The Effects of Subsidized Transit on the Unemployed Youth: A Regression Discontinuity Design

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Abstract

I assess the effects of a simple and low-cost intervention targeted at the youth: subsidized public transport. Using a regression discontinuity design in the Spanish region of Madrid, I examine the short-term effects of a price cut in public transport that reduced job search and commuting costs for unemployed youths under 26. In particular, I compare the future labor market outcomes of unemployed assistance recipients who were laid off *just* before and after turning 26. Results suggest that subsidized transit may bring meaningful employment gains for young assistance recipients. Specifically, I estimate a (local) treatment effect of 23 percentage points on the job-finding probability and 30 days on the number of cumulative days in work six months after layoff. Finally, I find supporting evidence that these gains are driven by increased geographical mobility among those living farther away from jobs.

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1 Introduction

The youth are three times more likely to be unemployed than adults, a ratio similar to that observed over a decade ago (ILO, 2017, 2020). Moreover, young people have lower-quality jobs, with a growing number of them facing unstable and precarious employment (O'Higgins, 2017). To address these pressing concerns, policymakers typically use "active labor market policies" to help young unemployed people find work —including training, job-search assistance, subsidized labor, and public employment programs. And yet, these traditional interventions have shown, on average, small effects for the youth (Kluve et al., 2019).

A key factor likely explaining these findings is the limited institutional capacity many countries face in designing and implementing these labor market policies. Indeed, in many poor and rich countries alike, training programs are poorly targeted at labor demand, and public employment services are heavily understaffed (e.g., Angel-Urdinola et al., 2012; Escudero et al., 2016; OECD, 2018a,b; EU, 2019a,b; Martin, 2022). Moreover, employment initiatives targeting the youth are often fragmented and ad hoc (ILO, 2022).

With this policy context in mind, this research investigates the labor market effects of a *simple and low-cost* activation policy. In particular, I examine the effects of subsidizing public transport for the youth in urban regions with large and well-integrated public transport networks. This policy may prove cost-effective for two reasons. First, it is a straightforward-to-implement intervention that builds on the *existing* resources of urban areas with good-quality mass transport. Second, lower transport costs may boost labor market outcomes by encouraging jobseekers to *search farther away* from home and accept jobs with otherwise high *commuting* costs (see Gobillon et al. (2007) for a review of the "spatial mismatch" literature supporting these mechanisms). Moreover, more affordable transportation may not only allow for a broader job-search radius but also one with *better* job opportunities. In particular, cheaper transport may help those living in *peripheral* areas to search for jobs in *central and denser* areas benefiting from agglomeration economies. Better access to these areas may help workers find jobs faster (Di Addario, 2011) and provide them with valuable and portable experience with long-lasting returns (Roca and Puga, 2017). These search mechanisms for subsidized transport are likely to be particularly relevant for cash-constrained jobseekers with poor social networks (Picard and Zenou, 2018) and reliant on public transportation (Patacchini and Zenou, 2005).

In this research, I focus on *young* jobseekers claiming (means-tested) unemployment assistance benefits. To examine the causal effects of subsidized transit on their future labor market outcomes, I exploit the regression discontinuity design created by an *age-based eligibility cutoff* for a transit subsidy in the Spanish region of Madrid. More precisely, since October 2015, all youths *under the age of 26* are eligible for a 20-euro monthly transit pass allowing journeys throughout the *entire* Madrid region, and to and from nearby municipalities in the bordering provinces of Toledo, Guadalajara, and Cuenca. Importantly, this pass, known as "*abono jóven*" in Spanish, is offered to *all* youths under 26 *regardless of* their employment status. The pass may allow them to save between 35 and 112 euros per month. For those searching for work and relying on unemployment assistance, this saving equals between 7 and 23% of unemployment benefits.

Using social security records, I compare the future labor market outcomes of unemployment assistance entrants laid off at ages *just* under and over 26. The outcomes I examine are the probability to find a job, the number of cumulative days in employment and cumulative earnings, measured one to six months after layoff. Additionally, in order to test for potential mechanisms, I construct a discrete measure of geographical mobility capturing job finding *outside the home area*.

Results suggest that subsidized transit may bring meaningful employment gains for young assistance recipients around age 26. More precisely, I estimate a (local) treatment effect of 23 percentage points on the job-finding probability and 30 days on the number of cumulative days in work six months after job loss. Moreover, I find supporting evidence that these gains are driven by increased geographical mobility *from outer to inner* areas of Madrid, where labor demand is concentrated. Treatment effects on earnings are positive but imprecisely estimated.

This research relates to the literature on active labor market policies in developed countries —see Crépon and Van Den Berg (2016); Card et al. (2018); Vooren et al. (2019) for a general review of the literature and Kluve et al. (2019) for a review focused on the youth. Compared to most previous research on this strand of the literature, I aim to understand the labor market effects of a *simple, low-cost* activation policy. Furthermore, I focus on a Southern European country with long-standing resource and capacity constraints to deliver activation programs, and one of Europe's highest youth unemployment rates.

More closely related to this research is the recent empirical literature studying the effects of low-cost interventions to enhance job search. Two relevant papers are those by Altmann et al. (2018) and Belot et al. (2019) focused on informational frictions in the labor market. Altmann et al. (2018) study the effects of sending out the unemployed a brochure aimed to motivate and inform them about key aspects of the job-search process. The authors find that this simple brochure increases earnings and cumulative days in employment for jobseekers at risk of long-term unemployment —although, the authors exclude youths (under 25) from their sample. Belot et al. (2019) assess the effects of providing jobseekers with online tailored advice to search for alternative occupations to their preferred one. Their results suggest that this online support broadens the set of occupations that jobseekers target and increases the number of interviews they get, especially for those who initially searched narrowly and had been unemployed for more than two months. Nonetheless, the authors cannot provide evidence of their intervention boosting employment outcomes.
This paper adds to this literature by assessing a potentially *complementary* cost-effective intervention to foster the labor market outcomes of the unemployed youth by subsidizing public transport to encourage them to broaden their job-search radius.

This research also builds on and contributes to the urban economics literature assessing the effects of transport subsidies to *job search*. This empirical literature focuses on a markedly different or narrower pool of unemployed workers from the one I study, or on private rather than public transport subsidies. For instance, Franklin (2018) and Abebe et al. (2021) assess the employment effect of a public transport subsidy on young unemployed jobseekers in urban Ethiopia, while Phillips (2014) on African-American unemployed jobseekers from economically disadvantaged neighborhoods in Washington DC. Overall, these studies show positive effects on employment, although these effects fade over time. In addition, Le Gallo et al. (2017) study the effects of providing young, mostly jobless individuals with a 1000-euro voucher for driving lessons. The authors find that while this subsidy to private transport increased car ownership, it only had limited effects on employment.

Similarly, this research contributes to the literature evaluating the effects of *subsidized commuting*. For example, Paetzold (2019) shows that commuter tax breaks encourage employees to travel longer distances. Similarly, Caliendo et al. (2022) find that "mobility programs", including commuting and relocation subsidies *conditional* on having a concrete job offer, lead the unemployed to expand their search radius and accept jobs in distant *regions*. Nonetheless, the authors also find that these programs hamper re-employment and earnings. In view of these unintended consequences, Caliendo et al. (2022) hypothesize that searching for jobs in distant regions may come at the expense of lower search effort in *local* ones, where jobskeers live, likely know much better and, hence, where their job search may be more efficient. In contrast to this study, I focus on the effects of subsidizing commuting at an *intra-regional* level, for which the previous search efficiency concerns are likely of second-order importance.

Finally, this paper builds on the findings of Arranz et al. (2019). The authors also study the youth transit pass offered in the Madrid region but focus on its effects on household spending on transport. Using a difference-in-differences model, they find suggestive evidence that the 20-euro flat price set in 2015 encouraged the *poorest* households to use *more* public transport. Their findings are thus consistent with subsidized transport allowing *cash-constrained* job seekers to expand their search to more distant areas they would not have searched otherwise.

This paper is organized as follows. Section 2 details the institutional background of the policy rule I exploit. Section 3 describes the data and estimation sample. Section 4 outlines the empirical strategy. Section 5 presents the results, including tests for the potential mechanisms. Section 6 discusses robustness checks, and Section 7 concludes.

2 The Institutional Background

Madrid is one of the largest metropolitan regions in Europe and offers a well-integrated, extensive, and affordable public transport network, particularly for the youth. Indeed, a 20-euro monthly pass allows individuals *under the age of 26* to travel throughout the *entire* region and all public transport modes (bus, tram, metro, and train). Importantly, this youth pass, known as "*abono jóven*" in Spanish, is offered to *all* individuals under age 26, *irrespective of* their employment status. Moreover, it also allows journeys to and from nearby municipalities located in the bordering provinces of Toledo, Guadalajara, and Cuenca.

In contrast, regular (full-price) passes not only cost more but follow a zone-based pricing scheme, whereby those with longer commutes (crossing more transit zones) pay more.[1] For instance, those over age 26 commuting to Madrid's center and living in the *farthest* zone pay 132 euros for a monthly pass, while their counterparts already living in the *closest* zone pay 55 euros. On the contrary, given the flat price for a youth pass, those under age 26 pay only 20 euros, regardless of their travel distance, thus saving from 35 to 112 euros per monthly pass.

This generous subsidy policy for young people started in October 2015 when the regional government of Madrid made two important changes to reduce transit costs for

[1]Madrid's commuting area is divided into six concentric zones in the region, around the capital city, and two outward zones, including adjacent municipalities in Toledo, Guadalajara, and Cuenca.

the youth. First, it increased the age limit for youth passes from 23 *to 26*. Second, it set a *flat* rate for all youth passes, which until then had a zone-based pricing scheme, as the regular (full-price) passes. These changes affected youths differently. Those under age 23, who were already eligible for a youth pass, solely benefited from the new flat pricing scheme, saving an additional 15 to 42 euros per monthly pass. However, those between the ages of 23 and 26 gained both eligibility for a youth pass and a larger subsidy level, given its new flat rate. Indeed, before October 2015, these youths not only paid a zone-based price for a transit pass but the regular unsubsidized price. The policy changes in October 2015 thus allowed them to save between 35 to 112 euros per monthly pass.

In this research, I focus on these youths aged 23 to 26 benefiting from a fairly generous transit subsidy after October 2015. In particular, I exploit the age-26 cutoff for a youth pass to assess the causal effect of subsidized transit on young *cash-constrained job seekers*. More precisely, using the regression discontinuity design created by the age-26 cutoff, I compare the labor market outcomes of *unemployment assistance (UA) entrants* laid off at ages *just* under and over 26.

3 Data

I use data from the "Continuous Sample of Working Lives with Tax Records" for the reference years 2006-2019. This data set allows tracking the employment history of a 4% random sample of all individuals in Spain who pay to or receive social security. These include employed workers and recipients of unemployment benefits. Notably, I observe the start and end dates of all their employment contracts and unemployment assistance spells.

The unit of analysis is a job separation resulting in a UA claim. To construct my analysis sample, I consider job separations between October 1, 2015, and June 30, 2019. These are thus layoffs that occurred *after* the age-26 cutoff for a youth pass went into effect. By restricting the sample to those occurring before July 2019, I am able to observe future labor market outcomes up to six months after layoff.

