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Working Paper No. 2603
This version: March 2026

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Mitigating Mobility Frictions:

The Effect of Cash-on-Hand on Labor Mobility

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March 25, 2026

Abstract

Providing recently laid off workers with cash benefits may help them overcome mobility costs and thereby stimulate labor mobility. On the other hand, cash benefits may dampen the employment shock and reduce the incentive to move. In this paper, we test these two competing mechanisms against each other. For this we use a severance pay regulation in Austria, which generated a sharp cutoff after which workers became eligible to a severance payment of two monthly salaries. Our results indicate that this cash payment increased labor mobility by around 8% to 12%. This increase is much stronger for worker groups with lower baseline mobility rates.

Keywords: Unemployment, labor mobility, internal migration, commuting

JEL classification: J18, J61, J65, R23

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Contact: bekhtiar@ihs.ac.at. We want to thank Andrea Weber, Wilbert Van der Klaauw, Ines Helm, Wolfgang Frimmel, Oliver Schlenker, Martin Huber as well as several seminar participants at the Institute for Advanced Studies (IHS), the ESPE conference 2025, the 3rd Vienna Micro Workshop 2025, the Austrian Labor Market Workshop 2025 and the University of Bratislava for useful comments. The research presented in this paper was funded by the Anniversary Funds of the Austrian National Bank (OeNB; grant number 19026).

1 Introduction

Against the backdrop of demographic change, rising skill shortages and regional mismatch unemployment, the question of how to increase workers' inter-regional labor mobility gains increasing policy relevance. One possible policy tool to increase regional mobility is to provide monetary incentives to help workers overcome mobility costs. While high-skilled workers are comparatively mobile, Bound and Holzer (2000) have shown that heterogeneous mobility costs, in particular, hamper the mobility of low wage workers, who are more restricted by these financial mobility frictions. Providing monetary incentives, thus, might be a suitable policy tool to increase the mobility of this relatively immobile group of workers. Recent work by Caliendo, Künn, and Mahlstedt (2017, 2023) has shown in this context that financial incentives are suitable to stimulate inter-regional mobility of unemployed workers.

A notable study by Notowidigdo (2020), however, offers a different perspective on this question. Notowidigdo (2020) argues that more generous cash payments – in the form of means-tested benefit programs that offer relatively higher safety nets for low-wage workers – *reduce* labor mobility after job displacement. Notowidigdo (2020) argues that this is the case, because low-wage workers face a relatively lower shock incidence after job displacement (because of means-tested benefit programs), and thus face smaller incentives to move. If this is indeed the case, then providing unemployed low-wage workers with financial means to help them overcome mobility costs might, in fact, have the opposite effect, as it ameliorates the shock incidence, and thus reduces inter-regional mobility.

In this paper we use a sharp policy discontinuity in the Austrian severance pay system to test whether cash-on-hand payments increase or decrease labor mobility. Before January 1st 2003, every laid off worker in Austria was entitled to receiving two monthly salaries worth of severance pay, *if* they have been employed for at least 36 months. This system generated a sharp discontinuity at the threshold of 36 months of tenure after which laid off workers received this severance payment.[1]

[1]Using this setting to identify the causal effect of cash-on-hand payments was pioneered by Card, Chetty, and Weber (2007). See also Ringling (2025) for long-term effects of cash-on-hand and Haller and Osterwalder (2025) for a study on repeat unemployment.

We use this sharp discontinuity in a regression discontinuity design (RDD), to estimate the causal effect of cash-on-hand payments on labor mobility of laid off workers. This treatment has the nice feature that it leads to different predictions if mobility is either hampered by heterogeneous mobility costs (as in Bound and Holzer, 2000) or moderated by heterogeneous incidence of employment shocks due to means-tested benefit programs (as in Notowidigdo, 2020). If mobility costs are what hampers the inter-regional mobility of low-wage workers, then the provision of cash-on-hand payments can help to overcome these higher mobility costs, and thereby stimulate labor mobility after job displacement. On the contrary, if labor mobility is hampered by reduced severity of employment shocks, then we would expect the severance payment to have a reducing impact on labor mobility. Therefore, the Austrian severance pay system is a well-suited laboratory to test these competing explanations against each other.

Our results show that severance payments do increase labor mobility by around 8-12%. This indicates that heterogeneous mobility costs play a much stronger role in determining labor mobility decisions (as in Bound and Holzer, 2000). While we cannot strictly rule out that the mechanism described by Notowidigdo (2020) exists as well, our results clearly indicate that any mitigating effect of cash-on-hand payments on the severity of the employment shock (and thereby also on the incentive to move) is strongly dominated by the simultaneous mitigation of financial mobility frictions. Mobility costs are therefore the more important factor to consider when aiming at increasing labor mobility of immobile worker groups.

Exploring heterogeneous effects reveals that the increase in labor mobility is driven by those workers, who show relatively low baseline mobility. Consistent with the findings in Bound and Holzer (2000) our results indicate that the positive effect on labor mobility is mostly driven by low-wage and low-skilled workers. For this group, we find substantial increases in labor mobility. Furthermore, we find increases in female labor-mobility, where the baseline mobility rates for women are also smaller compared to men. This highlights that the cash-on-hand payment increases labor mobility especially for those workers that are comparatively immobile and are likely to face higher mobility frictions.

Lastly, we also document larger mobility reactions in regions with higher unemployment rates. This echoes the early results in Blanchard and Katz (1992) which have shown that internal migration plays a crucial role in the return to equilibrium of local labor markets after local labor demand shocks. This highlights the importance of regional economic conditions for individual labor mobility decisions.

This paper contributes to the extensive literature on the determinants of labor mobility.[2] This literature has shown that workers react to employment shocks partly via internal migration, which plays an important role in a return of labor markets to economic equilibrium (Blanchard and Katz, 1992). If labor mobility is low and displaced workers stay stuck in declining regions, this is typically accommodated by high structural unemployment rates. This risk is especially important for low-wage workers who are known to be relatively immobile (Bound and Holzer, 2000, Notowidigdo, 2020). We contribute to this literature by showing that the relatively lower mobility response of low-skilled workers is primarily due to the presence of financial mobility frictions. Therefore their inter-regional mobility can be stimulated through cash payments which help to overcome these mobility costs.

Furthermore, we contribute to the literature on the effects of workers' liquidity constraints on the labor market. This literature has shown that such liquidity constraints influence labor supply decisions (Hajivassiliou and Ioannides, 2007, Rossi and Trucchi, 2016), job search behavior (Fontaine, Jensen, and Vejlin, 2024), or job quality (Browning, Crossley, and Smith, 2007). We add to this literature by showing that liquidity constraints are also an important determinant of inter-regional labor mobility.

The rest of this paper is structured as follows: Section 2 presents the institutional background of the old Austrian severance pay system. Section 3 presents the used data sources and the sample construction. Section 4 discusses our identification strategy, Section 5 presents the results of our analysis and Section 6 discusses their robustness. Lastly, Section 7 summarizes and concludes.

[2] See for example Blanchard and Katz (1992), Bound and Holzer (2000), Cadena and Kovak (2016), Huttunen, Møen, and Salvanes (2018), Neffke, Otto, and Hidalgo (2018), Monras (2018), Foote, Grosz, and Stevens (2019), Greenland, Lopresti, and McHenry (2019), Notowidigdo (2020), or Bekhtiar (2025).

2 Background: The Old Austrian Severance Pay System

Starting with January 1st 1981, most employment relations in the Austrian private sector were fully covered by a severance pay scheme. With some exceptions (like the construction sector and the public sector which were subject to different severance schemes) this law granted all laid off workers, who were employed with their current employer for at least three years, a severance payment of two monthly salaries. Importantly, only workers who were laid off by their employer (i.e., did not quit voluntarily) were eligible to receive the severance payment. This severance payment increased stepwise to a maximum possible severance pay of 12 monthly salaries after 25 years of tenure. Payments are typically made together with the last salary of the worker and are exempt from social security contributions and induce only very low income taxes. Importantly, workers who did not meet the necessary threshold of three years of current job tenure were not eligible for any severance payment. Therefore, this law created a sharp policy discontinuity at 36 months of current job tenure.

This severance pay scheme generally covered all employment contracts in the private sector that were formed between January 1st 1981 and January 1st 2003. All jobs that started after this date were subject to a new severance pay law. The new law stipulates severance payments of firms which accrue continuously over job tenure which are then contributed to a firm pension. There is no discontinuity in the new system. Workers who were covered by the old severance pay system had the option to switch to the new scheme after January 1st 2003.

3 Data and Sample Construction

Our analysis is based on administrative employment records from the Austrian Social Security Database (ASSD; see Zweimüller et al., 2009). The ASSD contains detailed information on the employment histories of all private sector employees in Austria, starting in the mid 1970s. Since the ASSD includes detailed information on the start- and end-dates of any employment relation, it allows precise measurement of the length of job tenure. Although we do not observe the severance payment itself in our data, we can thus measure which workers were eligible at the time of job termination. Since compliance with the old Austrian severance pay law is widely believed to have been nearly universal (see Card, Chetty, and Weber, 2007) this allows to use the eligibility cutoff of 36 months of current job tenure as a sharp treatment in a regression discontinuity setting.

Following Card, Chetty, and Weber (2007), we restrict our sample to job spells in the private sector, which were formed after January 1, 1981 and were terminated before January 1, 2003. We focus on the private sector to ensure that all job spells in our sample were subject to the Austrian severance pay law. Furthermore, we remove job spells in the construction sector, because construction workers were subject to a different severance scheme. We further restrict the sample to workers who were older than 20 years at the time of separation (to remove apprenticeships) and to workers who were younger than 50 years (to remove special job programs for the elderly).

In some sectors of the Austrian labor market, especially those subject to strong seasonal fluctuations (such as the tourism sector), it is very common that firms release workers during the off-season, but giving them an informal re-employment promise (Eppel and Mahringer, 2025). These laid off workers then know that they will be able to return to their prior employer, which renders their employment shock temporary and strongly removes the incentive to move. To deal with this issue, we further remove all job-spells for which we observe a return to the prior employer.

