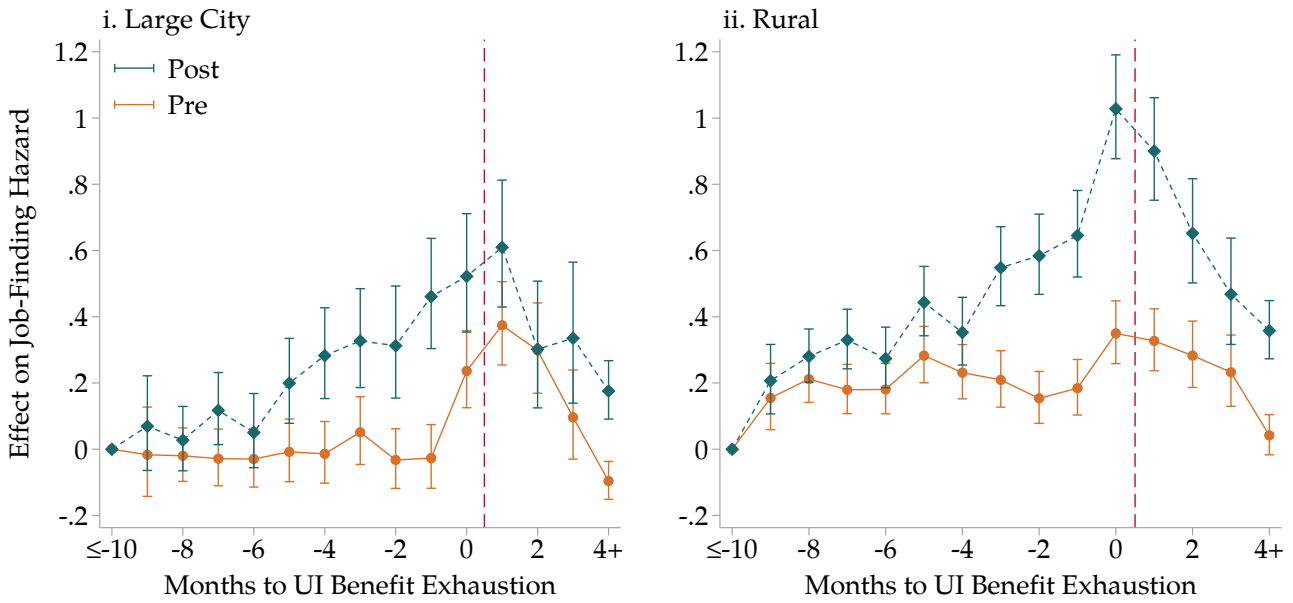
Summary and Interpretation In summary, my results on job-finding by location suggest

¹⁵Given that wages are positively correlated with city size, previous wages among potential UI exhaustees might also be higher in cities, which would increase the reform-induced benefit cuts in urban areas. However, Figure INSERT HERE shows that *among exhaustees*, the difference in previous wages between urban and rural areas is small. As one robustness check, I also estimate Equation (12) without including previous wages as a control variable.

¹⁶Around 75% of the unemployment spells in my estimation sample end with re-employment.