Out of the resulting UA claims, I make some further sample restrictions. First, I focus on job separations observed in the region of Madrid or the provinces of Toledo, Guadalajara, and Cuenca. As mentioned in the previous section, the youth pass also allows travel to and from nearby municipalities in these bordering provinces. Second, I exclude UA entrants over age 50 since they are eligible for unlimited assistance benefits within at most five years.[2] Third, I exclude individuals having any part-time job when entering UA ("part-time unemployed jobseekers"). These individuals likely had weaker economic incentives or tighter time constraints to move around and broaden their search area than those without any job. Fourth, I also exclude UA entrants who were recalled by their last employer within three months or who were under a permanent contract with discontinuous involvement so as to drop those who were on temporary layoff. Finally, I exclude those entering UA after working as civil servants, apprentices, self-employed;

[2]Between 2012 and 2019, UA recipients over 55 were eligible for unlimited benefits until retirement. It is worth noting though that excluding UA entrants over 50 is a non-binding sample restriction as I will ultimately estimate the effects of subsidized transit using a "narrow" window around the age-26 cutoff. Following (Calonico et al., 2014), this window is chosen to optimize the bias-variance trade-off of including observations far away from the cutoff.

in the agriculture, fishing, mining, or other extractive industries; or under any atypical work contract as reported in the Spanish social security records, any remaining special contribution regime, or multiple job contracts.[3] The final (broad) sample includes 8,158 layoffs resulting in a UA claim from 4,206 individuals. The median number of layoffs is one.[4]

To measure eligibility for a youth pass, I calculate the approximate age of individuals at the time of layoff by taking the difference between the *exact* date of layoff and the *approximate* date of birth. For anonymity reasons, the Spanish social security data reports birth dates only up to the month-year. I impute the missing day of birth as the 15th. Therefore, workers may be up to 15 days younger or older than their approximate age at layoff. In Section 6, I show that the results of this research are not sensitive to this measurement error. To check for this, I exclude individuals within 15 days under or over the age-26 cutoff.

The main outcomes I examine are job finding, cumulative days in employment, and earnings *one to six months after layoff*. Job finding equals one if the job seeker found a job during the observation window, irrespective of how long it lasted, and zero otherwise. Cumulative days in employment is defined as the total number of days worked over the observation window, regardless of whether they were sporadic or at different firms, accounting for work hours.[5] By construction, this outcome equals zero if the job seeker did not find any job. Similarly, cumulative earnings are defined as the total labor income accumulated over the observation window. Therefore, they equal zero if the job seeker always remained jobless, or otherwise, the total income from all employment contracts held.

As discussed in Section 1, subsidized transit is likely to boost job finding and match quality by encouraging job seekers to expand their search radius to more distant areas away from home. To test this hypothesis, I build a measure of geographical mobility using data on residence (origin) and work location (destination). For anonymity, these start and end points are observed at the municipality level *only if* they fall within municipalities with more than 40,000 inhabitants; otherwise, they are observed at the province level. The data thus rules out any continuous measure of geographical mobility, such as commuting time or distance. To circumvent this limitation, I construct a *discrete* mobility measure, distinguishing between *outer and inner areas* of the Madrid region. This classification leverages the region's administrative division, whereby small municipalities (with less than 40,000 inhabitants) are concentrated in the *peripheral* area, while large municipalities (with more than 40,000 inhabitants) in the *central* area.

Figure 1 depicts this administrative division using 2019 population data from the National Statistics Institute of Spain. The figure plots the map of the Madrid region with small municipalities shaded in dark blue and large municipalities in light blue. The figure shows that, with few exceptions, small municipalities are located in the periphery, whereas large municipalities in the center. Building on this spatial pattern, I am therefore able to measure geographical mobility *across outer and inner areas*. Specifically, I define the inner area as comprising Madrid *city*, shaded in red in Figure

[3] This sample restriction on multiple contracts excludes only 5% of observations.

[4] I observe only one layoff for 65% of individuals, two layoffs for 19% of them, and three or more layoffs for 16%.

[5] The Continuous Sample of Working Lives provides data on the number of work hours of each contract held throughout an individual's employment history.

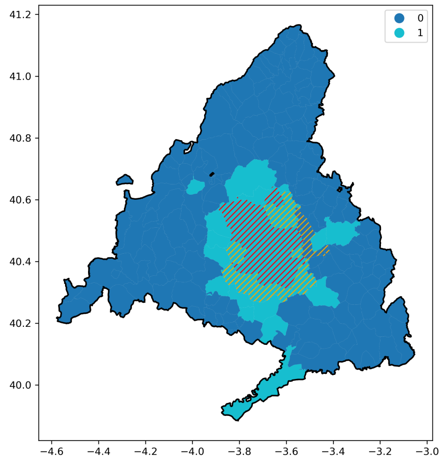
1, along with the surrounding cities shaded in orange.[6] This area covers the first two transit zones where the youth pass is valid. By construction, the outer area encompasses all remaining municipalities in the Madrid region, as well as those in the provinces of Toledo, Guadalajara, and Cuenca. Using this area classification, I examine the effect of the youth transit pass on job finding *outside the home area*. This outcome equals one if the job seeker found a job within the observation window and this (first new) job was *located* in an area *other than* his or her *residential area*; otherwise, the outcome equals zero. That is, for those living in outer Madrid, this outcome equals one only if they found a job in inner Madrid. For brevity, I shall refer to job finding outside the home area as "job mobility". In addition, I shall use the term "job mobility *from outer to inner areas*" to refer to finding a job in inner Madrid *conditional* on living in outer Madrid. Likewise, I shall use "job mobility *from inner to outer areas*" to refer to finding work in outer Madrid *for those* who live in inner Madrid.

Table 1 presents summary statistics for the *broad sample* of layoffs between October 2015 and June 2019 that resulted in a UA claim (column 1), and for layoffs *near the cutoff* (columns 2 and 3). More precisely, I estimate the effects of the youth pass using those layoffs occurring at ages *within a "narrow" bandwidth* around the age-26 cutoff. Following (Calonico et al., 2014), this bandwidth optimizes the bias-variance trade-off of excluding observations far from the cutoff. I estimate this mean squared error (MSE)-optimal bandwidth *for each* outcome and observation window using the broad sample of layoffs. Columns 2 and 3 of Table 1 report mean statistics for the sample of layoffs falling within the largest and smallest bandwidth, respectively. The first includes laid-off workers within roughly three years of the age-26 cutoff (those laid off at ages 23.1 to 28.9), while the second within roughly one year (those laid off at ages 24.85 and 27.15).

Table 1 shows that the unemployed job seekers I focus on are predominantly low-educated, live in the outer area of Madrid, were jobless for more than twelve months over the previous two years, and relied on temporary contracts in low-skill services occupations.

[6] In alphabetical order, these surrounding cities are: Alcobendas, Alcorcón, Coslada, Getafe, Leganés, Paracuellos de Jarama, Pozuelo de Alarcón, Rivas-Vaciamadrid, San Fernando de Henares, and San Sebastián de los Reyes.

Figure 1: Outer and inner areas of Madrid region



Notes: This figure plots the map of the Madrid region with small municipalities (with *less* than 40,000 inhabitants) shaded in dark blue, and large municipalities (with *more* than 40,000 inhabitants) in light blue. Madrid city is shaded in red. The surrounding cities, shaded in orange, fall within the second transit zone where the youth pass is valid. The inner area of Madrid region is defined as comprising the cities shaded in red and orange, whereas the outer area encompasses all remaining municipalities, as well as those in the provinces of Toledo, Guadalajara and Cuenca.

4 Empirical Setting

4.1 Regression Discontinuity Design

To assess the effects of subsidized transit on young UA entrants, I use the regression discontinuity (RD) design created by the age-26 cutoff for a youth transit pass. In particular, I compare the future labor market outcome of those who were laid off *just* under and over 26. Treatment is defined as *access to* a youth pass and is determined by age, both measured *at layoff*. To be consistent with the standard practice of having treated units to the right of the cutoff, I redefined the treatment assigment variable ("score") as the number of years *before the twenty-sixth birthday*. More precisely, I estimate the following local linear regression:

$$y_{is} = \alpha + \beta \times \mathbb{1}(x_{is} \geq 0) + f(x_{is}) + \epsilon_{is} \qquad \forall x_{is} \in [-h_{MSE}, h_{MSE}] \tag{1}$$

$$x_{is} \equiv 26 - age_{is}$$

$$f(x_{is}) \equiv \gamma x_{is} + \delta x_{is} \times \mathbb{1}(x_{is} \geq 0)$$

where y_{is} is an outcome of interest for individual i after the start of non-employment spell s, age_{is} is the age of i at the start date of s (measured in years), and x_{is} is the redefined score variable capturing the number of years left before turning 26. $f(\cdot)$ is a linear regression fit separately on each side of the age-26 cutoff. Observations are

Table 1: Descriptive statistics (mean)

	All layoffs	Near-cutoff layoffs within:	
		Largest bandwidth	Smallest bandwidth
	(1)	(2)	(3)
Individual characteristics			
Age	37.84	26.13	26.05
	[7.82]	[1.42]	[0.82]
Female	0.50	0.58	0.62
Foreign	0.19	0.19	0.17
Education attainment:			
Less than high school	0.73	0.71	0.70
High school	0.17	0.16	0.17
More than high school	0.09	0.12	0.13
Province of residence:			
Madrid region	0.64	0.71	0.71
Toledo	0.23	0.18	0.17
Guadalajara	0.08	0.06	0.06
Cuenca	0.05	0.05	0.07
Lives in outer area	0.65	0.61	0.62
Non-employment history (over last 2 years)			
Out of work < 6 months	0.03	0.03	0.03
Out of work 6-12 months	0.17	0.21	0.21
Out of work 12+ months	0.80	0.76	0.76
Last job characteristics			
Temporary contract	0.94	0.94	0.93
Part-time work	0.32	0.38	0.39
Low-skill occupation	0.86	0.84	0.84
Sector:			
Services	0.83	0.88	0.89
Construction	0.12	0.08	0.08
Industry	0.05	0.04	0.03
Outcomes			
Found job within 3 months	0.55	0.46	0.46
Found job within 6 months	0.71	0.65	0.65
Cum. days in work 3 months after	16.64	12.48	12.79
	[22.88]	[20.12]	[20.30]
Cum. days in work 6 months after	47.88	41.93	44.00
	[50.78]	[48.24]	[48.72]
Cum. earnings 3 months after (2016 euros)	836.61	456.77	474.53
	[1643.96]	[963.71]	[984.18]
Cum. earnings 6 months after (2016 euros)	2017.14	1400.56	1414.11
	[2775.55]	[2191.42]	[2186.17]
Observations	8158	850	499

Notes: This table presents mean descriptive statistics for the broad sample of layoffs between October 1, 2015, and June 30, 2019, resulting in a UA claim (Column 1). The effects of the youth pass are estimated on layoffs falling within a "narrow" bandwidth around the age-26 eligibility cutoff. This bandwidth is chosen to optimize the bias-variance trade-off of excluding observations far away from the cutoff, following (Calonico et al., 2014). This mean squared error (MSE)-optimal bandwidth is estimated for each outcome and observation window using the broad sample of layoffs. Columns 2 and 3 report mean statistics for the sample of layoffs falling within the largest and smallest bandwidth, respectively. The first includes laid-off workers within roughly three years of the age-26 cutoff (those laid off at ages 23.1 to 28.9), while the second within roughly one year (those laid off at ages 24.85 and 27.15). Standard deviations are reported in brackets.

weighted using a triangular kernel, and h_{MSE} is the mean squared error (MSE)-optimal bandwidth following Calonico et al. (2014).[7]

The parameter β captures any jump in average outcomes at the cutoff, after controlling for the score variable. More importantly, this parameter reflects the *causal* effect of eligibility for a youth pass under the assumption that individuals laid off *just* under and over 26 are comparable on all other aspects, except for their youth pass eligibility. Formally, the identifying assumption is that average potential outcomes are continuous functions of the score at the cutoff ($x_{is} = 0$). This continuity assumption allows us to use the average outcome of those just below the cutoff (barely over age 26) as a valid counterfactual for those just above the cutoff (barely under age 26).