Since workers were only eligible for the severance pay if they were laid off by their employer, we also have to remove voluntary quitters from the estimation sample. While we do not observe in the ASSD whether a worker was laid off or quit voluntarily, we can use the timing of their unemployment spell to identify voluntary quitters. In Austria, a worker is blocked from receiving unemployment benefits for four weeks if they quit their job voluntarily. Therefore we remove all job-spells from the data for which we observe the start of an unemployment spell later than 28 days after the end of the job spell. We further remove individuals with non-employment durations that exceed two years to remove individuals with low labor market attachment. To remove individuals who enter early retirement, we also remove job spells who were followed by a retirement spell within the same year.

After imposing these sample restrictions (which are analogous to Card, Chetty, and Weber, 2007), we are left with roughly 2.4 million job spells for which we observe some type of information on the mobility responses after job loss. Since we apply a regression discontinuity design for the estimation of the effect of the severance payment on the mobility decisions of displaced workers (see Section 4), we further restrict the analysis window to 24 months before and after the treatment cutoff of 36 tenure months as in Card, Chetty, and Weber, 2007. We use this as the maximum bandwidth for our RDD estimations, but estimate optimal bandwidths from this estimation sample (following the procedure laid out in Calonico, Cattaneo, and Farrell, 2020). After restricting the sample to observations with job tenure between 12 and 60 months, we are left with 533,918 job spells in our primary estimation sample, for which we have sufficient geographical information to measure their labor mobility responses.

To measure these mobility responses we rely on four primary measures of labor mobility. Firstly, we use the driving distance (in kilometers) between the firm of the terminated job spell and the firm of the next observed job spell.[3] We use this as our first measure of labor mobility, because data on the location of firms has much better coverage in the ASSD than data on the place of residence of workers. Using firm-to-firm distances thus allows us to use the largest possible number of observations for these estimations. Our second measure of labor mobility uses the driving distance (in kilometers) between the location of a worker's place of residence at the time of job termination and the location of the next observed job spell. While this is a more precise measure of labor mobility, because it is measured from the actual place an individual lives at, it comes with the drawback, that we only observe the place of residence from 1993 onward. Therefore, we can only perform these estimations on a markedly reduced sample. Our third measure of labor mobility is a dummy variable that takes on the value of one, if the next observed job spell is located in a different Austrian province than the terminated one. Similarly, our fourth measure is a dummy variable that takes on the value of one if the next observed job spell is in a different region than the place of

[3]We computed these driving distances using the OSRM package in R, which queries driving distances based on OpenStreetMaps (see Giraud, 2022).

Table 1: Sample Descriptives

	All Separations	Estimation Samples: Firm Location Available	Estimation Samples: Residence Available
	(1)	(2)	(3)
Panel A: Demographic and Job Characteristics			
Age at separation	32.13	31.63	32.29
	(11.26)	(8.04)	(7.83)
Female (%)	0.40	0.50	0.51
	(0.49)	(0.50)	(0.50)
Austrian citizen (%)	0.64	0.79	0.73
	(0.48)	(0.40)	(0.44)
Post-compulsory schooling (%)	0.55	0.58	0.60
	(0.50)	(0.49)	(0.49)
Blue collar occupation (%)	0.67	0.59	0.53
	(0.47)	(0.49)	(0.50)
Previous wage	2,013.63	2,122.17	2,264.25
	(1019.33)	(945.30)	(999.19)
Months of tenure	17.06	25.82	26.18
	(28.53)	(12.45)	(12.68)
Eligible for severance pay (%)	0.13	0.21	0.22
	(0.34)	(0.41)	(0.41)
Non-employment duration	292.73	172.36	165.21
	(805.63)	(157.95)	(157.41)
Wage in new job	2,159.25	2,064.45	2,189.39
	(994.57)	(857.91)	(912.74)
Panel B: Post-Layoff Mobility			
Distance in km: Firm-to-firm	42.84	55.94	56.89
	(95.38)	(98.60)	(97.28)
Distance in km: Home-to-firm	47.86	46.02	46.03
	(89.41)	(85.19)	(85.20)
Different province: Firm-to-firm	0.18	0.26	0.26
	(0.38)	(0.44)	(0.44)
Different province: Home-to-firm	0.20	0.21	0.21
	(0.40)	(0.41)	(0.41)
Coverage	1981-2002	1982-2002	1993-2002
Sample Size	20,916,858	533,918	221,000
Treated	2,755,976	111,687	48,385

Notes: This table compares the universe of all terminated job spells between 1981 and 2002 (column 1) with the estimation samples that result from the imposed sample restrictions described in Section 3 (columns 2 and 3). The primary estimation sample in column (2) shows all observations with sufficient information on firm-to-firm distances (available for the entire period), while the reduced estimation sample in column (3) includes only observations for which information on the place of residence at the time of job termination is available. Since this information is only available from 1993 onward, this sample contains fewer observations. Panel A presents descriptives for several demographic and job characteristics observed in the ASSD. Panel B presents descriptives for the mobility measures used as outcomes in the primary analysis. Wages are inflated to 2019 Euros. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

residence in the previous spell.

Table 1 presents a descriptive overview of our estimation samples. Column (1) shows the universe of all job separations between 1981 and 2002. Column (2) shows the full estimation sample where we use firm-to-firm distances to measure labor mobility responses. Column (3) shows the reduced sample for which residence-to-firm information is available. While our estimation samples are comparable to the universe of all job separations in terms of age, formal education and average wages, we observe minor differences for several other characteristics. In particular our sample contains slightly more males, more natives, as well as fewer blue collar workers. Also, our samples have longer average tenure durations and therefore higher eligibility rates for the severance payment. Since these differences between the universe of all job losers (column 1) and our estimation samples (columns 2 and 3) are mostly mechanical consequences of the sample restrictions we do not regard them as particularly concerning.[4] Comparing our mobility measures between the universe of all job separations and our estimation samples in Panel B of Table 1 gives mixed picture: while using firm-to-next firm distances suggest that our sample observations are slightly more mobile on average, the home-to-next firm distances suggest that the mobility responses are practically identical.

Regarding differences between our two estimation samples in Columns (2) and (3) we find that they are practically identical in terms of observed personal and job characteristics (Panel A) as well as in terms of post-layoff labor mobility (Panel B). Therefore, the limited availability of the regional information on the place of residence does not systematically impact the sample composition.

4 Estimation

To estimate the causal effect of the severance payment on displaced workers' mobility decisions, we rely on a non-parametric regression discontinuity design (RDD) as discussed in Hahn, Todd,

[4]For example, the lower rates of blue collar workers and migrant workers in the estimation samples is a direct consequence of the removal of the construction sector, where both of these worker groups are very prevalent. Removing the construction sector is necessary, because it was not covered by the severance payment law under investigation here and, therefore, our treatment does not apply for construction workers. Similarly, the longer average tenure durations and the higher rates of severance eligibility in the estimation samples are a direct consequence of the bandwidth restrictions that removes a large number of job spells which were terminated in the first year of the employment relation.

and Van der Klaauw (2001).[5] In this setting the causal effect τ of the severance pay on any outcome variable y_i can be non-parametrically identified as the difference between two conditional expectation functions evaluated at the cutoff point of 36 tenure months. Denoting discrete tenure months (i.e., the running variable) by z, this means that the causal effect of the severance pay, within some positive bandwidth b, is estimated as:

$$\tau(b) = \mu_+(b) - \mu_-(b) \tag{1}$$

whereby $\mu_+(b)$ and $\mu_-(b)$ denote the conditional expectation function to the right and to the left of the cutoff point $z = 36$. These conditional expectation functions are estimated via local polynomial regressions of order p:

$$\mu_+(b) = \underset{\beta \in \mathbb{R}^{p+1}}{\arg\min} \sum_{i=1}^{n} \mathbf{1}(z_i \geq 36)(y_i - Z_i'\beta)^2 K(z_i) \tag{2}$$

$$\mu_-(b) = \underset{\beta \in \mathbb{R}^{p+1}}{\arg\min} \sum_{i=1}^{n} \mathbf{1}(z_i < 36)(y_i - Z_i'\beta)^2 K(z_i) \tag{3}$$

whereby $\mathbf{1}(\cdot)$ denotes the indicator function, and $Z_i = (1, z_i, z_i^2, ..., z_i^p)$ denotes the p^{th} order polynomial of the running variable z.[6] The kernel function $K(z_i)$ determines how much weight each observation receives in the minimization of the sum of squared residuals to the left and to the right of the cutoff. For our baseline estimations we use a triangular kernel function, which gives higher weight to observations close to the cutoff and lets the weights decay linearly for observations farther away. In robustness checks we also present estimations for other kernel functions. Throughout all estimations we cluster standard errors at the level of discrete tenure months.

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[5] We rely on the empirical and software implementations of this non-parametric RDD estimator as discussed in Calonico, Cattaneo, and Titiunik (2014).

[6] As higher order polynomials are known to perform rather poorly in practice (Gelman and Imbens, 2019), we mostly rely on second order polynomials.