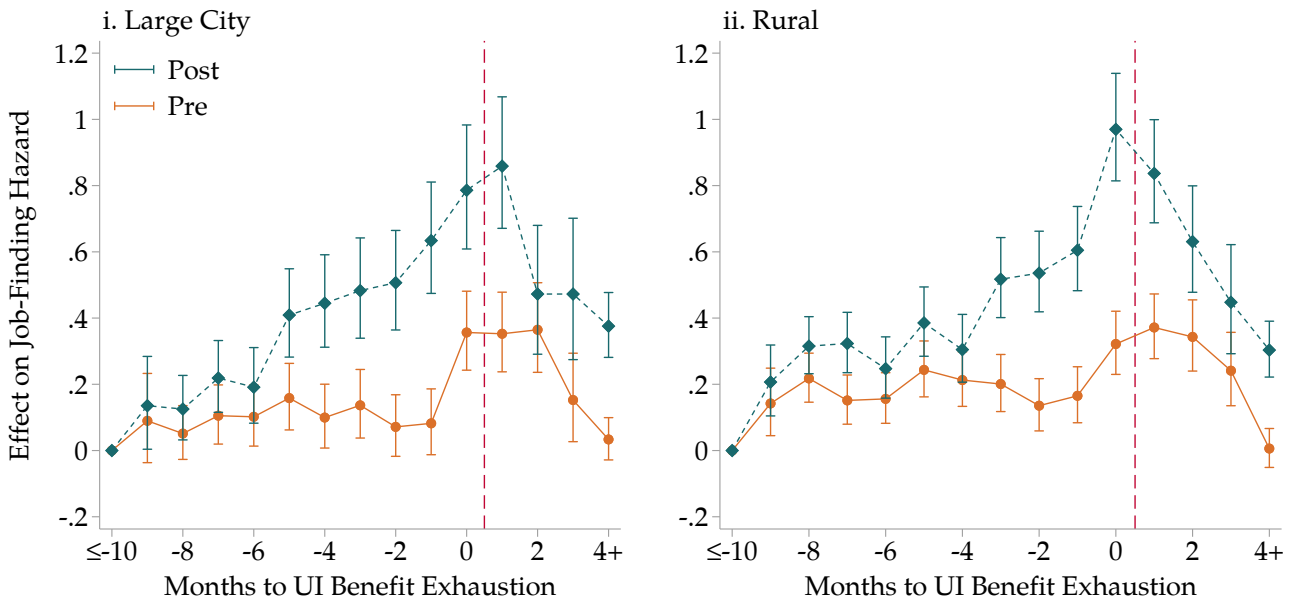
¹⁷For the jobseekers, who found a job. Unsuccessful jobseekers are still assigned based on their residency location at UI entry.

Figure 13: Relative Job-Finding Hazards among Pre- and Post-Reform Unemployment Cohorts by Residency Location at UI Entry



Notes: This figure shows the estimated δ_k coefficients from Equation (12), separately for workers who live in a large city (county with city with $\geq 100,000$ inhabitants) or outside a large city ("rural") at entry into unemployment. The Post coefficients refer to the job-finding profiles among cohorts, who would exhaust their UI benefits under the post-reform rules, i.e. in January 2005 or later. The pre coefficients refer to the job-finding profiles, who would exhaust before January 2005. Capped spikes denote 95 percent confidence intervals.

Figure 14: Relative Job-Finding Hazards among Pre- and Post-Reform Unemployment Cohorts by Workplace Location



Notes: This figure shows the estimated δ_k coefficients from Equation (12), separately for workers who find a job in a large city (county with city with $\geq 100,000$ inhabitants) or outside a large city ("rural") at entry into unemployment. Workers, who don't find a job, are still assigned based on their residency location at entry into unemployment. The Post coefficients refer to the job-finding profiles among cohorts, who would exhaust their UI benefits under the post-reform rules, i.e. in January 2005 or later. The pre coefficients refer to the job-finding profiles, who would exhaust before January 2005. Capped spikes denote 95 percent confidence intervals.

that the reform led to a larger spike in re-employment hazards for jobseekers residing (at UI entry) in rural locations, compared to those living in large cities. However, the jobs that jobseekers found in the months around exhaustion are almost equally likely to be located

in cities and rural areas. These two findings together indicate that either rural jobseekers at unemployment entry moved to cities during their unemployment spell, or that rural jobseekers stayed in their location, but found jobs in the nearby city. Given that I assign workers based on their residency county and not on their commuting zone, a substantial fraction of jobseekers with a rural residency location have access to a large city within their commuting zone (see Appendix Figure B2).

To disentangle between these two mechanisms, I conduct two additional analysis. First, I assign workers to large cities or rural regions based on their contemporaneous residency location when finding a job. If a large share of rural jobseekers at unemployment entry moved to large cities and found jobs there, the pre- to post-reform differences should be of similar magnitude for large cities and rural areas in this specification. Second, I assign jobseekers to large cities and rural regions based on their residency commuting zone at UI entry, as opposed to their residency county. If reform-induced changes in the re-employment hazard look similar between jobseekers in urban and rural commuting zones, this would be consistent with "rural" jobseekers finding jobs in large cities within their commuting zone. The corresponding results (currently being exported) suggest that the difference between Figure 13 and 14 is mostly driven by jobseekers, who live in rural locations and found jobs in the nearby city.

6 Conclusion and Next Steps

In this paper, I have studied the effects of regional (long-term) unemployment benefits on jobseekers' location choices and job-finding, using quasi-experimental variation from Germany's Hartz IV reform in 2005. My findings suggest that region-specific long-term unemployment benefits led to a relocation of unemployed workers into urban local labor markets with higher benefits. Housing assistance - the main regional component of the post-reform unemployment benefits - seemed to be crucial for jobseekers to decide to move. With respect to job-finding, my results point towards larger re-employment among unemployed workers in rural areas with smaller post-reform unemployment benefits. In contrast, while jobseekers in cities were also more likely to find jobs, introducing regional long-term unemployment benefits appears to have had less bite on this group of jobseekers.