Two important caveats are nonetheless worth considering in this age-based RD design (Lee and Lemieux, 2010). First, we are bound to estimate *short-run* treatment effects since job losers who are *initially under* 26 will eventually age and lose eligibility for a youth pass, thus no longer differing in their treatment status compared to those who are *initially over* 26.[8] Second, workers may adjust their behavior in anticipation of reaching the age threshold for a youth pass, thus potentially either accentuating or attenuating observed treatment effects. A simple and plausible story is that workers secure access to subsidized transit after their 26th birthday by purchasing a youth pass on that same day, the last day they are still entitled to do it. In this scenario, some control individuals, laid off at ages just over 26, would actually have access to a youth pass for up to a month, the length of time during which it is valid. To address this issue, in section 6, I show results from a donut-hole RD analysis dropping observations one month to the left and right of the cutoff.

4.2 Validity of the RD design

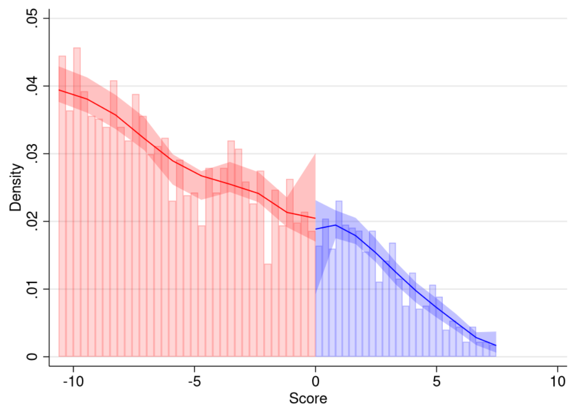
In this section, I assess the validity of the continuity assumption for potential outcomes. This assumption requires that individuals do not have precise control over the treatment assignment variable; otherwise, we could expect endogenous sorting around the discontinuity threshold. In this particular setting, a potential threat to this assumption is that firms may strategically adjust the timing of layoff in response to the age-26 cutoff for a youth pass. This behavior would create endogenous sorting and thus invalidate RD estimates if, for instance, firms laid off "less productive" workers always right after paying for their commuting costs becomes relatively expensive (when workers are no longer eligible for subsidized transit) while "more productive" workers at any time at random.

As a first validity check, I test whether the density of layoffs is continuous at age 26 ($x_{is} = 0$). Any jump at this cutoff would suggest that firms could precisely time their firing decisions (potentially to avoid paying for relatively high commuting costs). Using the density test by Cattaneo et al. (2020b), I fail to reject the null hypothesis that the density of layoffs is continuous at the cutoff (p-value=0.1313). Figure 2 illustrates this result by showing a histogram of the score x_{is} and a local polynomial density function fit separately on either side of the cutoff. The figure shows similar density estimates right

[7]Results are robust to using a quadratic polynomial on the score and a uniform kernel.

[8]In the spirit of Lee and Lemieux (2010), for the sake of argument, imagine we compared the labor market outcomes of those who were laid off at age 25.5 (eligible for a youth pass) to those laid off at age 26.5 (ineligible for a youth pass) *five years after lay off*. In this example, by the time we measure outcomes, those who were *initially* eligible and ineligible will be exposed to treatment over a fairly similar length of time (10 versus 0 percent of the 5-year observation window).

Figure 2: Histogram and Estimated Density of the Score

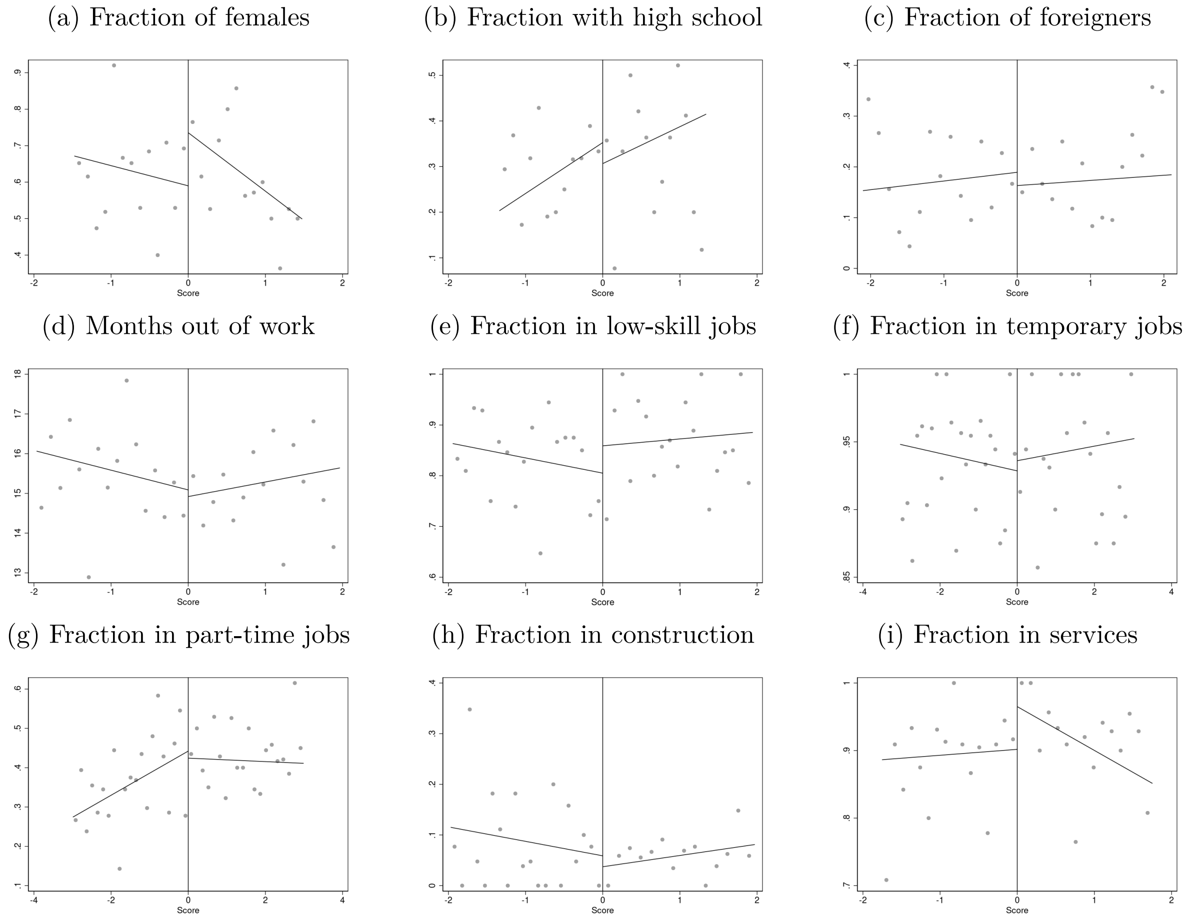


Notes: This figure shows: (i) the histogram of the normalized score variable (number of years before turning 26 at the time of layoff), and (ii) a local polynomial density estimate, fit separately on either side of the cutoff (solid blue and red), with 95% confidence intervals (shaded blue and red). This density estimate and the associated confidence intervals are computed following Cattaneo et al. (2020b).

below and above the cutoff, with the associated 95% confidence intervals (in shaded blue and red) overlapping.

Along the same lines, I test whether predetermined covariates change smoothly at age 26. In the absence of precise control over the score, treated and control units near the cutoff should be comparable in all characteristics determined before treatment. To check for this balance, I estimate Equation 1 for a set of observable covariates, treating each as the dependent variable. The parameter β now captures any potential discontinuity at the cutoff in a given *covariate*. Figure 3 plots average covariates against the score variable. Solid lines display the resulting local linear regressions using Equation 1. Reassuringly, these local regressions show no significant jump in any covariate at the cutoff. Indeed, while some show more noticeable discontinuities, none is statistically different from zero. Table A1 in the Appendix reports the corresponding RD estimates and standard errors. This finding therefore provides further evidence supporting the continuity assumption for potential outcomes.

Figure 3: Local Linear RD Effects for Predetermined Covariates



Notes: This figure plots average covariates against the normalized score (number of years before turning 26 at the time of layoff). The vertical line indicates the normalized cutoff for a youth transit pass. Solid lines plot local linear regressions fit separately on either side of the cutoff using Equation 1, with each covariate as the dependent variable. Months out of work in plot (d) is the number of months in nonemployment over the two years before layoff. Plots (e) to (i) correspond to the characteristics of the last job.

5 Results

This section presents results on the effects of the youth transit pass on future labor market outcomes. The analysis focuses on unemployed job seekers, specifically those who claimed UA benefits upon job separation. Outcomes are measured one to six months after layoff. First, I discuss main effects six months after layoff and show how these effects emerge over time. Second, I present supporting evidence of the potential mechanisms.

5.1 Main Effects

Figure 4 plots average labor market outcomes against the normalized score, which represents the number of years before turning 26 at the time of layoff. Displayed outcomes are measured over the first six months after layoff. The vertical line indicates the normalized cutoff for a youth pass. Units to the right of the cutoff comprise UA entrants who were laid off under 26 (treated), while units to the left those laid off over 26 (control). Solid lines plot linear regressions fit separately to either side of the threshold using Equation 1. The vertical distance at the cutoff between these two local regressions equals the RD estimate of the effect of the youth pass.

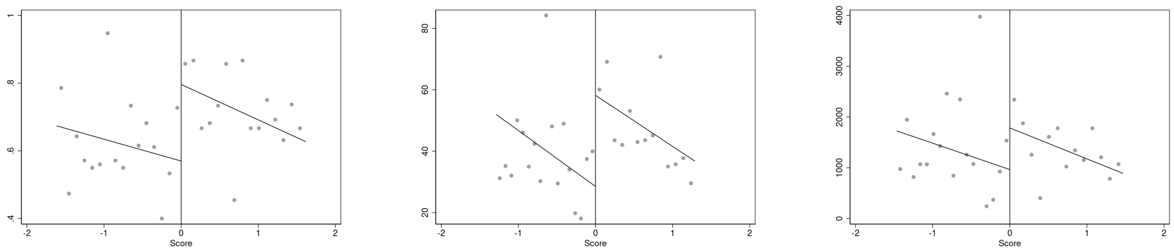
The figure shows that average outcomes tend to decrease with the score, consistent with the fact that younger individuals generally experience worse labor market outcomes. Nonetheless, the figure also shows an upward jump in average outcomes at the threshold. More precisely, Panel (a) of Figure 4 suggests a jump of about 20 percentage points in the job finding probability within six months of layoff. Similarly, Panels (b) and (c) show a jump of 30 days and roughly 800 euros in cumulative employment and earnings, respectively. These economically significant effects are statistically different from zero at the 5% level, except for the earnings effects, significant at the 10% level only (Table 2, last column). Altogether, these results suggest that the youth transit pass may not only help the unemployed find jobs faster but also remain employed longer. Moreover, the findings provide suggestive evidence that these employment gains may not come at the expense of lower earnings.

Figure 5 illustrates how the effect of the youth transit pass evolves over time. The x-axis indicates the number of months over which outcomes are measured, and the y-axis shows the corresponding RD estimates. The figure shows that treatment effects —especially on cumulative employment and earnings— emerge only gradually, consistent with job search being a time-consuming and costly process. It is worth noting that this increasing pattern *could* be seen as mechanical since I focus on *cumulative* outcomes measured over periods of *increasing* length (from one to six months after layoff). Thus, by construction, these outcomes are non-decreasing over time. Still, treatment *effects* may actually weaken or vanish if control individuals start to catch up or the treated simply fall behind. For instance, the effects on cumulative days in employment could fade away if the treated had gained an initial advantage only through *very short-term* jobs and their control counterparts slowly caught up with longer-lasting matches. Therefore, at a minimum, treatment effect estimates in Figure 5 suggest that treated individuals may not only be at an advantage one month after layoff but also *remain* so six months after.