Frequency of Job Separations

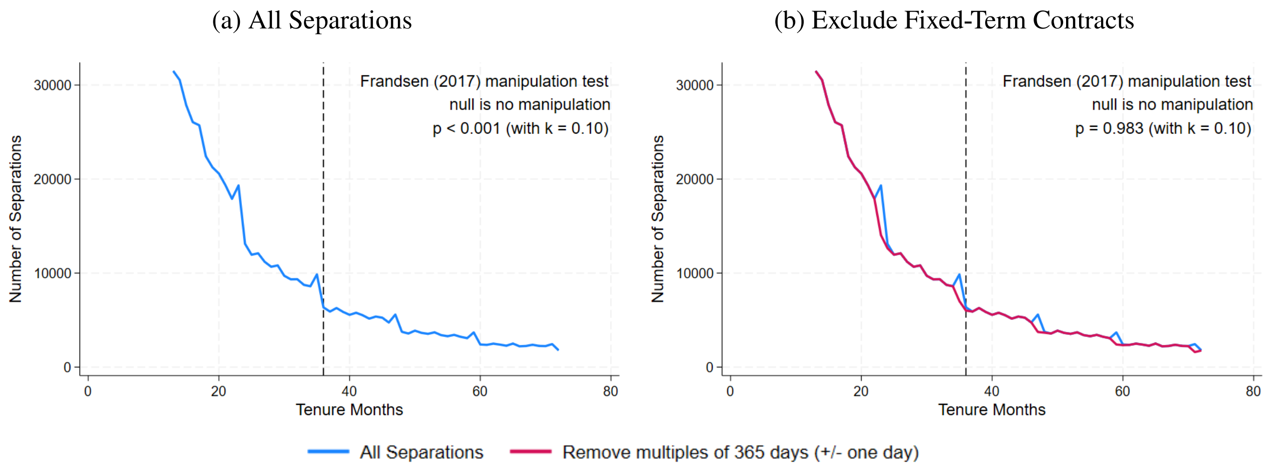
One major threat to identification in this setting arises from non-random selection around the cutoff point $z = 36$. This would, for example, be the case if firms strategically lay off employees just before they become eligible for severance pay. As can be seen from panel (a) of Figure 1 we do observe a spike in the number of separations just before the cutoff point of 36 tenure months. While the number of job separations mostly evolves very smoothly, there are also similar spikes before every multiple of 12 tenure months. This is especially worrying just before our treatment cutoff of 36 months of tenure. Here, this spike could be an indication that we have bunching just before the cutoff. This concern is also confirmed when formally testing for manipulation around the cutoff by applying a Frandsen (2017) manipulation test for discrete running variables. This test checks whether the distribution of job separations evolves smoothly around the cutoff (no manipulation) or whether there are statistically significant breaks in this distribution. Not very surprisingly, the Frandsen (2017) test indicates manipulation around the cutoff, and the spike just before 36 months of tenure thus represents a significant break in the distribution of job separations around the cutoff.

Since we observe the same spike in separations before every multiple of 12 tenure months, this might, however, not be driven by selection around the cutoff, but rather by fixed-term contracts, which automatically end after one, two, three or four years. While we cannot identify fixed-term contracts with certainty in the ASSD, panel (b) of Figure 1 shows the evolution of job separations, when we drop all employment spells that exactly last any multiple of 365 days (plus/minus one day to account for leap years). Here we see that all spikes disappear and the evolution of job separations evolves smoothly over the length of job tenure. Also the Frandsen (2017) test no longer indicates manipulation around the cutoff.[7]

To further investigate the demographic composition of the spikes in separations just before every multiple of 12 tenure months, Table 2 shows some descriptive statistics. Here column (1) shows all separations that happened after exactly 1094 to 1096 days (i.e., after 365×3 days plus/minus

[7]We find a similar pattern for the spikes at 24 and 48 months of tenure. For those spikes, the Frandsen-test also indicates significant manipulation around the cutoff which disappears once fixed-term contracts are dropped.

Figure 1: Frequency of Job Separations



Notes: This Figure shows the frequency of job-separations with increasing tenure. Panel (a) shows all job separations, while fixed-term contracts (i.e., separations that happen at a multiple of 365 tenure days +/- one day) are removed in panel (b).

Table 2: Characteristics of Fixed-Term Contracts

Spike in No. of Separations:	Tenure Month 35 (Before Treatment Cutoff)		Tenure Month 23		Tenure Month 47	
Contract Type:	Fixed-Term (1)	Others (2)	Fixed-Term (3)	Others (4)	Fixed-Term (5)	Others (6)
Panel A: Skill-Level						
Share: High-Skilled	0.241	0.093	0.257	0.100	0.184	0.087
Share: Medium-Skilled	0.469	0.445	0.469	0.445	0.504	0.444
Share: Low-Skilled	0.290	0.462	0.274	0.455	0.312	0.469
Panel B: Job Characteristics						
Wage	2412.64	2096.45	2372.60	2037.88	2565.77	2151.27
Share: Blue Collar	0.271	0.674	0.268	0.667	0.338	0.694
Firm Size	240.37	258.31	195.13	254.76	229.02	334.32
Panel C: Demographic Characteristics						
Age (at separation)	33.15	32.48	32.33	31.36	34.50	33.35
Share: Women	0.573	0.501	0.561	0.503	0.505	0.494
Share: Natives	0.832	0.800	0.818	0.780	0.843	0.802
Panel D: Labor Market Attachment						
Share: Extended UI-Benefits	0.989	0.720	0.542	0.522	1.000	1.000
Nonemp. Duration (in Days)	176.98	166.20	175.11	165.65	177.23	172.93
Panel E: Regional Mobility						
Firm to New Firm: Distance in km	57.55	51.01	58.10	54.07	53.63	52.25
Home to New Firm: Distance in km	43.92	43.41	45.79	45.69	45.61	45.61
Firm to New Firm: 1(Different Province)	0.259	0.239	0.277	0.252	0.260	0.245
Home to New Firm: 1(Different Province)	0.199	0.190	0.226	0.206	0.228	0.216
N	2826	7001	5268	13997	1848	3727

Notes: This Table compares the observed characteristics of these terminated fixed-term contracts with all other terminations within the same tenure month. Wages are inflated to 2019 Euros. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

one day). We dub those as 'fixed-term contracts'. Column (2) shows all separations that also happened in the 35th tenure month (i.e., the month before a displaced worker becomes eligible for the severance pay), excluding the fixed-term contracts from column (1). Hence, the comparison of columns (1) and (2) shows how those fixed-term contracts differ from other separations within the 35th tenure month. Columns (3) and (4) present a similar comparison for the 23rd tenure month, and columns (5) and (6) for the 47th tenure month. The fixed-term contracts in columns (1), (3) and (5) contain all observations that were dropped for Figure 1, Panel (b), i.e., those observations that are responsible for the visible spikes in Figure 1.

Comparing fixed-term contracts to other separations shows that fixed-term workers are generally of a higher skill level (Panel A), earn higher wages and are much more likely to be white-collar workers (Panel B). They also have a stronger labor market attachment (measured by the share of workers eligible for extended UI benefits in Panel D).[8] Importantly, this pattern points towards fixed-term workers being *more* mobile in response to job loss than the other separations in the 35th (and also the 23rd and 47th) tenure month. This is in particular suggested by their higher skill levels and higher wages, which are generally associated with *higher* mobility (see Bound and Holzer, 2000 or Bütikofer and Peri, 2021). This is also reflected in the differences in post-layoff mobility behavior shown in Panel E of Table 2. Here, workers in fixed-term contracts move slightly larger distances and also have slightly higher movement probabilities after layoff than other workers within the 35th tenure month. Since our results indicate that the severance payment *increased* labor mobility, these higher values for fixed-term contracts (that ended just before severance eligibility) cannot cause these results. If anything, these high-mobility observations' bunching just before the treatment cutoff would attenuate our estimated effects. This is also confirmed by estimating a Donut-RDD, which removes these observations from the estimation sample, and tends to slightly increases our estimated effect sizes (although the difference to our baseline results is not statistically

[8]Displaced workers become eligible for extended unemployment benefits, if they have been in employment for at least 36 months during the last 5 years. For the subset of workers that have no previous employment this discontinuity in the eligibility for extended unemployment benefit coincides perfectly with the eligibility cutoff for the severance pay. We discuss this double-discontinuity in corresponding robustness checks in Section 6.

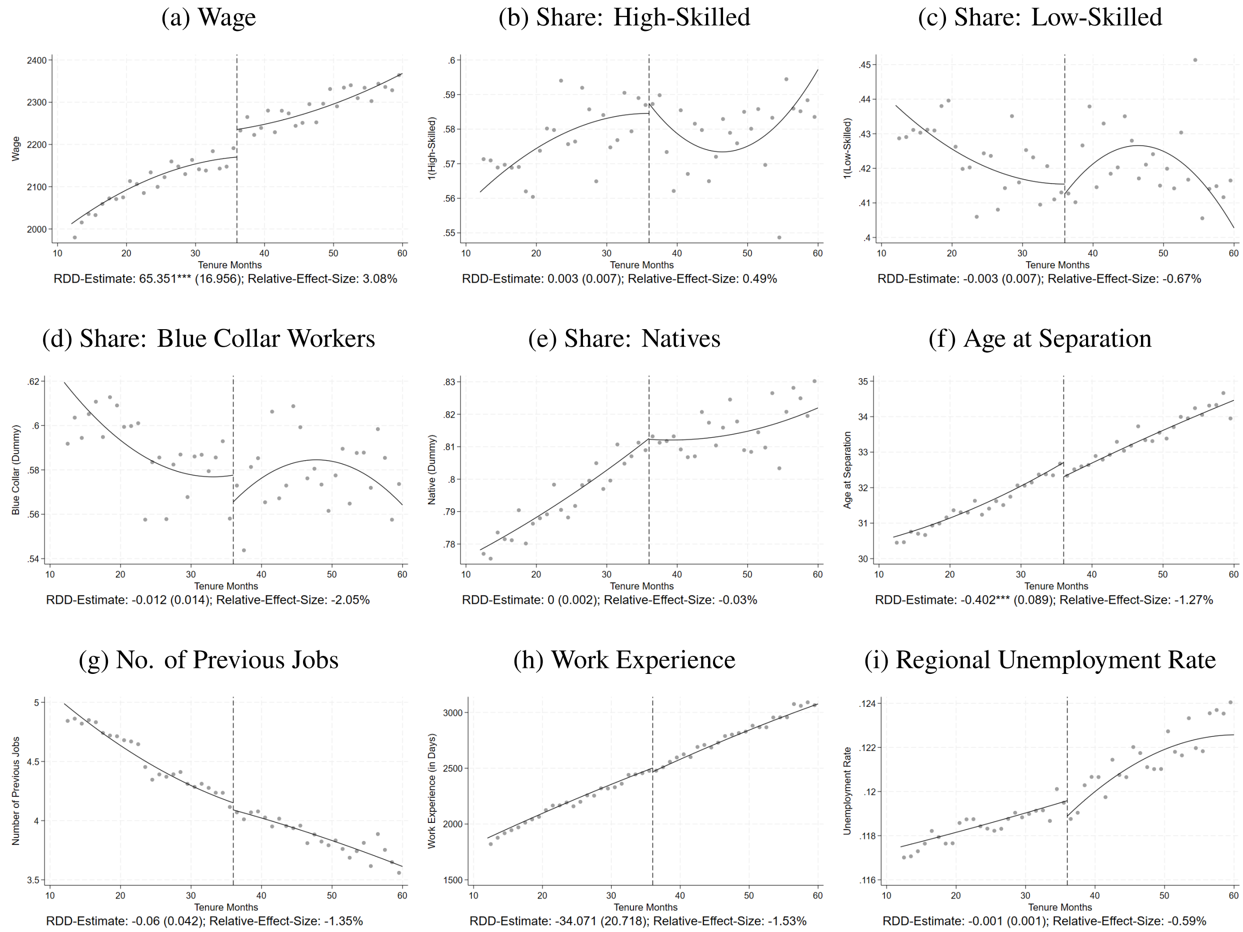
significant).