The main next step involves examining, whether the reform-induced changes in location patterns among the long-term unemployed had large enough spillover effects to impact location decisions of other worker groups as well. While the average county (or local labor market) only experienced an inflow rate of about 0.25% of its local workforce in the peak year of migration in 2005 coming from long-term unemployment entrants who moved, these specific inflows might have been large enough to crowd-out worker groups, who were most likely competing with long-term unemployed workers in the housing and labor markets. According to some preliminary tabulations using data from the German Socio-Economic Panel (GSOEP), these were primarily young, less educated and often migrant workers. As a concrete next step, I will therefore investigate whether i) higher long-term unemployment inflow rates led to increases in rents, particularly in the lower-quality segment of the housing market, and whether ii) these inflows had crowding out effects on other, most likely competing worker groups. The results of these exercises will then guide the modeling choice to evaluate the (spatial) general equilibrium effects of the Hartz IV reform, and to evaluate alternative policies to lower regional disparities in long-term unemployment.

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I Appendix

A Additional Tables

B Additional Figures

Figure B1: Large City as Opposed to Rural Counties, with State Borders

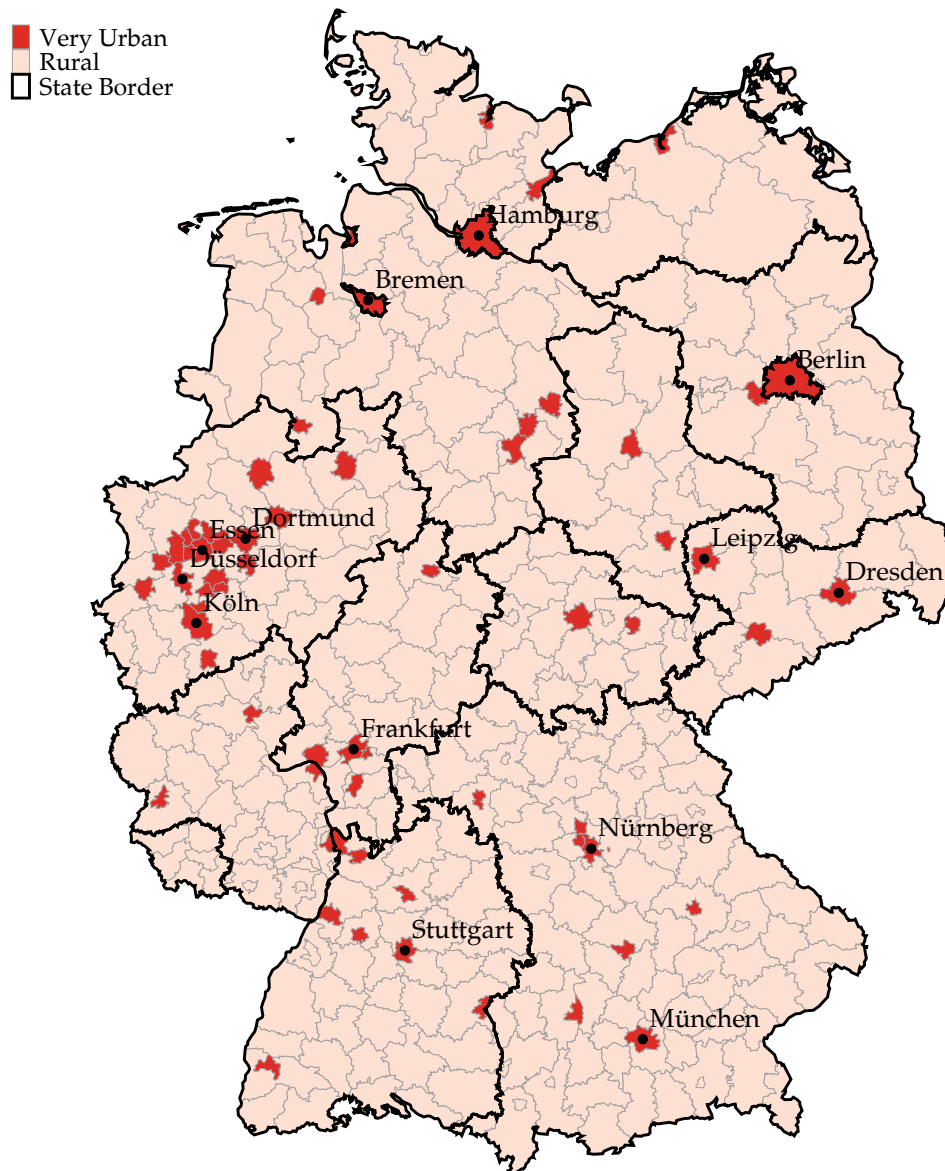


Figure B2: Large City as Opposed to Rural Counties, with Local Labor Market Borders from Kosfeld and Werner (2012)