Table 2 reports the RD estimates and standard errors for all outcomes and observation windows. The table shows that, six months after layoff, treatment effects on job finding and cumulative days in employment represent an increase of 40 and 104 percent, respectively, relative to the control mean. In addition, the treatment effect on cumulative earnings corresponds to an increase of 85 percent, although this effect is only significant at the 10% level.

Figure 4: RD Estimates of the Labor Market Effects of the Youth Transit Pass

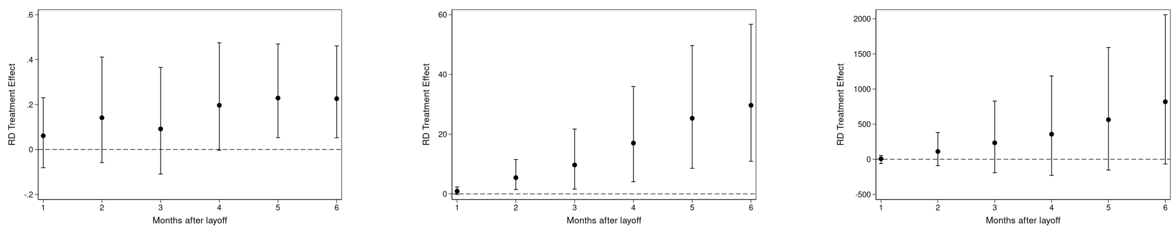
(a) Job finding rate (b) Cum. days in employment (c) Cum. earnings (2016 euros)



Notes: This figure plots average labor market outcomes against the normalized score (number of years before turning 26 at the time of layoff). Outcomes are measured six months after layoff. The vertical line indicates the normalized cutoff for a youth pass. Solid lines plot local linear regressions fit separately on either side of the cutoff using Equation 1.

Figure 5: Evolution of the Labor Market Effects of the Youth Transit Pass

(a) Job finding rate (b) Cum. days in employment (c) Cum. earnings (2016 euros)



Notes: This figure plots RD estimates and the associated 95% robust bias-corrected confidence intervals for the effects of the youth transit pass. The estimates are plotted against the number of months after layoff over which outcomes are measured.

Table 2: RD Estimates of the Labor Market Effects of the Youth Transit Pass

	Panel A: Job finding rate					
	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.06	0.14	0.09	0.20	0.23	0.23
Robust Std. Error	0.08	0.12	0.12	0.12	0.11	0.10
Robust p-value	0.348	0.141	0.290	0.053	0.014	0.014
Observations	8158	8158	8158	8158	8158	8158
Optimal Bandwidth	2.09	1.44	1.51	1.47	1.61	1.62
Effective Obs. (Left)	352	244	260	255	273	273
Effective Obs. (Right)	318	223	238	230	248	248
Control Mean	0.17	0.27	0.42	0.47	0.52	0.57
	Panel B: Cumulative days in employment					
	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.87	5.42	9.67	17.02	25.31	29.66
Robust Std. Error	0.63	2.56	5.13	8.15	10.49	11.70
Robust p-value	0.096	0.011	0.023	0.014	0.006	0.004
Observations	8158	8158	8158	8158	8158	8158
Optimal Bandwidth	1.99	1.30	1.24	1.15	1.18	1.29
Effective Obs. (Left)	342	225	219	204	210	224
Effective Obs. (Right)	305	200	189	180	182	197
Control Mean	0.43	2.08	7.05	13.37	20.17	28.52
	Panel C: Cumulative earnings (2016 euros)					
	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	6.74	111.12	234.22	357.73	563.52	819.14
Robust Std. Error	29.53	120.46	260.27	361.10	445.34	542.16
Robust p-value	0.915	0.227	0.221	0.185	0.107	0.066
Observations	7612	7145	6802	6514	6259	6010
Optimal Bandwidth	1.92	1.46	1.45	1.50	1.47	1.47
Effective Obs. (Left)	305	213	195	194	182	176
Effective Obs. (Right)	279	199	189	184	170	166
Control Mean	27.76	114.28	306.74	506.70	710.71	965.17

Notes: This table reports RD estimates of the labor market effects of the youth transit pass using Equation 1. Outcomes are measured one to six months after layoff. The estimation uses the mean squared error (MSE)-optimal bandwidth based on Calonico et al. (2014). Standard errors and p-values are computed using the robust bias-corrected inference procedure based on the same work.

5.2 Potential Mechanisms

As discussed in Section 1, subsidized transit may boost job finding and match quality by encouraging job seekers to expand their search to more distant areas and accept jobs with otherwise high commuting costs. Therefore, I now examine whether the observed employment effects of the youth transit pass may be attributed to increased geographical mobility. To further make the case that these effects are due to subsidized transit, I also examine mobility *by area of residence*. Since labor demand is heavily concentrated in the center of the Madrid region, we should see relatively large effects on mobility *from outer to inner* areas rather than the opposite. That is, any increase in geographical mobility should be driven by those who live far away from jobs and hence stand to benefit from expanding their job-search radius.

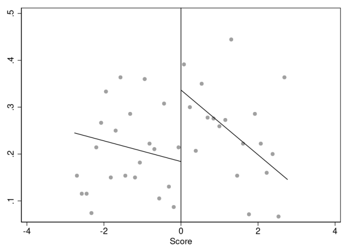
Figure 6 provides visual evidence supporting this spatial mechanism. Similarly to Figure 4, the figure plots average *mobility* outcomes against the normalized score, representing the number of years before turning 26 at the time of layoff. In particular, the outcome is job finding *outside the home area*, measured over the first six months after layoff. Panel (a) shows average outcomes for the full sample of UA entrants, while panels (b) and (c) for the subsamples living in the outer and inner areas of the Madrid region, respectively.

Figure 6 shows that overall job mobility jumps at the age-26 cutoff for a youth transit pass. Specifically, the jump in trend lines in panel (a) indicates that UA entrants *just under* 26 are 15 percentage points more likely to find a job outside their home area within six months of layoff than those *just over* 26. Moreover, the figure also reveals that this increased job mobility is driven by those living in outer areas of the Madrid region, farther away from jobs. Indeed, mobility trends spike by 25 percentage points for outer area residents but only by 6 percentage points for those in inner areas —panels (b) and (c), respectively.

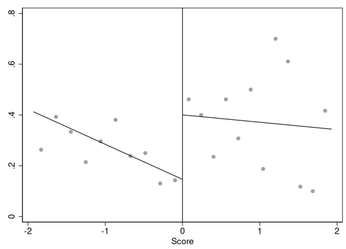
Figure 7 depicts how these mobility effects unfold over time. The figure shows increasing and statistically significant mobility gains for outer area residents by the end of the sixth month after layoff. In contrast, mobility gains for those living in inner areas remain relatively small and statistically insignificant. Table 3 reports the corresponding RD estimates and standard errors.

Figure 6: RD Estimates of the Mobility Effects of the Youth Transit Pass

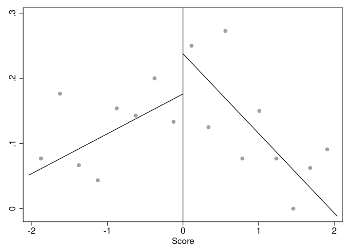
(a) Job mobility



(b) Job mobility from outer to inner areas

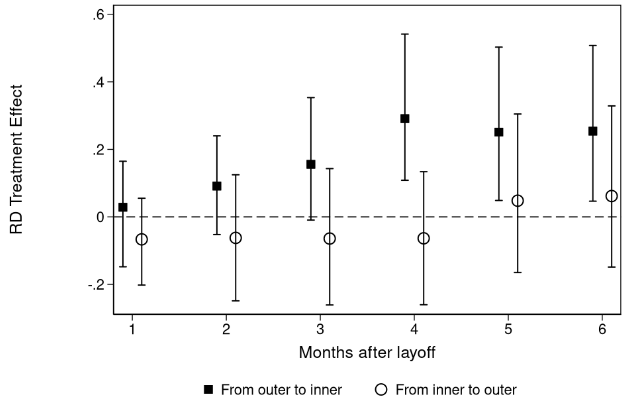


(c) Job mobility from inner to outer areas



Notes: This figure plots average job mobility outcomes against the normalized score (number of years before turning 26 at the time of layoff). Job mobility is defined as job finding outside the home area, as detailed in Section 3. Outcomes are measured six months after layoff. The vertical line indicates the normalized cutoff for a youth pass. Solid lines plot local linear regressions fit separately on either side of the cutoff using Equation 1.

Figure 7: Evolution of the Mobility Effects of the Youth Transit Pass



Notes: This figure plots RD estimates and the associated 95% robust bias-corrected confidence intervals for the job mobility effects of the youth transit pass. Job mobility is defined as job finding outside the home area, as detailed in Section 3. The estimates are plotted against the number of months after layoff over which outcomes are measured.

6 Robustness Checks

This section provides two types of robustness checks to validate the previous findings. First, I show that the age-26 effects so far reported are not sensitive to modeling choices such as adjusting for covariates, selecting alternative bandwidths, and excluding observations "very close" to the cutoff. Second, I present results from a *difference-in-discontinuities* design to address the potential concern that interventions other than the youth transit pass may also switch on or off at age 26.

6.1 Modeling choices

Adding covariates

Tables 4 and 5 report RD estimates for the effects of the youth transit pass *controlling for* a set of predetermined covariates (Panels A.2, B.2, and C.2). The estimates are obtained by adding covariates in a linear and additive-separable way in Equation 1. Covariates include controls for individual characteristics such as gender, education, nationality, and non-employment history over the previous two years; and last job characteristics such as type of contract, occupation skill level, and sector. Table 4 presents covariate-adjusted RD estimates of the labor market effects of the youth pass, and Table 5 of the mobility effects. Both tables show that the estimates of the youth pass effects remain quite stable when including covariates.

Table 3: RD Estimates of the Mobility Effects of the Youth Transit Pass

	Panel A: Job mobility across outer and inner areas					
	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.00	0.04	0.05	0.11	0.14	0.15
Robust Std. Error	0.05	0.07	0.07	0.07	0.08	0.08
Robust p-value	0.776	0.572	0.507	0.083	0.034	0.022
Observations	8154	8153	8151	8150	8150	8150
Optimal Bandwidth	1.59	2.43	2.84	2.90	2.82	2.77
Effective Obs. (Left)	269	424	509	517	507	493
Effective Obs. (Right)	248	372	411	420	408	402
Control Mean	0.05	0.13	0.16	0.19	0.19	0.18
	Panel B: Job mobility from outer to inner areas					
	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.03	0.09	0.16	0.29	0.25	0.25
Robust Std. Error	0.08	0.07	0.09	0.11	0.12	0.12
Robust p-value	0.916	0.209	0.063	0.003	0.017	0.019
Observations	5278	5277	5277	5276	5276	5276
Optimal Bandwidth	1.42	2.95	2.12	1.92	1.98	1.93
Effective Obs. (Left)	146	326	221	207	214	208
Effective Obs. (Right)	128	273	200	180	185	182
Control Mean	0.03	0.14	0.13	0.12	0.15	0.15
	Panel C: Job mobility from inner to outer areas					
	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.07	-0.06	-0.06	-0.06	0.05	0.06
Robust Std. Error	0.07	0.10	0.10	0.10	0.12	0.12
Robust p-value	0.263	0.515	0.567	0.529	0.559	0.460
Observations	2876	2876	2874	2874	2874	2874
Optimal Bandwidth	2.29	2.30	2.17	2.45	2.14	2.04
Effective Obs. (Left)	144	146	135	166	135	130
Effective Obs. (Right)	133	133	125	137	124	123
Control Mean	0.09	0.15	0.17	0.19	0.19	0.18

Notes: This table reports RD estimates of the job mobility effects of the youth transit pass using Equation 1. Job mobility is defined as job finding outside the home area, as detailed in Section 3. Outcomes are measured one to six months after layoff. The estimation uses the mean squared error (MSE)-optimal bandwidth based on Calonico et al. (2014). Standard errors and p-values are computed using the robust bias-corrected inference procedure based on the same work.