To further ensure that our results are not driven by the visible spikes in Figure 1, we explore falsifications tests where we use placebo cutoffs at 24 and 48 months of tenure as placebo treatments. If our results were driven by the spike just before our real treatment at 36 months of tenure, then we would expect to find similar results at these placebo cutoffs. Reassuringly, these placebo tests do not lead to significant estimates, providing further evidence that our results are not driven by selection but rather represent causal effects of the severance payment. We provide a more detailed discussion of both the Donut-estimations and these placebo tests in Section 6.

Covariate Balance around the Cutoff

To assess systematic selection around the treatment cutoff, Figure 2 presents balancing tests for several observed covariates. Panel (a) shows the balancing test for the wage rate, where we find a clear and statistically significant discontinuity. For the treated group right of the cutoff wages are on average around €65 higher. This discontinuity in the wage rate has also been documented by Card, Chetty, and Weber (2007). They highlighted that, while statistically significant, this discontinuity is very small and thus not economically meaningful. In our case, the point estimate for the discontinuity (65 Euros) amounts to less than 3% of the average wage just before the cutoff (2,200 Euros). While the economic magnitude of this discontinuity, thus, is very small, it could still be an indication that we have skill-based selection around the cutoff, with low-skilled workers (which are likely to earn lower wages) facing a higher probability of being laid off just before severance eligibility. Since individual skill levels are known in the literature to be a strong predictor of mobility behavior (see for example Bound and Holzer, 2000 or Bütikofer and Peri, 2021), such a pattern of selection is particularly worrisome. To further probe if there is indeed skill-based selection around the cutoff, panels (b) and (c) of Figure 2 show balancing test for the shares of high- and low-skilled workers. Here we do not detect any significant discontinuities in these skill shares, which points at the absence of skill-based selection around the cutoff. The same holds true for the share of blue-collar workers in panel (d), which is also balanced.

Figure 2: Balancing Tests



Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. All estimations use triangular kernel function. Wages are inflated to 2019 Euros.

Panel (e) of Figure 2 presents a balancing test for the share of natives. This balancing variable is of particular importance, as having a personal migration history is known to positively influence the probability of migrating again in the future (see Basso and Peri, 2020). This test again suggests that there is no selection around the cutoff based on nationality. Another important determinant of inter-regional mobility is age, as older individuals are less likely to move greater distances. As is shown in panel (f) of Figure 2 we detect a very small discontinuity in the age at separation. This discontinuity indicates, that treated workers are on average around five months younger than untreated workers. While this discontinuity is statistically significant, its magnitude is again very small and is not meaningful in economic terms. Furthermore, this discontinuity does not translate into similar discontinuities in several measures of previous work experience (panels g and h). Lastly, we also do not detect any discontinuity in the regional unemployment rate in panel (i), indicating that labor market conditions are balanced between treated and untreated individuals.

5 Results

Figure 3 presents our primary estimation results. To measure mobility responses, we rely on four different measures of inter-regional mobility. First, we use the driving distance (in kilometers) between the previous firm (i.e., the firm the worker has been laid off from) and the firm of the next documented job spell. Since data on the geographical location of firms has much better coverage in the ASSD than data on the geographical location of workers' residences, this allows forming these estimations with the largest possible sample.[9] Figure 3 presents the estimated effect of the severance payment on this mobility measure using an optimal bandwidth estimator (in panel a) and a much longer bandwidth of 24 months (panel e).[10] Both estimations result in a precisely estimated positive effect, indicating that the severance payment has *increased* the mobility of displaced workers. Relative to the baseline mobility rates in the respective estimation samples these estimates suggest sizable mobility increases of around 7.87‰ to 10.31‰, whereby the optimal bandwidth estimation results in a slightly larger estimate.

Panels (b) and (f) of Figure 3 show the estimation results for our second mobility measure. Here, we use the driving distance (in kilometers) between the place of residence at the time of job termination and the location of the firm in the next observed job spell. Although this is a more precise measure for changes in labor mobility than the distance between the old and new firm in panels (a) and (e), it comes with the drawback that data on the residence are only available from 1993 onward. Therefore, these estimations can only regard job terminations that happened in the sub-period 1993–2002. As before, these estimations indicate a positive effect. The receipt of

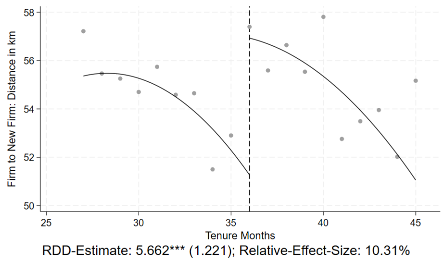
[9]See Section 3.

[10]For the optimal bandwidth estimation we rely on the procedure proposed by Calonico, Cattaneo, and Farrell (2020).

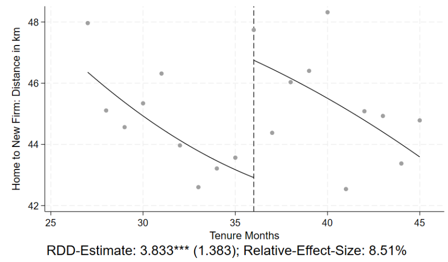
Figure 3: Main Results

Optimal Bandwidth

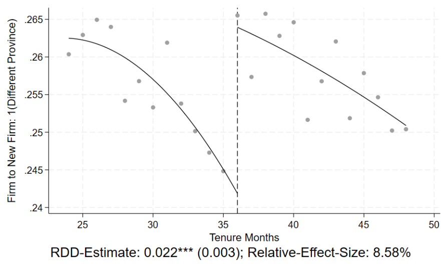
(a) Firm to New Firm: Distance in km



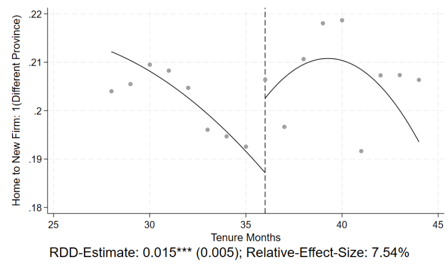
(b) Home to New Firm: Distance in km



(c) Firm to New Firm: 1(Different Province)

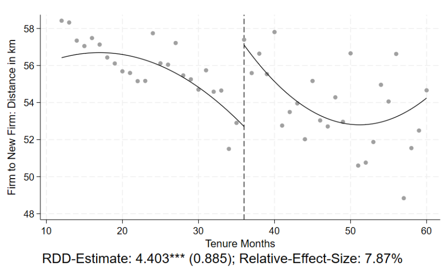


(d) Home to New Firm: 1(Different Province)

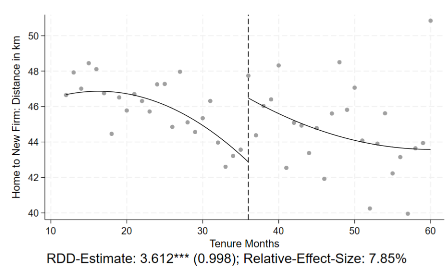


Maximum Bandwidth

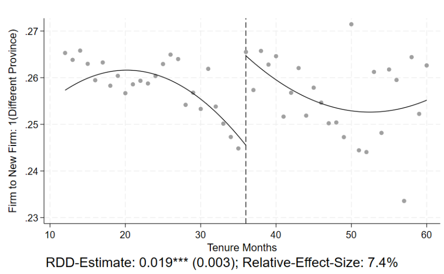
(e) Firm to New Firm: Distance in km



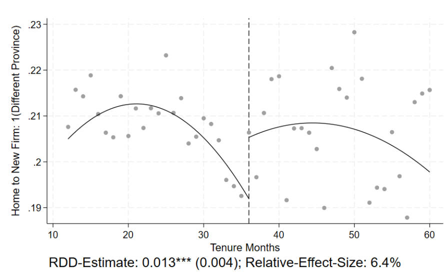
(f) Home to New Firm: Distance in km



(g) Firm to New Firm: 1(Different Province)



(h) Home to New Firm: 1(Different Province)



Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

severance pay increases the distance to the new job by around 7.85% to 8.51%.

Panels (c) and (g) of Figure 3 show estimation results for the probability that the next observed job spell is located in a different Austrian province than the previous terminated job spell. Again, the estimates show a clear increase in the probability that the next job will be located in a different province when the worker is eligible for the severance payment. The magnitude of the estimates suggests an increase of around 7.40% to 8.58% relative to the average in the data.

Lastly, panels (d) and (h) present similar estimations for the probability that the next job is located in a different province than the previous place of residence. As before, we find a clear and robust positive effect on the probability of working in a different province, indicating a mobility increase of around 6.4% to 7.54% when receiving the severance payment.

In general, the results documented in Figure 2 clearly show that receiving a severance pay increases inter-regional mobility of displaced workers. Moreover, no matter how we define mobility, the resulting liquidity-mobility elasticities are very similar – between 6.4 and 10.3. This picture is consistent with the presence of financial mobility frictions that hinder labor mobility (as in Bound and Holzer, 2000). As workers receive severance pay, they are better equipped to pay for mobility costs, making them more likely to seek new employment further away. These results do not support the theory of Notowidigdo (2020) who argues that severance pay will reduce the severity of the employment shock and, thereby, could also reduce the incentive to move to a different region. While our estimations cannot rule out that such a mechanism exists, our results clearly point towards a much stronger importance of mobility frictions.