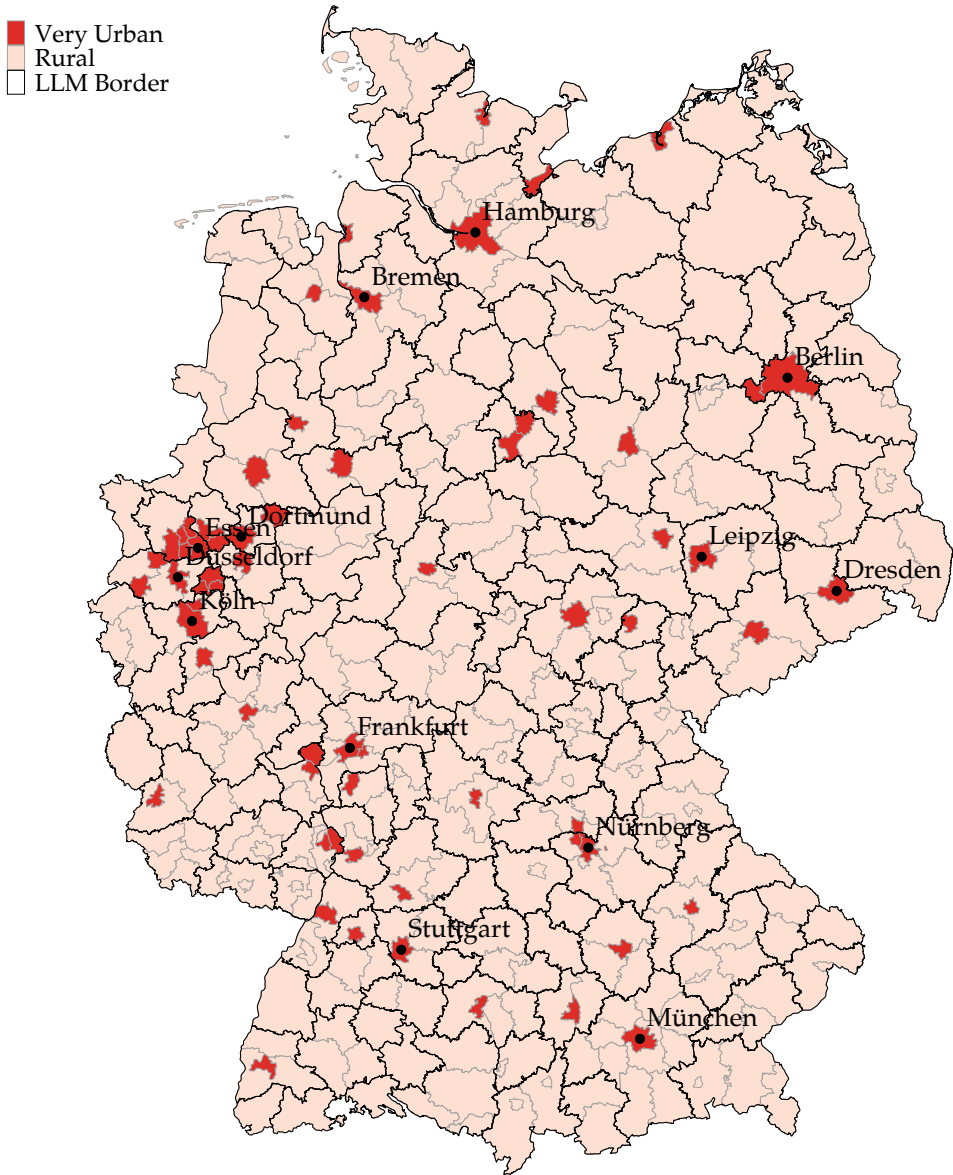


Figure B3: Moments of Average Housing and Total Benefits of ALG 2 Recipients across Counties