Table 4: RD Estimates of the Labor Market Effects of the Youth Transit Pass — Including Covariates

			Panel A: Job finding rate			
A.1 Without covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.06	0.14	0.09	0.20	0.23	0.23
Robust Std. Error	0.08	0.12	0.12	0.12	0.11	0.10
Robust p-value	0.348	0.141	0.290	0.053	0.014	0.014
Observations	8158	8158	8158	8158	8158	8158
A.2 With covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.03	0.12	0.10	0.21	0.23	0.23
Robust Std. Error	0.07	0.11	0.12	0.12	0.10	0.10
Robust p-value	0.656	0.160	0.248	0.030	0.011	0.010
Observations	8065	8065	8065	8065	8065	8065
			Panel B: Cumulative days in employment			
B.1 Without covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.87	5.42	9.67	17.02	25.31	29.66
Robust Std. Error	0.63	2.56	5.13	8.15	10.49	11.70
Robust p-value	0.096	0.011	0.023	0.014	0.006	0.004
Observations	8158	8158	8158	8158	8158	8158
B.2 With covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.64	5.23	9.71	18.28	27.90	32.30
Robust Std. Error	0.62	2.51	5.07	8.12	10.38	11.60
Robust p-value	0.233	0.012	0.020	0.008	0.002	0.001
Observations	8065	8065	8065	8065	8065	8065
			Panel C: Cumulative earnings (2016 euros)			
C.1 Without covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	6.74	111.12	234.22	357.73	563.52	819.14
Robust Std. Error	29.53	120.46	260.27	361.10	445.34	542.16
Robust p-value	0.915	0.227	0.221	0.185	0.107	0.066
Observations	7612	7145	6802	6514	6259	6010
C.2 With covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.91	123.13	279.51	444.87	699.28	1040.06
Robust Std. Error	30.30	117.99	257.55	357.17	448.96	544.28
Robust p-value	0.702	0.154	0.145	0.105	0.052	0.022
Observations	7523	7061	6722	6438	6184	5937

Notes: This table reports RD estimates of the labor market effects of the youth transit pass using Equation 1 with and without covariates (Panels A.2-C.2 and A.1-C.1, respectively). These include controls for individual characteristics (gender, education, nationality, and non-employment history over the previous two years) and last job characteristics (temporary contract, part-time work, occupation skill level, and sector). Outcomes are measured one to six months after layoff. The estimation uses the mean squared error (MSE)-optimal bandwidth based on Calonico et al. (2014). Standard errors and p-values are computed using the robust bias-corrected inference procedure based on the same work.

Table 5: RD Estimates of the Mobility Effects of the Youth Transit Pass — Including Covariates

	Panel A: Job mobility across outer and inner areas					
A.1 Without covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.00	0.04	0.05	0.11	0.14	0.15
Robust Std. Error	0.05	0.07	0.07	0.07	0.08	0.08
Robust p-value	0.776	0.572	0.507	0.083	0.034	0.022
Observations	8154	8153	8151	8150	8150	8150
A.2 With covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.01	0.04	0.05	0.11	0.13	0.14
Robust Std. Error	0.05	0.06	0.06	0.07	0.07	0.07
Robust p-value	0.617	0.632	0.497	0.080	0.047	0.029
Observations	8061	8060	8058	8057	8057	8057
	Panel B: Job mobility from outer to inner areas					
B.1 Without covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.03	0.09	0.16	0.29	0.25	0.25
Robust Std. Error	0.08	0.07	0.09	0.11	0.12	0.12
Robust p-value	0.916	0.209	0.063	0.003	0.017	0.019
Observations	5278	5277	5277	5276	5276	5276
B.2 With covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.02	0.10	0.18	0.30	0.27	0.27
Robust Std. Error	0.08	0.07	0.09	0.10	0.11	0.11
Robust p-value	0.982	0.145	0.030	0.001	0.006	0.007
Observations	5227	5226	5226	5225	5225	5225
	Panel C: Job mobility from inner to outer areas					
C.1 Without covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.07	-0.06	-0.06	-0.06	0.05	0.06
Robust Std. Error	0.07	0.10	0.10	0.10	0.12	0.12
Robust p-value	0.263	0.515	0.567	0.529	0.559	0.460
Observations	2876	2876	2874	2874	2874	2874
C.2 With covariates	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.06	-0.05	-0.04	-0.06	0.04	0.05
Robust Std. Error	0.06	0.10	0.10	0.10	0.11	0.11
Robust p-value	0.351	0.595	0.764	0.625	0.640	0.514
Observations	2834	2834	2832	2832	2832	2832

Notes: This table reports RD estimates of the job mobility effects of the youth transit pass using Equation 1 with and without covariates (Panels A.2-C.2 and A.1-C.1, respectively). These include controls for individual characteristics (gender, education, nationality, and non-employment history over the previous two years) and last job characteristics (temporary contract, part-time work, occupation skill level, and sector). Job mobility is defined as job finding outside the home area, as detailed in Section 3. Outcomes are measured one to six months after layoff. The estimation uses the mean squared error (MSE)-optimal bandwidth based on Calonico et al. (2014). Standard errors and p-values are computed using the robust bias-corrected inference procedure based on the same work.

Bandwidth selection

The RD effects reported so far are estimated using observations falling within the mean squared error (MSE)-optimal bandwidth proposed by Calonico et al. (2014). As mentioned in Section 3, this bandwidth is specific to each outcome and observation window, and is chosen to optimize the bias-variance trade-off of excluding observations far from the cutoff.

Notwithstanding this MSE optimality property, bandwidth selection is one of the most consequential modeling choices in RD analysis (Cattaneo et al., 2020a). I, therefore, now assess whether the RD results reported in Section 5 are robust to selecting alternative bandwidths. More precisely, I re-estimate the local liner regression in Equation 1 for bandwidths that are ± 30, 60, 90, 120, 150, and 180 days relative to the benchmark MSE-optimal bandwidth reported in Tables 2 and 3. That is, up to six months larger and smaller than the benchmark bandwidth.[9]

Figures 8 and 9 report the results of this sensitivity check. The x-axis shows the bandwidth used to estimate the effect of the youth transit pass, and the y-axis, the resulting RD estimate and associated 95% robust bias-corrected confidence interval. The vertical blue line indicates the benchmark MSE-optimal bandwidth. For brevity, bandwidth sensitivity checks for cumulative earnings and job mobility across outer and inner areas are reported in Figures A1 and A2 in the Appendix.

Figures 8-9 and A1-A2 show that the main empirical findings of this research are robust to using alternative bandwidths near the MSE-optimal bandwidth.

Donut-RD analysis

I now investigate how sensitive results are to units whose scores fall "very close" to the age-26 cutoff. Specifically, I exclude units within 15, 30, and 45 days of the cutoff and repeat the estimation outlined in Section 4. This sensitivity analysis addresses a number of potential concerns.

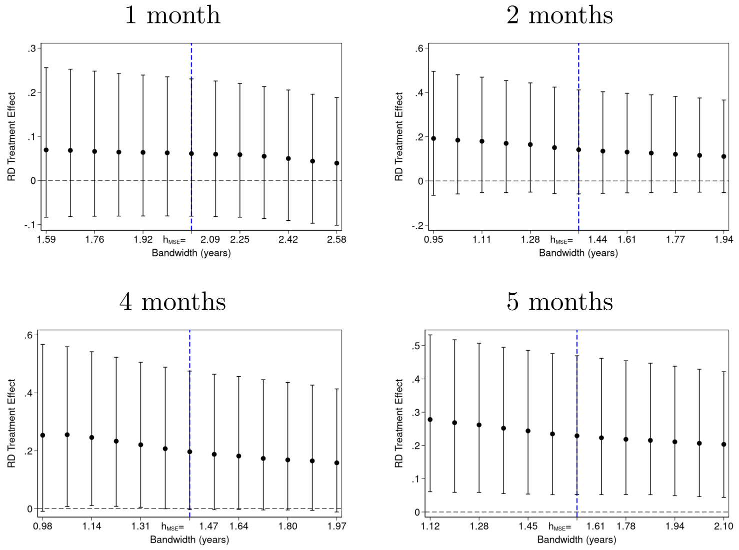
First, as detailed in Section 3, I observe workers' birth dates only up to the month and year and impute their missing *day* of birth as the 15th of the month. Hence, workers may be up to 15 days younger or older than their *approximate* age at layoff. To check whether results are sensitive to this measurement error, I exclude units within a 15-day range below or above the age-26 cutoff. Second, as mentioned in Section 4, workers may fully anticipate and *react* strategically to losing eligibility for a youth pass *before* they turn 26. In particular, they may secure access to subsidized transit after their 26th birthday by purchasing a youth pass on that same day, the last day they are still entitled to do it. If so, some *control* individuals laid off over 26 would actually have access to a youth pass for up to 30 days, the period it remains valid. I, therefore, assess whether results are robust to such potential anticipation behavior by excluding units 30 days to the left and right of the cutoff. Third, at a more general level, units "very close" to the cutoff are most likely to have manipulated their score and generated endogenous sorting. Furthermore, they are likely to have the greatest influence on the local polynomial estimation in RD analysis (Cattaneo et al., 2020a).

Figures 10 and 11 show the results of this "donut-hole" analysis excluding units "very

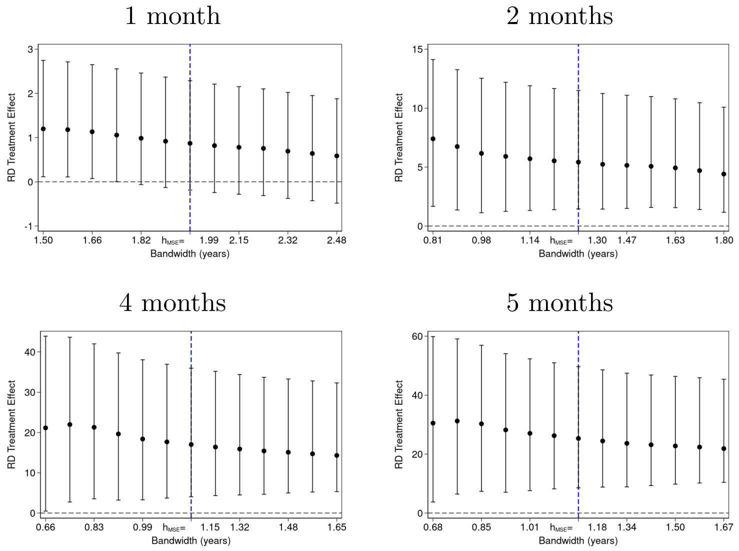
[9] Cattaneo et al. (2020a) and Cattaneo and Titiunik (2022) advise against investigating bandwidth sensitivity using bandwidths that are *much* larger (smaller) than the MSE-optimal one since, by construction, they will lead to RD estimates with too much bias (variance).