5.1 Covariate Adjustment

Table 3 presents the corresponding point estimates for all four mobility measures using the optimal bandwidth estimator. Column 1 shows our parsimonious main specification, also depicted graphically in Figure 3. Column 2 further adds a set of predetermined covariates for which the corresponding balancing tests (see Figure 2) suggest a continuous evolution around the cutoff. Including these balanced control variables leaves our estimates basically unchanged.

Table 3: Covariate Adjustment

	(1)	(2)	(3)	(4)	(5)
Panel A: Firm to New Firm: Distance in km					
Severance Pay	5.662***	5.735***	5.721***	5.577***	4.750**
	(1.221)	(1.448)	(1.420)	(1.442)	(2.250)
Relative Effect Size (in %)	10.1	10.3	10.2	10.0	8.5
N	126050	122405	122405	122405	122405
Panel B: Home to New Firm: Distance in km					
Severance Pay	3.833***	3.739**	3.535**	3.306**	3.640**
	(1.383)	(1.476)	(1.484)	(1.512)	(1.780)
Relative Effect Size (in %)	8.3	8.1	7.7	7.2	7.9
N	52966	50976	50976	50976	50976
Panel C: Firm to New Firm: 1(Different Province)					
Severance Pay	0.022***	0.022***	0.022***	0.022***	0.017***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
Relative Effect Size (in %)	8.5	8.6	8.4	8.3	6.5
N	179277	173062	173062	173062	173062
Panel D: Home to New Firm: 1(Different Province					
Severance Pay	0.015***	0.012**	0.011**	0.010*	0.015**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
Relative Effect Size (in %)	7.4	5.8	5.1	4.7	7.4
N	46790	44992	44992	44992	44992
Controls:					
Gender (Dummy)		x	x	x	x
Native (Dummy)		x	x	x	x
Blue Collar (Dummy)		x	x	x	x
No. of Previous Jobs		x	x	x	x
Work Experience (in Years)		x	x	x	x
High Skill (Dummy)		x	x	x	x
Medium Skill (Dummy)		x	x	x	x
Regional Unemployment Rate		x	x	x	x
Wage			x	x	x
Age (at Separation)				x	x
Estimator:	CC&F (2020)	CC&F (2020)	CC&F (2020)	CC&F (2020)	F&H (2019)

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022). The covariate adjustments presented in columns (1) to (4) use the covariate adjustment proposed by Calonico, Cattaneo, and Farrell (2020) (indicated as CC&F), while the estimation in column (5) uses the covariate adjustment proposed by Frölich and Huber (2019) (indicated by F&H).

Columns 3 and 4 include covariates for which the balance tests in Figure 2 indicated numerically small but statistically significant discontinuities around the cutoff. These variables are the wage rate (column 3) which is on average around €65 (or around 3.08%) higher for units just to the right of the cutoff. Column 4 also includes the age at the time of separation for which the balance test in Figure 2 indicated that the treated individuals are on average about 5 months (or around 1.27%) younger than the untreated workers. Including the wage rate and the age at the time of separation in columns 3 and 4 again leaves the magnitude of the estimated effects largely unchanged.

All covariate-adjusted estimates in columns (2) to (4) of Table 3 have in common that they are estimated using the covariate adjustment proposed by Calonico, Cattaneo, and Farrell (2020). As emphasized by Cattaneo and Titiunik (2022) and Cattaneo, Keele, and Titiunik (2023), this estimator cannot adequately control for the possible bias that is caused by selection into treatment, even if the variables on which this selection is based are observed and controlled for. If firms, thus, selectively lay off workers just before they become eligible for the severance payment based on their wage rate or age, and if these selection processes are what are driving our results, than simply controlling for the wage rate and the age would not remove this bias reliably when using the Calonico, Cattaneo, and Farrell (2020) estimator. An alternative estimator that can account for bias caused by such selection on observables has recently been proposed by Frölich and Huber (2019). This estimator is able to control for selection as long as the variables the selection process is based on are observed. In our context, this means that this estimator can remove any bias that may be caused by the observed (but very small) discontinuities in the wage rate and the age at separation documented in the balancing test in Figure 2. Column (5) of Table 3 presents estimation results using the Frölich and Huber (2019) estimator, where the same set of control variables is included as in column (4). Comparing the estimation results from these two estimators shows that all of our results remain stable when using this alternative estimator. We are therefore confident that the very minor, and thus economically insignificant, imbalances around the cutoff documented for the wage rate and the age at the time of separation in Figure 2 are not driving our results.

5.2 Effect Heterogeneities

An important result in the literature on labor mobility, first documented by Bound and Holzer (2000), is that low-wage workers are less likely to respond to an employment shock by seeking new employment in more distant regions. This is already reflected in the much lower baseline mobility rates of low-wage and low-skill workers in Table 4 (indicated by the mean values of the outcome variables). Here, a comparison between columns (2) and (3) shows that low-wage and high-wage workers show markedly lower rates of baseline mobility. For example, the distance to the next job spell measured from the location of the terminated job spell (panel A) is 13% lower for low-wage workers as opposed to high-wage workers. The same finding emerges when the sample is split by formal education between low- and high-skilled workers in columns (4) and (5). Here, low-skilled workers' baseline distance between the old and new job (panel A) is around 19% lower. The same picture emerges for all other mobility measures used. This limited inter-regional mobility of low-wage and low-skill workers reduces their ability to recover from an employment shock and might leave them stuck in declining regions (Bound and Holzer, 2000). Since financial mobility frictions are more likely to affect those types of workers we would expect them to increase their inter-regional mobility more strongly when receiving the severance payment.

This is confirmed in the estimates shown in columns (2) and (3) of Table 4 where we estimate the effect of the severance payment separately for workers earning below the median wage rate (column 2) and above the median wage rate (column 3). We find that low-wage workers react more strongly to severance pay. This pattern emerges for all four measures of interregional mobility. Furthermore, this result is robust to splitting the sample according to the highest level of formal education between low- and high-skilled workers in columns (4) and (5).

In sum, these results clearly show that low-wage and low-skilled workers show much stronger mobility reactions when receiving the severance payment. Since those workers are the ones that are most affected by high mobility costs in the first place, this picture lends strong support for the importance of financial mobility frictions.

Table 4: Wage and Skill Heterogeneities

		Wage		Education	
	All	< Median	≥ Median	Low	Medium/High
	(1)	(2)	(3)	(4)	(5)
Panel A: Firm to New Firm: Distance in km					
Severance Pay	5.662***	10.309***	3.616**	4.557***	4.650***
	(1.221)	(0.889)	(1.647)	(1.533)	(1.771)
Mean of Outcome Variable	54.9	51.8	59.2	47.9	59.2
Relative Effect Size (in %)	10.3	19.9	6.10	9.51	7.86
N	126050	49353	49452	46530	64507
Panel B: Home to New Firm: Distance in km					
Severance Pay	3.833***	8.058***	0.316	5.197***	1.113
	(1.383)	(1.049)	(2.094)	(1.646)	(1.977)
Mean of Outcome Variable	45.0	41.2	48.0	38.1	48.1
Relative Effect Size (in %)	8.51	19.6	0.66	13.6	2.32
N	52966	21549	21659	17199	26968
Panel C: Firm to New Firm: 1(Different Province)					
Severance Pay	0.022***	0.028***	0.024***	0.030***	0.017***
	(0.003)	(0.007)	(0.004)	(0.005)	(0.004)
Mean of Outcome Variable	0.26	0.23	0.29	0.23	0.27
Relative Effect Size (in %)	8.58	12.3	8.13	12.9	6.14
N	179277	69905	70685	66161	91535
Panel D: Home to New Firm: 1(Different Province)					
Severance Pay	0.015***	0.031***	0.003	0.019**	0.019*
	(0.005)	(0.006)	(0.008)	(0.008)	(0.011)
Mean of Outcome Variable	0.20	0.18	0.23	0.17	0.22
Relative Effect Size (in %)	7.54	17.5	1.29	11.4	8.74
N	46790	19043	19111	15144	23828

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

Table 5: Further Heterogeneities

		Gender		Unemployment Rate	
	All	Female	Male	< Median	≥ Median
	(1)	(2)	(3)	(4)	(5)
Panel A: Firm to New Firm: Distance in km					
Severance Pay	5.662***	7.080***	4.099**	4.470**	6.506***
	(1.221)	(1.204)	(1.595)	(2.239)	(1.181)
Mean of Outcome Variable	54.9	52.9	56.9	56.6	51.9
Relative Effect Size (in %)	10.3	13.4	7.21	7.89	12.5
N	126050	63592	62458	61298	60956
Panel B: Home to New Firm: Distance in km					
Severance Pay	3.833***	5.763**	1.488	2.349	5.069***
	(1.383)	(2.441)	(2.834)	(2.232)	(1.825)
Mean of Outcome Variable	45.0	43.1	47.2	46.2	42.7
Relative Effect Size (in %)	8.51	13.4	3.16	5.09	11.9
N	52966	27243	25723	25540	25358
Panel C: Firm to New Firm: 1(Different Province)					
Severance Pay	0.022***	0.023***	0.020***	0.022***	0.026***
	(0.003)	(0.008)	(0.006)	(0.005)	(0.004)
Mean of Outcome Variable	0.26	0.24	0.28	0.24	0.27
Relative Effect Size (in %)	8.58	9.55	7.27	8.88	9.91
N	179277	90352	88925	86483	86364
Panel D: Home to New Firm: 1(Different Province)					
Severance Pay	0.015***	0.019*	0.010	0.016**	0.017*
	(0.005)	(0.010)	(0.010)	(0.008)	(0.009)
Mean of Outcome Variable	0.20	0.19	0.22	0.18	0.22
Relative Effect Size (in %)	7.54	9.99	4.32	8.99	7.75
N	46790	24119	22671	22441	22479

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

Columns (2) and (3) of Table 5 explore effect heterogeneity by gender. Comparing the baseline mobility rates reveals that women are comparatively less mobile than men. For example, the average distance between the location of the terminated and the next observed job spell in panel A is around 7% lower for women. The same pattern holds for all of our mobility measures. While this difference is not as pronounced as the differences between skill-groups documented in Table 4 there is still clear gender heterogeneity in baseline mobility rates, with women generally being less mobile than men. Looking at the estimated effects of the severance payment on inter-regional mobility by gender in Table 5 shows that women increase their mobility much more than men. As before for the skill-groups in Table 4, the severance payment, in particular, prompts mobility responses by those worker groups who show comparatively lower baseline mobility rates. [11]

Columns (4) and (5) of Table 5 show heterogeneous effects by the regional unemployment rate.[12] If workers' inter-regional mobility also responds to local economic conditions (as is suggested by Blanchard and Katz, 1992 or Bound and Holzer (2000), among others) rather than just to their individual employment shock, then we would expect to see stronger mobility responses in regions with higher unemployment rates. This is precisely what we find in columns (4) and (5) of Table 5. Here, our estimation results suggest stronger mobility responses for workers in regions with a higher unemployment rate. However, while the mobility responses are stronger in high-unemployment regions, workers in lower unemployment regions also increase their inter-regional mobility in response to their individual employment shock. Hence, the individual experience of job loss also plays an important role in the probability to increase labor mobility, which is amplified by adverse economic conditions.