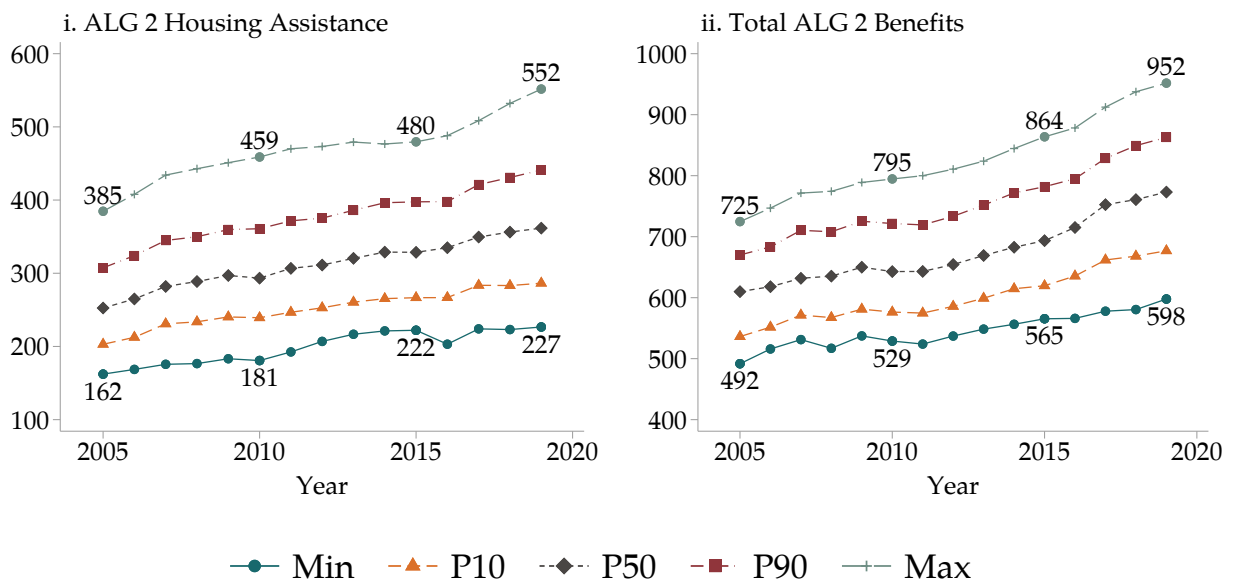
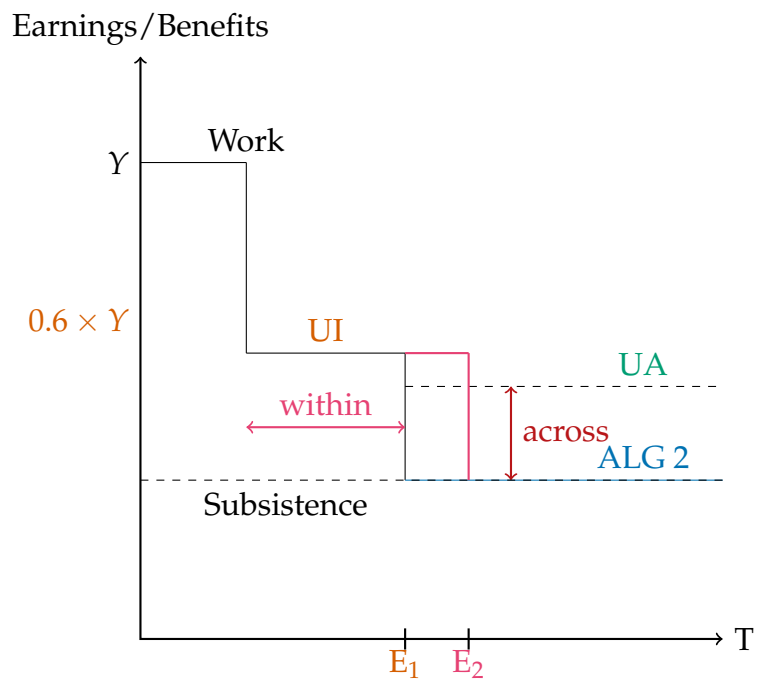


Figure B4: Identifying Variation used For Results on Job-Finding



C Mobility Restrictions for Unemployed Workers

Recipients of short-term unemployment benefits faced no restrictions in their mobility decisions, neither before nor after the Hartz IV reform. However, the responsible jobcenter asked movers to be informed about their change of residency at least one week in advance in order to potentially forward the UI claim to the jobcenter in the destination county.¹⁸ UA recipients before the reform were subject to the same set of rules when moving.