Figure 8: Bandwidth Sensitivity Check — Labor Market Effects

(a) Job finding rate



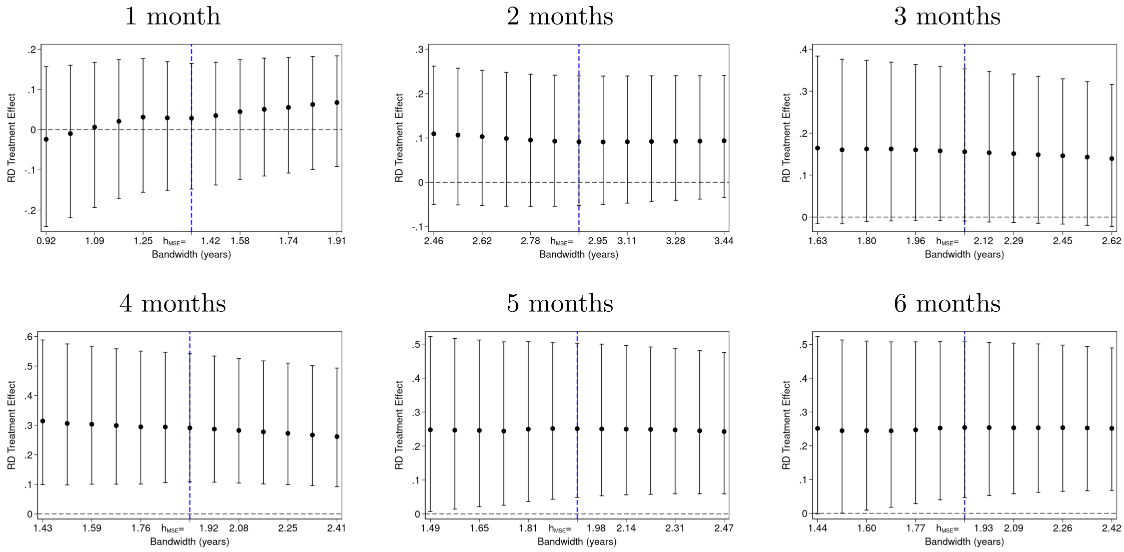
(b) Cumulative days in employment



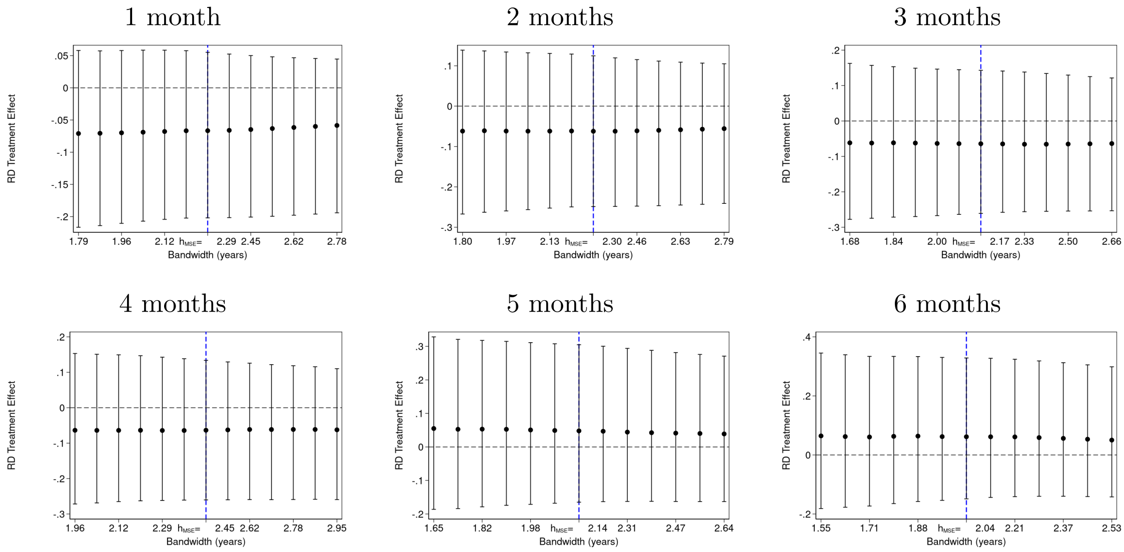
Notes: This figure plots RD estimates and the associated 95% robust bias-corrected confidence intervals for the labor market effects of the youth transit pass using different bandwidths around the cutoff. The bandwidths used are ± 30, 60, 90, 120, 150, and 180 days relative to the benchmark mean squared error (MSE)-optimal bandwidth reported in Table 2. Estimates are obtained using Equation 1. The vertical blue line indicates the MSE-optimal bandwidth. For brevity, Figure A1 in the Appendix plots the results of this bandwidth sensitivity check for the effects of the youth pass on earnings.

Figure 9: Bandwidth Sensitivity Check — Mobility Effects

(a) Job mobility from outer to inner areas



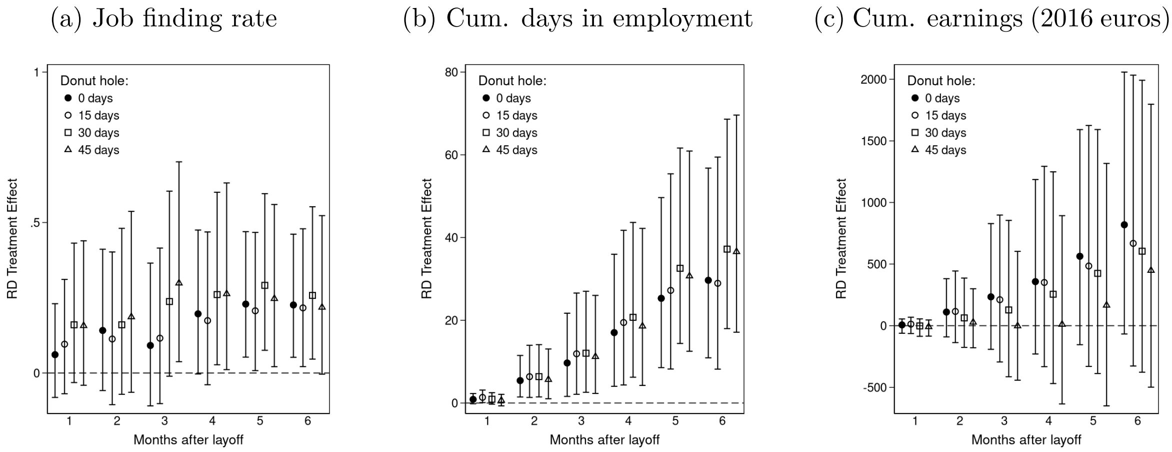
(b) Job mobility from inner to outer areas



Notes: This figure plots RD estimates and the associated 95% robust bias-corrected confidence intervals for the mobility effects of the youth transit pass using different bandwidths around the cutoff. The bandwidths used are ± 30, 60, 90, 120, 150, and 180 days relative to the benchmark mean squared error (MSE)-optimal bandwidth reported in Table 3. Estimates are obtained using Equation 1. The vertical blue line indicates the MSE-optimal bandwidth. For brevity, Figure A1 in the Appendix plots the results of this bandwidth sensitivity check for the effects of the youth pass on job mobility *across* outer and inner areas.

near” the cutoff. The x-axis indicates the observation window used to measure outcomes. The y-axis shows RD estimates after removing units falling in different neighborhoods, or “donut holes”, around the threshold. The RD estimates using a donut hole of zero days correspond to the baseline estimates using the full sample of layoffs. Reassuringly, Figures 10 and 11 show that estimates are largely unchanged by excluding units in the immediate vicinity of the age-26 cutoff.

Figure 10: Donut-Hole RD Analysis — Labor Market Effects of the Youth Transit Pass



Notes: This figure plots “donut” RD estimates and the associated 95% robust bias-corrected confidence intervals for the labor market effects of the youth transit pass. These estimates are obtained by excluding units within different neighborhoods, “donut holes”, around the cutoff and reestimating Equation 1 on the remaining sample. RD estimates using a 0-day donut hole correspond to the baseline estimates using the full sample of layoffs. The estimates are plotted against the number of months after layoff over which outcomes are measured.

6.2 Local randomization approach

Results reported so far have followed the “continuity-based” framework to RD analysis which relies on the assumption of continuity of potential outcomes at the cutoff. While this is the standard RD approach in empirical research, I now assess whether results remain similar under the local randomization framework. This alternative framework formalizes the idea that RD designs may be interpreted as randomized experiments near the cutoff (Cattaneo et al., 2023a). More specifically, under this approach, we assume that there exists a small window $\mathcal{W}$ around the cutoff where two conditions hold: 1) placement above or below the cutoff is (as if) randomly assigned (e.g., all units have the same probability of receiving all score values in the window), and 2) score values do not affect potential outcomes. Under these conditions, potential outcomes are not only continuous but also *flat inside* $\mathcal{W}$, and, thus, we may use the difference in *mean* outcomes between treated and control units inside $\mathcal{W}$ to estimate average treatment effects and make statistical inference.

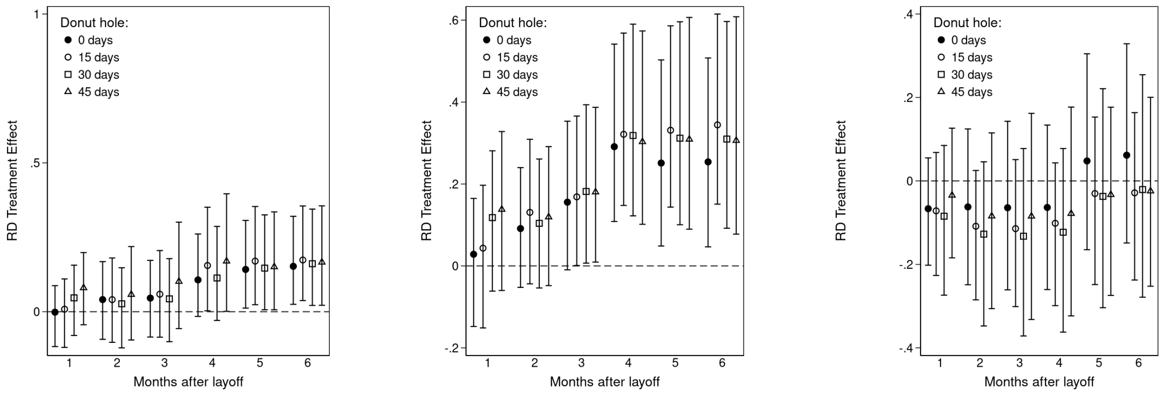
This approach requires two steps. First, we must choose $\mathcal{W}$ where the previous “as if random assignment” conditions are assumed to hold. Cattaneo et al. (2015) suggest using a data-driven procedure to find the largest window where predetermined covariates are balanced. Or, more formally, where we fail to reject the null hypothesis that treatment assignment is unrelated to predetermined covariates. Since failing to reject a false null hypothesis is the main concern in this step, Cattaneo et al. (2023a,b)

Figure 11: Donut-Hole RD Analysis — Mobility Effects of the Youth Transit Pass

(a) Job mobility

(b) Job mobility from outer to inner areas

(c) Job mobility from inner to outer areas



Notes: This figure plots "donut" RD estimates and the associated 95% robust bias-corrected confidence intervals for the mobility effects of the youth transit pass. These estimates are obtained by excluding units within different neighborhoods, "donut holes", around the cutoff and reestimating Equation 1 on the remaining sample. RD estimates using a 0-day donut hole correspond to the baseline estimates using the full sample of layoffs. The estimates are plotted against the number of months after layoff over which outcomes are measured.

suggest choosing a significance level α^* that is higher than the usual 0.05. I set $\alpha^* = 0.10$. Second, in order to apply randomization inference techniques, we must specify a treatment assignment mechanism describing how units "are placed" on either side of the cutoff. Following Cattaneo et al. (2015), I assume a "complete or fixed-margins randomization" where the probability of each treatment assignment vector is $\binom{N_{\mathcal{W}}}{N_{\mathcal{W}}^{+}}^{-1}$, with $N_{\mathcal{W}}$ denoting the total number of units within $\mathcal{W}$ and $N_{\mathcal{W}}^{+}$, the number of treated units (assumed fixed). Moreover, I also implement Fisherian inference, which as opposed to alternative randomization inference methods is well suited when the number of units within $\mathcal{W}$ is small.[10] In particular, as Cattaneo and co-authors point out, this method provides *finite-sample valid* inference for the sharp null hypothesis of no treatment effect for any unit.