[11] This compares to recent evidence about the effect of an Austrian job guarantee program (Ahammer et al., 2025) where effects for men and women are rather similar.

[12] To split the sample along the median of the regional unemployment rate, these unemployment rates have been standardized within each year t (by subtracting the mean $\hat{\mu}_t$ and dividing by the standard error $\hat{\sigma}_t$). Since unemployment rates have been steadily rising during the observational period, this ensures that this sample split does not sort earlier periods in the first group and later periods in the second (i.e., does not result in a split by time periods). Rather, this ensures that both subsamples in columns (4) and (5) of Table 5 contain observations from all years in the sample period. This ensures that the estimations are not contaminated by a temporal drift in the unemployment rates.

5.3 Benchmarking the Effect of Existing Mobility Subsidies

Since 2005, the Austrian unemployment agency (AMS) offers a mobility subsidy to unemployed workers who take on jobs in distant regions. To be eligible for this mobility subsidy, these workers have to have spent some time in unemployment during which they were unable to find a job closer to their place of residence. Additionally, their gross monthly salary has to be less than 2,700 Euro (in 2026 Euros). This subsidy thus explicitly targets unemployed workers with lower skill levels who face difficulties finding a suitable new job. If these conditions are fulfilled, the AMS offers to subsidize commuting costs up to 260 Euros per month (for a maximum of 6 months), or rent costs of up to 400 Euros per month (for a maximum of 12 months). The total maximum subsidy thus amounts to 4,800 Euros (for a rent subsidy of 400 Euros over the full 12 months), or 178% of the maximum possible monthly gross salary (of 2,700 Euros) to still be eligible for the subsidy. It is thus comparable in magnitude to the severance payment from which we estimate the mobility effect of cash-on-hand payments. Since this subsidy explicitly targets low-wage workers, our results in Table 4 (Column 2) suggest that this subsidy leads to an increase in the regional mobility of these unemployed workers of around 12% to 20%.

6 Robustness Checks

6.1 Bandwidth Choice

While the primary estimations in Figure 3 show results for an optimal bandwidth estimator as well as a fixed bandwidth of 24 months (as in Card, Chetty, and Weber, 2007), Figure A1 in the Appendix shows more detailed estimations to assess the robustness with respect to bandwidth choice. Here we plot the estimates for bandwidths ranging from 8 to 24 months, whereby the bandwidth used for estimation is successively increased in steps of one tenure month. Overall, all estimates are very stable with respect to the bandwidth choice. In all cases the estimated 95% confidence intervals overlap over the entire range of bandwidths, and we thus cannot reject equality of the estimated effects with regard to bandwidth choice.

6.2 Donut Estimator

As is documented in Figure 1 we find a small spike in the frequency of separations immediately before our cutoff point of 36 tenure months. Since we observe similar spikes at each multiple of 12 tenure months, we attribute this spike to the presence of fixed-term contracts. To probe how sensitive our estimates are to the presence of this bunching immediately before our cutoff point, Figure A2 in the Appendix presents alternative estimation results where we drop the bunching point (+/− one day). This essentially amounts to estimating a Donut-RDD, with a hole of one day to the left and right of the cutoff point. As is shown in Figure 1 (panel B), this very narrow donut-hole is sufficient to remove the visible spike, such that the distribution of separations evolves smoothly around the cutoff point (as is also indicated by the Fransen-Test). Figure A2 in the Appendix demonstrates that all of our results are robust to the exclusion of the bunching point. Figure A2 also presents Donut-RDD estimations with wider donut holes of one week and one month. As before all results are robust to these additional donut estimations.

6.3 Placebo Treatments

While we clearly see, that individuals to the right of 36 months of job tenure do exhibit more regional mobility, it is not clear, whether this discontinuity is ultimately caused by the severance pay. To exclude other potential reasons for this – including the influence of fixed contracts – we look into some placebo treatments. Obvious choices are 24 and 48 tenure months, which we use as placebo cutoff points. If our results were driven by the spike before the treatment cutoff of 36 tenure months, and thus by the presumed presence of fixed-term contracts, then we would expect to see similar results at the placebo cutoffs. Figure A3 in the Appendix presents the estimation results for these placebo treatments. All estimations in A3 include all available observations. Hence, the fixed-term contracts which cause the visible spikes in Figure 1 are included in the estimation sample. Therefore, we would expect to find similar positive mobility results for the placebo treatments, if our main results were driven by the spike in separations just before our treatment cutoff. Reassuringly, the estimated placebo effects are close to zero and not statistically significant. For none of the placebo treatments we are able to estimate a robust positive effect on our labor mobility measures. We are, therefore, confident that the results discussed in the main part of this paper are indeed driven by the treatment (i.e., the eligibility for severance pay after 36 months of tenure) and are not an artifact of the presence of fixed-term contracts visible in Figure 1 or anything else.

6.4 Composite Covariate Index

Another threat to identification arises from the small discontinuity in the wage rate documented in Figure 2. This discontinuity amounts to €65 of additional income (or around 3% of the average wage just before the cutoff) and is thus very small and not significant in economic terms. These additional €65 are, thus, unlikely to drive the substantial mobility increases indicated by our primary results (which generally range between 7% to 10%). The same holds true for the very small discontinuity in the age at the time of separation, which indicates that treated workers are on average around 5 months younger. As with the wage rate, we regard this discontinuity as too small in magnitude to be a relevant driver of our primary results. Furthermore, all our estimation results proved to be robust to including the wage rate and the age at separation as control variables (see Table 3).

To be sure that our results are not driven by possible selection around the cutoff, Figure A4 in the Appendix compares our main results with corresponding estimations using a composite covariate index. This index is constructed by predicting our mobility measures from the wage rate and the age at time of separation (i.e., the covariates for which the balancing tests in Figure 2 indicated discontinuous jumps at the cutoff point). Thus, it is obtained as the fitted value from a linear regression of mobility measure y_i on a vector of control variable X_i (including the wage rate and the age), and captures the part of the mobility response that is explained by these unbalanced covariates. In Figure A4 we test this composite covariate index for the presence of discontinuities around the cutoff, which would be an indication of selection bias. As is visible from Figure A4 we do not detect any economically or statistically significant discontinuities. While all four composite covariate indexes result in very small discontinuities, the magnitude of those discontinuities comes nowhere near the magnitude of our main results.

6.5 Alternative Kernel Functions

Table A1 in the Appendix presents robustness checks for the kernel choice. Throughout this paper we rely on a triangular kernel in the estimation of the RDD-model. This kernel gives larger weights to observations close to the cutoff, and lets these weights decay linearly the further away an observation lies from the cutoff. These baseline results are depicted in column (2) of Table A1. Alternative kernel functions are the uniform-kernel (which assigns every observation the same weight; column 1) or the Epanechnikov-kernel (which lets the weights decay quadratically for observations farther away from the cutoff; column 3). As depicted in Table A1, our results are robust to the kernel choice.

6.6 Eligibility for Extended UI-Benefits

In Austria, displaced workers become eligible for extended unemployment benefits, if they have been in employment for at least 36 months during the last 5 years. For the subset of workers that have no previous employment this discontinuity in the eligibility for extended unemployment benefit coincides perfectly with the eligibility cutoff for the severance pay. This is also visualized in Figure A5 in the Appendix. Here, the fraction of workers who are eligible for extended unemployment benefits rises steadily from just bellow 0.40 in tenure month 12 to around 0.80 in tenure month 35, with everybody being eligible thereafter.

To make sure that our results are not driven by this additional discontinuity in the eligibility for extended unemployment benefits, Table A2 in the Appendix compares our main results (panel A) with estimations where we drop all observations who are ineligible for the extended unemployment benefit (panel B and C). Dropping those workers from the estimation sample ensures that all workers below and above the cutoff are eligible for extended unemployment benefits, and only differ in their eligibility for the severance payment. As is shown in Table A2 in the Appendix, all of our results are robust to eliminating the discontinuity in the eligibility for extended unemployment benefits, with most estimators even slightly increasing in size.

7 Conclusion

Most European countries are characterized by a comparatively low degree of regional labor mobility. Against the backdrop of rising skill and labor shortages, as well as regional mismatch unemployment the question of how to increase workers' willingness to seek employment in other geographical regions gains growing relevance for researchers and policy makers alike.

One possible policy tool to increase workers' willingness to move is the provision of monetary incentives. However, the effects of such a cash payment on mobility decisions are theoretically ambiguous. One the one hand, receiving a cash payment may help workers who are affected by the presence of mobility costs to overcome those frictions and thereby stimulate their regional mobility. On the other hand, cash payments may dampen the adverse effects of an employment shock, thereby reducing the incentive to move. Both of these arguments have been advocated in the economic literature, offering two plausible - but opposite - predictions for the effect of cash payments on labor mobility.

In this paper we test those two competing mechanisms against each other, using a severance pay regulation in Austria that was in effect from 1981 to 2003. This severance pay scheme offered workers a severance payment of two monthly salaries if they were laid off from an employment relation that lasted for at least 36 months. Since this severance pay scheme generated a sharp discontinuity in the eligibility for the payment, it presents itself as an ideal laboratory to test the effects of this cash-on-hand payment on workers' mobility decisions.