After the reform, long-term unemployment benefit recipients were subject to stricter rules when they wanted to move. Under the new benefit scheme, ALG 2 claimants would only remain eligible for housing assistance in their new flat, if the responsible jobcenter in the destination had approved the move ex-ante. Approval was typically given if the new rent was considered locally appropriate and in the presence of necessary (*erforderlich*) motives for the move, for instance inappropriately small housing or family reunions (§22 Paragraph 4, SGB II). In practice, ALG 2 recipients therefore had to inform the jobcenter about their specific destination flat and the motives for moving before changing their residency. If permission had been granted, the local jobcenter could cover moving-related expenditures such as moving costs or rent deposits (§22 Paragraph 6, SGB II). If the move had not been approved, any rent and heating expenditures above the previous ones would not be paid by the jobcenter.

ALG 2 claimants with less than 25 years of age faced even more strict rules regarding their mobility. On top of the restrictions for all ALG 2 claimants (see previous paragraph), young claimants would not receive *any* housing assistance until they turned 25, if they had moved to a new flat without ex-ante permission by the jobcenter. Moreover, the ineligibility for housing assistance could even apply, if they move had occurred before entering ALG 2 (§22 Paragraph 5, SGB II).

¹⁸If 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.

D 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):

$$\begin{aligned} \text{Spell Enters LTUE}_{it} = & \beta_0 + \text{Worker Characteristics}'_{it}\gamma \\ & + \text{Previous Job Characteristics}'_{it}\delta \\ & + \text{Previous Working History}'_{it}\eta + \epsilon_{it} \quad (13) \end{aligned}$$

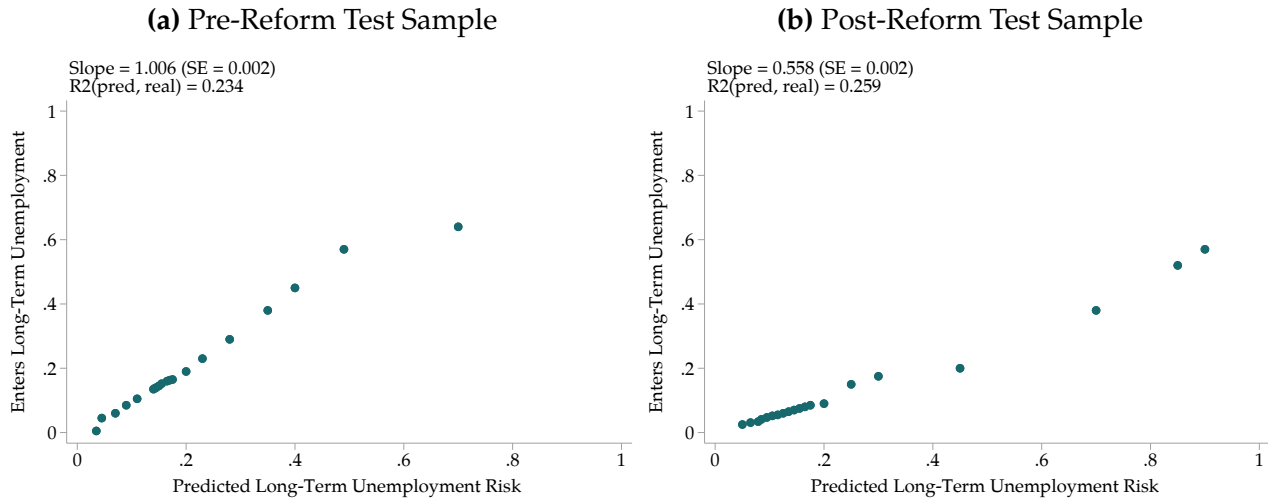
In practice, I step-wise add worker characteristics, previous job information and finally characteristics of the previous working history. Similar to [Mueller and Spinnewijn \(2023\)](#), information on the previous working history comes with the largest predictive gains, compared to the other set of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

Table D.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	Involuntary separation	

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 D5 shows the predictive performance for the pre-reform 10% test sample, and the post-reform test sample, using the logit specification.

Figure D5: Binscatter Plots of Predictive Performance



As can be seen from the panels in Figure D5, 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. Using more state-of-the-art techniques as in [Mueller and Spinnewijn \(2023\)](#) could potentially help me overcome the lack of accuracy.