Tables 6 and 7 present the results of the local randomization approach (Panels A.2-C.2), compared to those of the continuity-based framework (Panels A.1-C.1). The predetermined covariates used to choose $\mathcal{W}$ are the same included in the first robustness check of this section (see Tables 4 and 5). The resulting window is $[-0.44, 0.44]$, roughly five months to the left and right of the age-26 cutoff. Panels A.2-C.2 report the difference in mean outcomes between treated and control units inside the window and the p-value associated with the test of the null hypothesis of no treatment effect for any unit. Overall, point estimates using the local randomization approach lead to the same qualitative conclusions as those with the continuity-based method. Specifically, they suggest that the youth transit pass *boosts employment* for young UA entrants and that these gains are driven by those living *far away* from jobs. Nonetheless, the local randomization approach has less statistical precision, given its much smaller (effective) sample size.[11]

[10] For a clear and detailed exposition of Fisherian inference, the reader may refer to Rosenbaum et al. (2010) and Imbens and Rubin (2015).

[11] Local randomization requires much stronger assumptions than the continuity-based framework

6.3 Difference-in-discontinuities design

This section aims to address the potential concern that interventions other than the youth transit pass *may also* switch on or off at age 26. To the best of my knowledge, only one other policy in Spain has the same cutoff. In particular, parents with dependent children *under 26* are eligible for more generous unemployment benefits, including higher maximum and minimum of unemployment insurance (UI) benefits,[12] as well as longer unemployment assistance (UA) benefits.[13] Hence, the age-26 effects reported so far could potentially reflect the *combined* effect of subsidized transit *and* more generous unemployment benefits. To address this potential concern, I leverage the differential timing of these policies. In particular, the age-26 cutoff for access to a youth transit pass was introduced *only after* October 2015, although it determined benefit generosity for parents long before. Therefore, following Grembi et al. (2016), I take the difference between pre- and post-treatment discontinuities at age-26 to difference out the effect of higher benefit generosity. To be precise, I estimate the following model using all layoffs resulting in a UA claim between January 1, 2006 and June 30, 2019:

$$\begin{aligned} y_{is} = \alpha_0 + \beta_0 \times \mathbb{1}(x_{is} \geq 0) + f_0(x_{is}) + T_{t(s)}[\alpha_1 \qquad & \forall x_{is} \in [-h^{min}_{MSE}, h^{min}_{MSE}] \qquad (2) \\ + \beta_1 \times \mathbb{1}(x_{is} \geq 0) + f_1(x_{is})] + \epsilon_{is} & \end{aligned}$$

$$x_{is} \equiv 26 - age_{is}$$

where y_{is} is an outcome of interest for individual i after the start of non-employment spell s, age_{is} is the age of i at the start date of s (measured in years), and x_{is} is the score variable capturing the number of years left before turning 26. $T_{t(s)}$ is an indicator for post-treatment layoffs, i.e. those occuring after October 1, 2015, when the age-26 cutoff started to determine eligibility for a youth pass. $f_0(\cdot)$ and $f_1(\cdot)$ are linear regressions fit separately on each side of the cutoff using pre- and post-treatment layoffs, respectively. Observations are weighted using a triangular kernel. h^{min}_{MSE} is the smaller of the two mean squared error (MSE)-optimal bandwidths estimated separately on pre- and post-treatment layoffs following Calonico et al. (2014).

The parameter β_1 captures the causal effect of the youth pass, *net of* any confounding effects of parental benefit generosity, *under* three identifying assumptions.[14] First, as before, potential outcomes are assumed to be continuous at the cutoff. Second, the local effect of the confounding policy —here, more generous benefits for parents— is assumed to remain constant over time *in the absence of treatment*. Last, the local effect of the treatment is assumed to be independent of the confounding policy.[15]

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and, thus, relies on much narrower neighborhoods for estimation of treatment effects. However, this alternative approach offers two advantages: a) it allows us to apply inference methods that are valid even with small sample sizes —a common constraint in the analysis of RD designs, and b) it reduces extrapolation by relying only on the few closest units to the cutoff (Cattaneo et al., 2015).

[12] These maximum and minimum levels apply to the UI benefit replacement rate, which equals 70% of the previous wage during the first six months and 50% afterwards.

[13] The magnitude of these age-based discontinuities depend on the age of parents themselves, the number of months they have paid to social security, and their number of children under 26.

[14] These assumptions are formally stated in Grembi et al. (2016).

[15] Should this assumption be called into question, the first two assumptions would still let us identify the causal effect of the treatment at the cutoff *for units receiving the confounding policy*. In this particular setup, we would be able to identify the local effect of access to a youth pass for unemployed youths whose parents are eligible for more genereous unemployment benefits.

Tables 8 and 9 present difference-in-discontinuity estimates of the effects of the youth pass using Equation 2 (Panels A.2, B.2, and C.2). Even though estimates tend to become smaller —and sometimes statistically insignificant— under this alternative design, they remain economically significant. In particular, I estimate local effects of roughly 15 percentage points in job finding, 30 days in cumulative employment, and 880 euros in cumulative earnings *over the first six months after layoff* (the associated p-values are 0.101, 0.004, and 0.076, respectively). Moreover, effects on job mobility from *outer* to inner areas continue to be positive and on the order of 20 percentage points (p-value=0.084). Similarly, effects on job mobility from *inner* to outer areas remain small, at 5 percentage points, and statistically insignificant (p-value=0.659).

Overall, these results continue to provide *suggestive* evidence of rising employment for unemployed youths under 26, driven by those living farther away from jobs.

Table 6: RD Estimates of the Labor Market Effects of the Youth Transit Pass — Local Randomization

	Panel A: Job finding rate					
A.1 Continuity-Based Method	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.06	0.14	0.09	0.20	0.23	0.23
Robust p-value	0.348	0.141	0.290	0.053	0.014	0.014
Optimal Bandwidth	2.09	1.44	1.51	1.47	1.61	1.62
Effective Obs. (Left)	352	244	260	255	273	273
Effective Obs. (Right)	318	223	238	230	248	248
A.2 Local Randomization Method	1 month	2 months	3 months	4 months	5 months	6 months
Diff. in Means	0.08	0.10	0.07	0.18	0.21	0.19
Fisherian p-value	0.372	0.338	0.504	0.056	0.022	0.028
Window	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]
Effective Obs. (Left)	70	70	70	70	70	70
Effective Obs. (Right)	68	68	68	68	68	68
	Panel B: Cumulative days in employment					
B.1 Continuity-Based Method	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.87	5.42	9.67	17.02	25.31	29.66
Robust p-value	0.096	0.011	0.023	0.014	0.006	0.004
Optimal Bandwidth	1.99	1.30	1.24	1.15	1.18	1.29
Effective Obs. (Left)	342	225	219	204	210	224
Effective Obs. (Right)	305	200	189	180	182	197
B.2 Local Randomization Method	1 month	2 months	3 months	4 months	5 months	6 months
Diff. in Means	0.72	3.31	5.73	9.39	15.12	19.06
Fisherian p-value	0.190	0.084	0.096	0.072	0.022	0.018
Window	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]
Effective Obs. (Left)	70	70	70	70	70	70
Effective Obs. (Right)	68	68	68	68	68	68
	Panel C: Cumulative earnings (2016 euros)					
C.1 Continuity-Based Method	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	6.74	111.12	234.22	357.73	563.52	819.14
Robust p-value	0.915	0.227	0.221	0.185	0.107	0.066
Optimal Bandwidth	1.92	1.46	1.45	1.50	1.47	1.47
Effective Obs. (Left)	305	213	195	194	182	176
Effective Obs. (Right)	279	199	189	184	170	166
C.2 Local Randomization Method	1 month	2 months	3 months	4 months	5 months	6 months
Diff. in Means	11.25	15.83	67.70	118.54	252.15	376.16
Fisherian p-value	0.554	0.816	0.748	0.764	0.526	0.476
Window	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]
Effective Obs. (Left)	67	64	57	56	55	54
Effective Obs. (Right)	59	53	49	45	44	42

Notes: This table reports continuity-based and local randomization RD estimates of the labor market effects of the youth transit pass (Panels A.1-C.1 and A.2-C.2, respectively). Continuity-based estimates are obtained using Equation 1 with mean squared error (MSE)-optimal bandwidth, following Calonico et al. (2014). The associated p-values are computed using the robust bias-corrected inference procedure based on the same work. Local randomization estimates are obtained by taking the difference in mean outcomes between treated and control units inside a narrow window around the cutoff. This window is selected using the procedure by Cattaneo et al. (2015) based on covariate balance. Predetermined covariates include controls for individual characteristics (gender, education, nationality, and non-employment history over the previous two years) and last job characteristics (temporary contract, part-time work, occupation skill level, and sector) as in Tables 4 and 5. P-values reported in Panels A.2-C.2 are randomization-based and correspond to the test of the sharp null hypothesis of no treatment effect for any unit, assuming a fixed margins randomization mechanism and using the difference-in-means as the test statistic. Outcomes are measured one to six months after layoff.

Table 7: RD Estimates of the Mobility Effects of the Youth Transit Pass — Local Randomization

	Panel A: Job mobility across outer and inner areas					
A.1 Continuity-Based Method	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.00	0.04	0.05	0.11	0.14	0.15
Robust p-value	0.776	0.572	0.507	0.083	0.034	0.022
Optimal Bandwidth	1.59	2.43	2.84	2.90	2.82	2.77
Effective Obs. (Left)	269	424	509	517	507	493
Effective Obs. (Right)	248	372	411	420	408	402
A.2 Local Randomization Method	1 month	2 months	3 months	4 months	5 months	6 months
Diff. in Means	-0.01	0.00	0.01	0.11	0.12	0.12
Fisherian p-value	1.000	1.000	1.000	0.188	0.136	0.136
Window	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]
Effective Obs. (Left)	70	70	70	70	70	70
Effective Obs. (Right)	68	68	68	68	68	68
	Panel B: Job mobility from outer to inner areas					
B.1 Continuity-Based Method	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.03	0.09	0.16	0.29	0.25	0.25
Robust p-value	0.916	0.209	0.063	0.003	0.017	0.019
Optimal Bandwidth	1.42	2.95	2.12	1.92	1.98	1.93
Effective Obs. (Left)	146	326	221	207	214	208
Effective Obs. (Right)	128	273	200	180	185	182
B.2 Local Randomization Method	1 month	2 months	3 months	4 months	5 months	6 months
Diff. in Means	0.03	0.05	0.08	0.23	0.20	0.20
Fisherian p-value	0.994	0.740	0.578	0.042	0.092	0.092
Window	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]
Effective Obs. (Left)	41	41	41	41	41	41
Effective Obs. (Right)	40	40	40	40	40	40
	Panel C: Job mobility from inner to outer areas					
C.1 Continuity-Based Method	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.07	-0.06	-0.06	-0.06	0.05	0.06
Robust p-value	0.263	0.515	0.567	0.529	0.559	0.460
Optimal Bandwidth	2.29	2.30	2.17	2.45	2.14	2.04
Effective Obs. (Left)	144	146	135	166	135	130
Effective Obs. (Right)	133	133	125	137	124	123
C.2 Local Randomization Method	1 month	2 months	3 months	4 months	5 months	6 months
Diff. in Means	-0.07	-0.07	-0.10	-0.07	0.01	0.01
Fisherian p-value	0.652	0.658	0.424	0.722	1.000	1.000
Window	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]	[-0.44, 0.44]
Effective Obs. (Left)	29	29	29	29	29	29
Effective Obs. (Right)	28	28	28	28	28	28

Notes: This table reports continuity-based and local randomization RD estimates of the job mobility effects of the youth transit pass (Panels A.1-C.1 and A.2-C.2, respectively). Job mobility is defined as job finding outside the home area, as detailed in Section 3. Continuity-based estimates are obtained using Equation 1 with mean squared error (MSE)-optimal bandwidth, following Calonico et al. (2014). The associated p-values are computed using the robust bias-corrected inference procedure based on the same work. Local randomization estimates are obtained by taking the difference in mean outcomes between treated and control units inside a narrow window around the cutoff. This window is selected using the procedure by Cattaneo et al. (2015) based on covariate balance. Predetermined covariates include controls for individual characteristics (gender, education, nationality, and non-employment history over the previous two years) and last job characteristics (temporary contract, part-time work, occupation skill level, and sector) as in Tables 4 and 5. P-values reported in Panels A.2-C.2 are randomization-based and correspond to the test of the sharp null hypothesis of no treatment effect for any unit, assuming a fixed margins randomization mechanism and using the difference-in-means as the test statistic. Outcomes are measured one to six months after layoff.