Applying a sharp regression discontinuity design, we find that the severance payment had an increasing effect on labor mobility. Overall, workers who were eligible for the severance payment increased their interregional mobility by around 6% to 10%. This suggests that the presence of mobility frictions in the form of financial mobility costs play a crucial role in impeding the mobility of displaced workers. While we cannot rule out the possibility that the reduction in the severity of the employment shock may have a negative effect on labor mobility, our results suggest that this channel is clearly dominated by the positive effect caused by the reduction of financial mobility frictions.

Exploring heterogeneous effects by population subgroups further strengthens this point. Here we find that those worker groups that show lower levels of baseline mobility (i.e., low-skilled workers and women) show much stronger increases in labor mobility after receiving the severance payment. This is especially pronounced for low-skilled workers who are plausibly stronger affected by the presence of financial mobility frictions. Providing these workers with cash-payments helps ease these frictions and thereby stimulates their labor mobility.

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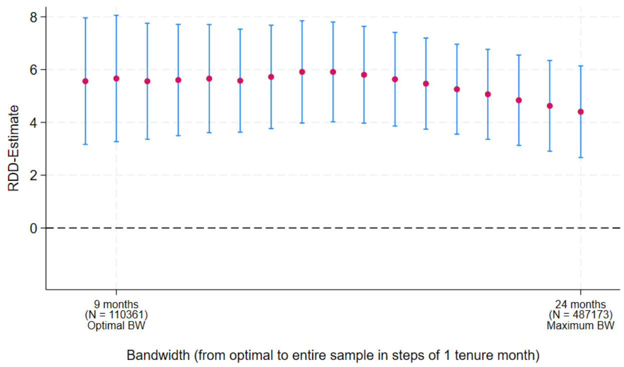
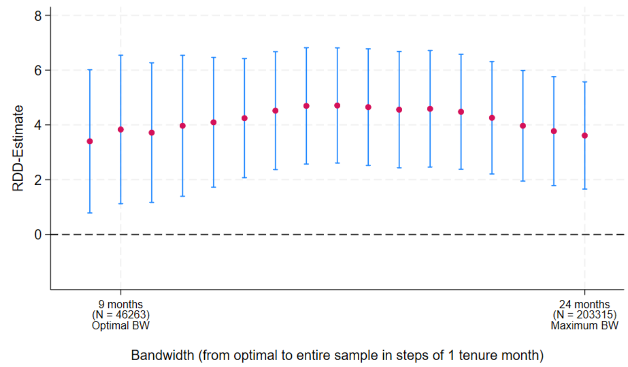
Appendix

Figure A1: Bandwidth Robustness

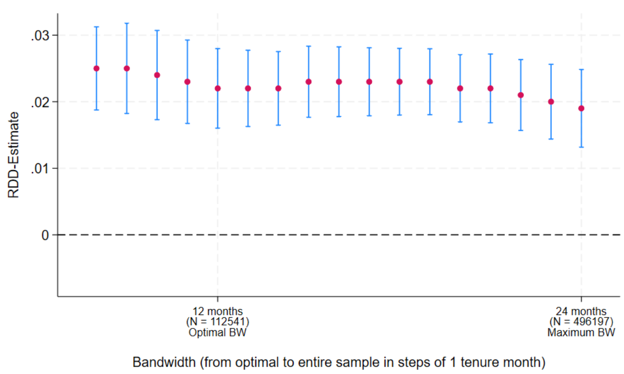
(a) Firm to New Firm: Distance in km



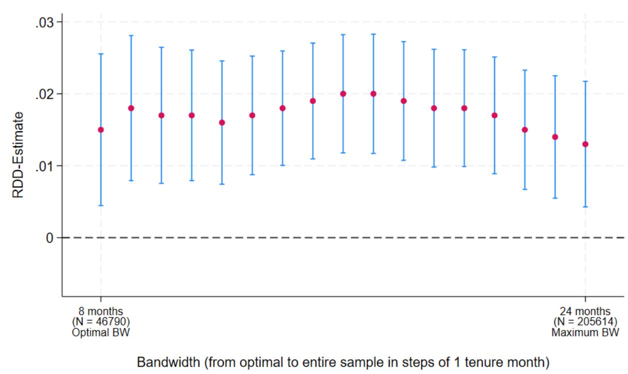
(b) Home to New Firm: Distance in km



(c) Firm to New Firm: 1(Different Province)

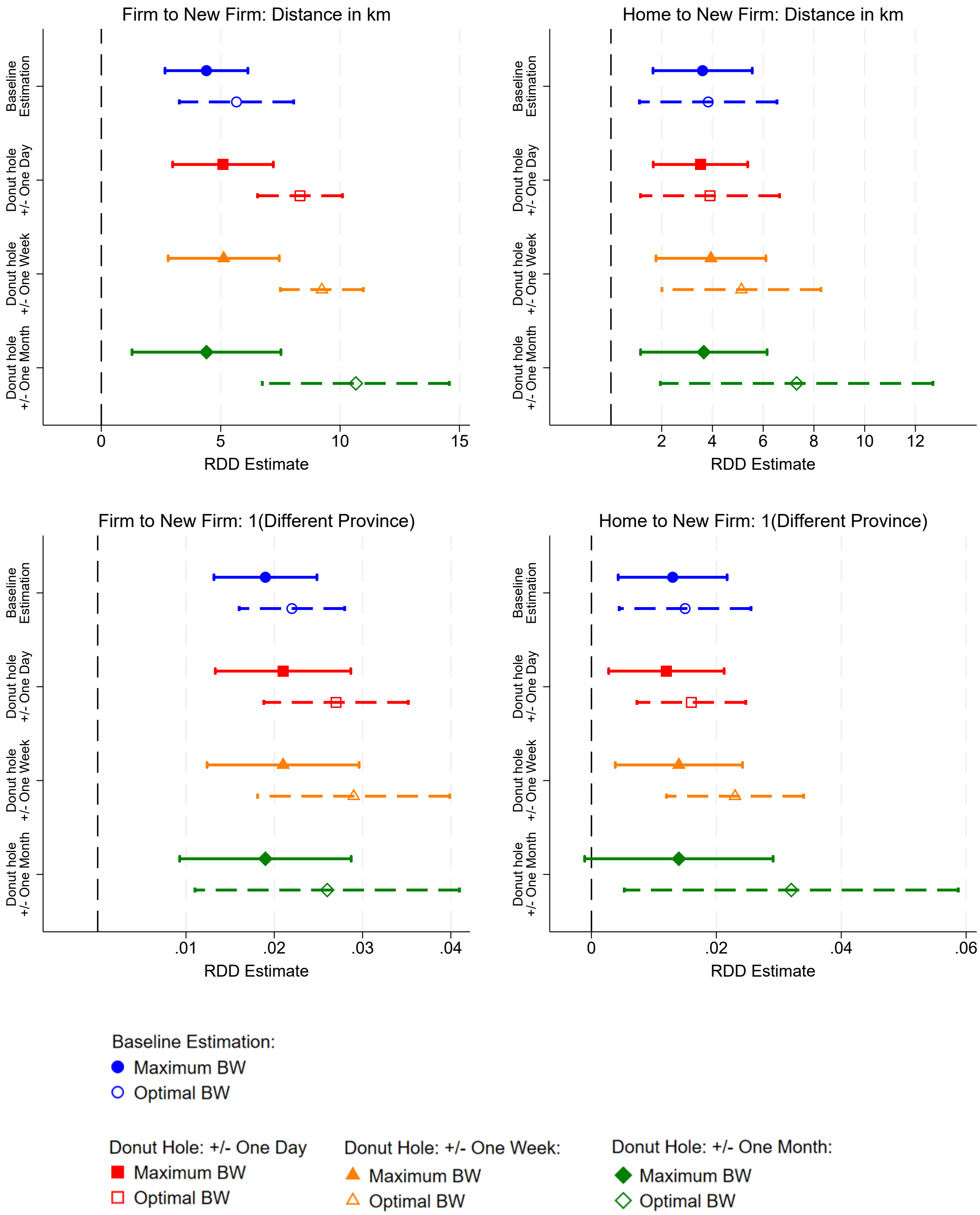


(d) Home to New Firm: 1(Different Province)



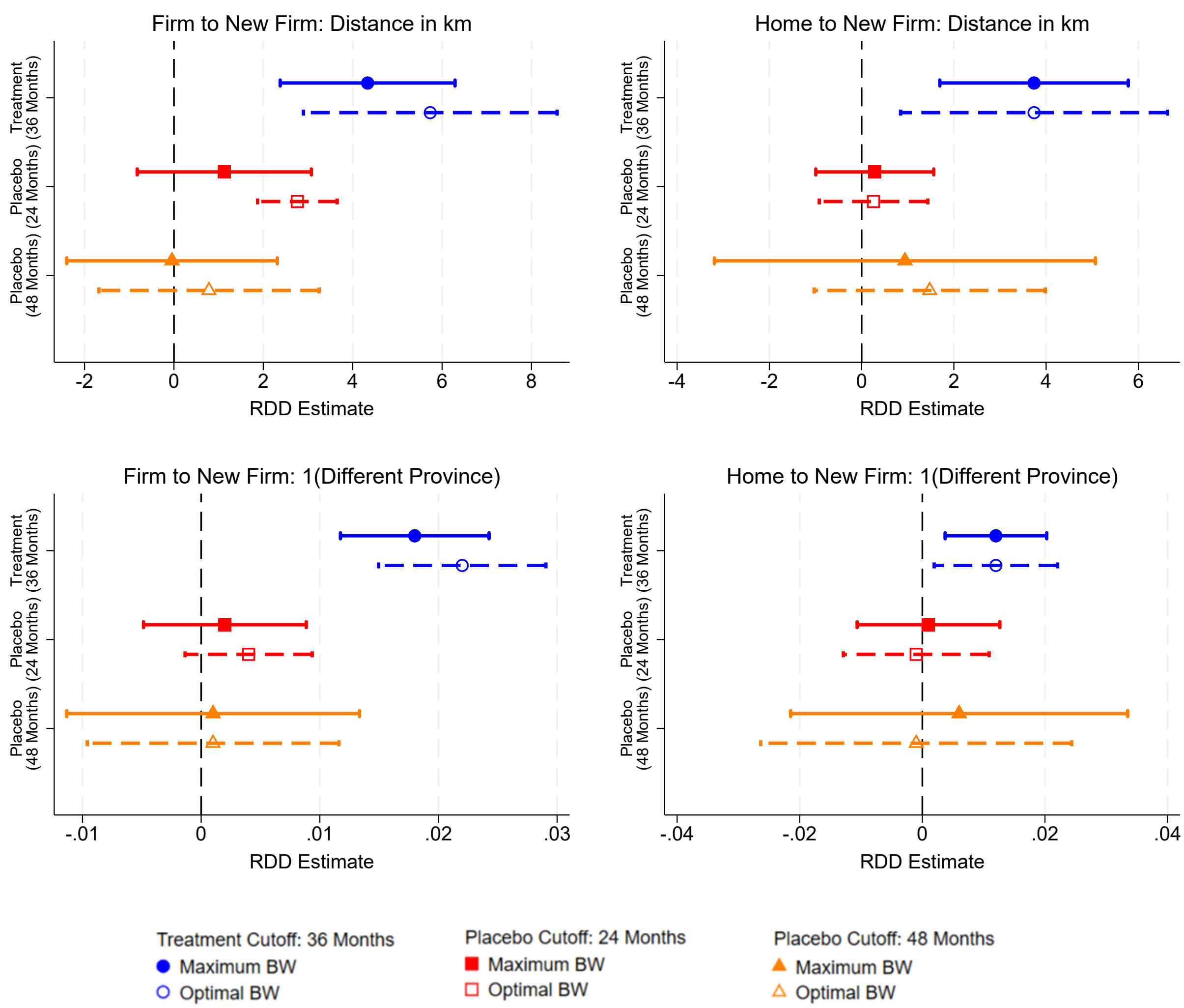
Notes: This figure plots point estimates and 95%-confidence intervals for RDD estimations using different bandwidths. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

Figure A2: Donut Estimations



Notes: This figure plots point estimates and 95%-confidence intervals for donut-RDD estimations using different sized donut holes. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

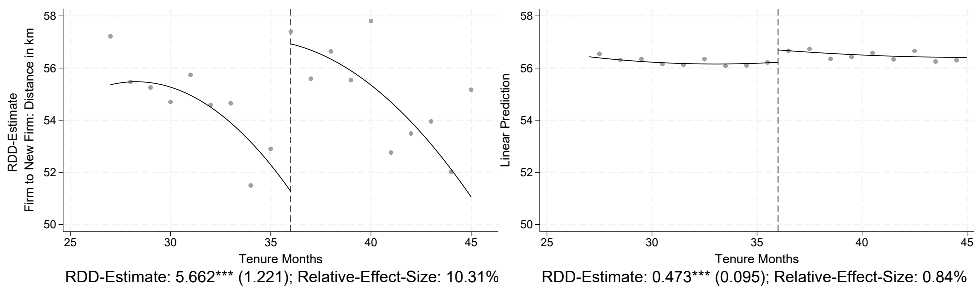
Figure A3: Placebo Tests



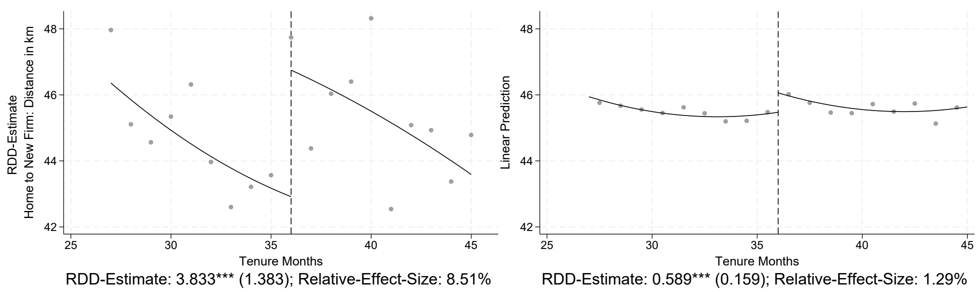
Notes: This figure plots point estimates and 95%-confidence intervals for different RDD estimations. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). All estimations use triangular kernel functions. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

Figure A4:
Discontinuities in Outcome Variables (left) and Predicted Composite Covariate Index (right)

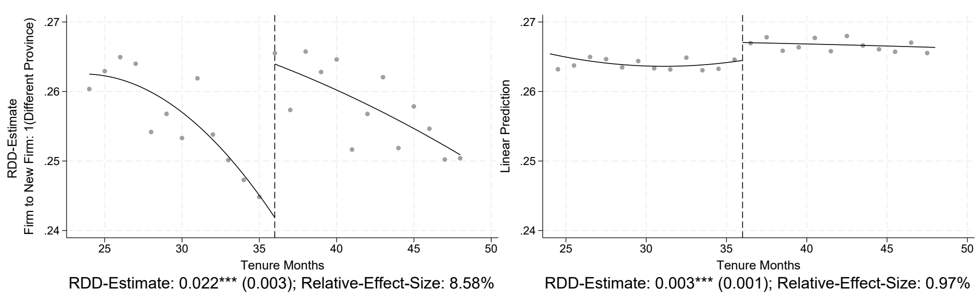
(a) Firm to New Firm: Distance in km



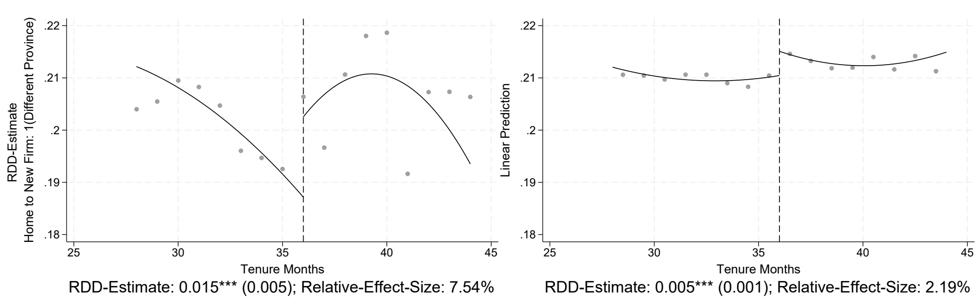
(b) Home to New Firm: Distance in km



(c) Firm to New Firm: 1(Different Province)



(d) Home to New Firm: 1(Different Province)



Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. All estimations use triangular kernel functions. The Composite Covariate Indexes in the right column are the fitted values from an OLS regression of the unbalanced covariates from Figure 2 (i.e, the wage rate and the age at separation and their squared values) on the respective dependent variables. Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

Table A1: Kernel Robustness

		Baseline	
Kernel Function:	Uniform	Triangular	Epanechnikov
	(1)	(2)	(3)
Panel A: Firm to New Firm: Distance in km			
Severance Pay	5.526***	5.662***	5.764***
	(1.132)	(1.221)	(1.324)
Relative Effect Size (in %)	10.1	10.3	10.5
N	142167	126050	126050
Panel B: Home to New Firm: Distance in km			
Severance Pay	3.571***	3.833***	3.989***
	(1.291)	(1.383)	(1.512)
Relative Effect Size (in %)	7.9	8.5	8.9
N	59612	52966	52966
Panel C: Firm to New Firm: 1(Different Province)			
Severance Pay	0.022***	0.022***	0.022***
	(0.003)	(0.003)	(0.003)
Relative Effect Size (in %)	8.6	8.6	8.4
N	196105	179277	179277
Panel D: Home to New Firm: 1(Different Province)			
Severance Pay	0.021***	0.015***	0.016***
	(0.006)	(0.005)	(0.006)
Relative Effect Size (in %)	10.3	7.5	7.9
N	53572	46790	46790

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).

Figure A5: Elibible for Extended UI-Benefits (Share)

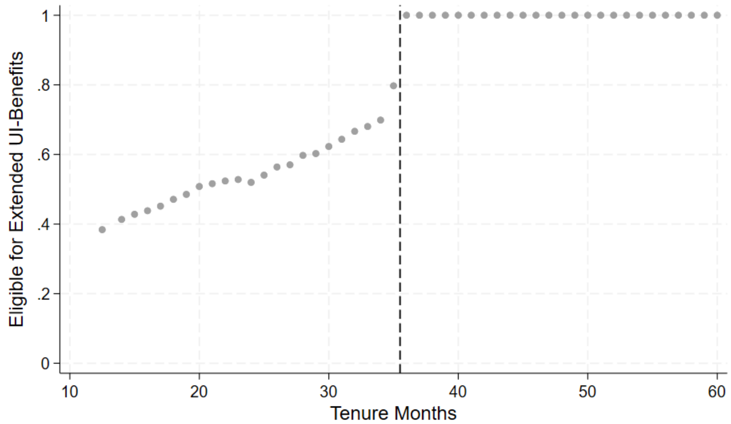


Table A2: Extended UI-Benefit Eligibility

	Distance in km		1(Different Province)	
	Firm to New Firm (1)	Home to New Firm (2)	Firm to New Firm (3)	Home to New Firm (4)
Panel A: Baseline Estimations (Optimal Bandwidth)				
Severance Pay	5.662***	3.833***	0.022***	0.015***
	(1.221)	(1.383)	(0.003)	(0.005)
Relative Effect Size (in %)	10.3	8.5	8.6	7.5
N	126050	52966	179277	46790
Panel B: Only Spells Eligible for Extended UI-Benefits (Optimal Bandwidth)				
Severance Pay	3.997***	5.477***	0.024***	0.021***
	(1.225)	(1.421)	(0.004)	(0.005)
Relative Effect Size (in %)	7.2	12.1	9.4	10.1
N	100210	44963	137305	40076
Panel C: Only Spells Eligible for Extended UI-Benefits (Maximum Bandwidth)				
Severance Pay	4.446***	4.320***	0.022***	0.014***
	(0.900)	(1.053)	(0.003)	(0.006)
Relative Effect Size (in %)	7.9	9.4	8.3	6.8
N	300406	141732	306140	143293

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors are clustered by discrete tenure months. Optimal bandwidths are estimated using the procedure described in Calonico, Cattaneo, and Farrell (2020). Driving distances are calculated using the Open-Streets-Map application in the OSRM-package in R (Giraud, 2022).