Table 8: Diff-in-Disc. Estimates of the Labor Market Effects of the Youth Transit Pass

	Panel A: Job finding rate					
A.1 Baseline RD design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.06	0.14	0.09	0.20	0.23	0.23
Robust Std. Error	0.08	0.12	0.12	0.12	0.11	0.10
Robust p-value	0.348	0.141	0.290	0.053	0.014	0.014
Observations	8158	8158	8158	8158	8158	8158
A.2 Difference-in-discontinuity design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.03	0.13	0.03	0.14	0.16	0.16
Std. Error	0.07	0.10	0.10	0.10	0.10	0.09
p-value	0.714	0.199	0.765	0.182	0.092	0.101
Observations	28106	28106	28106	28106	28106	28106
	Panel B: Cumulative days in employment					
B.1 Baseline RD design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.87	5.42	9.67	17.02	25.31	29.66
Robust Std. Error	0.63	2.56	5.13	8.15	10.49	11.70
Robust p-value	0.096	0.011	0.023	0.014	0.006	0.004
Observations	8158	8158	8158	8158	8158	8158
B.2 Difference-in-discontinuity design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.38	5.67	11.83	18.24	26.73	31.07
Std. Error	0.63	2.36	4.41	6.72	8.75	10.27
p-value	0.551	0.016	0.007	0.007	0.002	0.003
Observations	28106	28106	28106	28106	28106	28106
	Panel C: Cumulative earnings (2016 euros)					
C.1 Baseline RD design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	6.74	111.12	234.22	357.73	563.52	819.14
Robust Std. Error	29.53	120.46	260.27	361.10	445.34	542.16
Robust p-value	0.915	0.227	0.221	0.185	0.107	0.066
Observations	7612	7145	6802	6514	6259	6010
C.2 Difference-in-discontinuity design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-5.77	105.04	232.08	281.68	471.13	881.41
Std. Error	42.02	110.17	217.11	322.58	403.59	496.88
p-value	0.891	0.341	0.285	0.383	0.243	0.076
Observations	27104	26357	25773	25287	24865	24421

Notes: This table reports estimates of the labor market effects of the youth transit pass using both the baseline RD equation (1) and the difference-in-discontinuities equation (2). Outcomes are measured one to six months after layoff. The baseline RD estimation uses the mean squared error (MSE)-optimal bandwidth for post-treatment layoffs, following the procedure by Calonico et al. (2014). The difference-in-discontinuities estimation uses the smaller of the two MSE-optimal bandwidths calculated separately for pre- and post-treatment layoffs. Standard errors and p-values corresponding to baseline RD estimates are computed using the robust bias-corrected inference procedure suggested by Calonico et al. (2014). However, the current methodological literature on RD designs does not offer specific guidance on implementing this procedure for difference-in-discontinuities estimates. I, therefore, report conventional heteroskedasticity-robust standard errors and p-values for them.

Table 9: Diff-in-Disc. Estimates of the Mobility Effects of the Youth Transit Pass

	Panel A: Job mobility across outer and inner areas					
A.1 Baseline RD design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.00	0.04	0.05	0.11	0.14	0.15
Robust Std. Error	0.05	0.07	0.07	0.07	0.08	0.08
Robust p-value	0.776	0.572	0.507	0.083	0.034	0.022
Observations	8154	8153	8151	8150	8150	8150
A.2 Difference-in-discontinuity design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.04	-0.04	-0.04	0.02	0.06	0.05
Std. Error	0.05	0.05	0.06	0.06	0.06	0.06
p-value	0.464	0.443	0.526	0.795	0.311	0.441
Observations	28064	28050	28039	28025	28021	28012
	Panel B: Job mobility from outer to inner areas					
B.1 Baseline RD design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	0.03	0.09	0.16	0.29	0.25	0.25
Robust Std. Error	0.08	0.07	0.09	0.11	0.12	0.12
Robust p-value	0.916	0.209	0.063	0.003	0.017	0.019
Observations	5278	5277	5277	5276	5276	5276
B.2 Difference-in-discontinuity design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.05	0.04	0.04	0.18	0.18	0.19
Std. Error	0.08	0.07	0.09	0.10	0.11	0.11
p-value	0.502	0.565	0.686	0.081	0.096	0.084
Observations	16556	16547	16539	16532	16530	16526
	Panel C: Job mobility from inner to outer areas					
C.1 Baseline RD design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.07	-0.06	-0.06	-0.06	0.05	0.06
Robust Std. Error	0.07	0.10	0.10	0.10	0.12	0.12
Robust p-value	0.263	0.515	0.567	0.529	0.559	0.460
Observations	2876	2876	2874	2874	2874	2874
C.2 Difference-in-discontinuity design	1 month	2 months	3 months	4 months	5 months	6 months
RD Effect	-0.09	-0.10	-0.11	-0.11	0.00	0.05
Std. Error	0.05	0.07	0.08	0.08	0.10	0.10
p-value	0.077	0.173	0.171	0.188	1.000	0.659
Observations	11508	11503	11500	11493	11491	11486

Notes: This table reports estimates of the job mobility effects of the youth transit pass using both the baseline RD equation (1) and the difference-in-discontinuities equation (2). Outcomes are measured one to six months after layoff. The baseline RD estimation uses the mean squared error (MSE)-optimal bandwidth for post-treatment layoffs, following the procedure by Calonico et al. (2014). The difference-in-discontinuities estimation uses the smaller of the two MSE-optimal bandwidths calculated separately for pre- and post-treatment layoffs. Standard errors and p-values corresponding to baseline RD estimates are computed using the robust bias-corrected inference procedure suggested by Calonico et al. (2014). However, the current methodological literature on RD designs does not offer specific guidance on implementing this procedure for difference-in-discontinuities estimates. I, therefore, report conventional heteroskedasticity-robust standard errors and p-values for them.

7 Conclusion

In this paper, I assess the labor market effects of subsidized transit on *young* unemployed jobseekers relying on (means-tested) unemployment assistance. In particular, I exploit the regression discontinuity (RD) design created by an age threshold determining eligibility for subsidized public transport in the Spanish region of Madrid. Using social security data, I compare the future labor market outcomes of unemployment assistance claimants laid off *just* under and over the age of 26. Unemployed youths under 26 can benefit from cheaper spatial job search and (future) commuting. Nonetheless, as in many age-based RD designs, individuals eventually age out of (or into) treatment, here subsidized transit. I, therefore, focus on *short-term* labor market outcomes, measured one to six months after layoff.

Results suggest that subsidized transit may bring significant employment gains for young assistance recipients. More precisely, I estimate a (local) treatment effect of 23 percentage points on their job-finding probability and 30 days on their number of cumulative days in employment six months after layoff. Moreover, results indicate that these employment gains are driven by those living in outer areas of Madrid, farther away from jobs. Treatment effects on earnings are positive, although imprecisely estimated.

These empirical findings suggest that targeting subsidized transit to young individuals may be a cost-effective labor market intervention in urban regions with good-quality mass transport and dense inner areas concentrating labor demand.
Finally, this paper warrants further empirical research on the *longer-term* employment effects of subsidized transit on the unemployed youth, as well as on its displacement and other equilibrium effects.

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Appendix

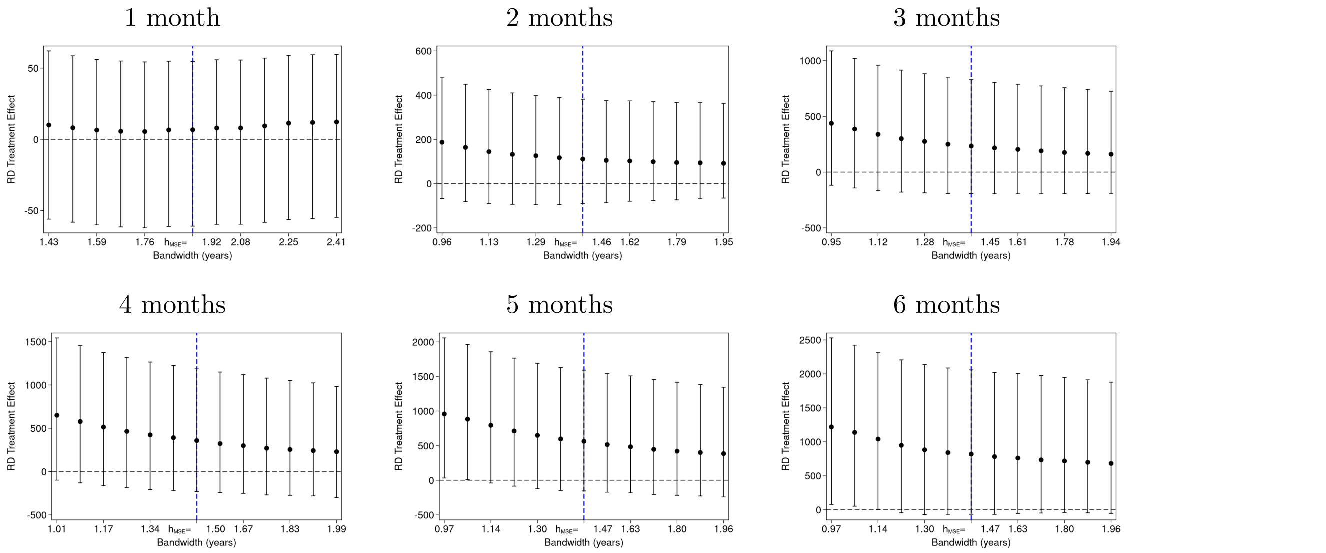
Table A1: RD Effects for Predetermined Covariates

	Panel A: Individual Characteristics				
	Female	High school	Foreign	Months out of work	
RD Effect	0.15	-0.05	-0.03	-0.17	
Robust Std. Error	0.12	0.12	0.08	1.08	
Robust p-value	0.126	0.480	0.747	0.964	
Observations	8158	8066	8155	8158	
Optimal Bandwidth	1.48	1.34	2.10	1.97	
Effective Obs. (Left)	255	228	355	338	
Effective Obs. (Right)	233	204	318	302	
	Panel B: Last Job Characteristics				
	Low-skill	Temporary	Part-time	Construction	Services
RD Effect	0.05	0.01	-0.02	-0.02	0.06
Robust Std. Error	0.08	0.05	0.09	0.05	0.05
Robust p-value	0.430	0.885	0.805	0.564	0.139
Observations	8158	8158	8158	8158	8158
Optimal Bandwidth	1.94	3.04	2.99	1.97	1.75
Effective Obs. (Left)	336	546	539	340	308
Effective Obs. (Right)	301	434	431	303	278

Notes: This table reports local linear RD estimates for predetermined covariates using Equation 1, with each covariate as the dependent variable. The estimation uses the mean squared error (MSE)-optimal bandwidth based on Calonico et al. (2014). Standard errors and p-values are computed using the robust bias-corrected inference procedure based on the same work.

Figure A1: Bandwidth Sensitivity Check — Labor Market Effects

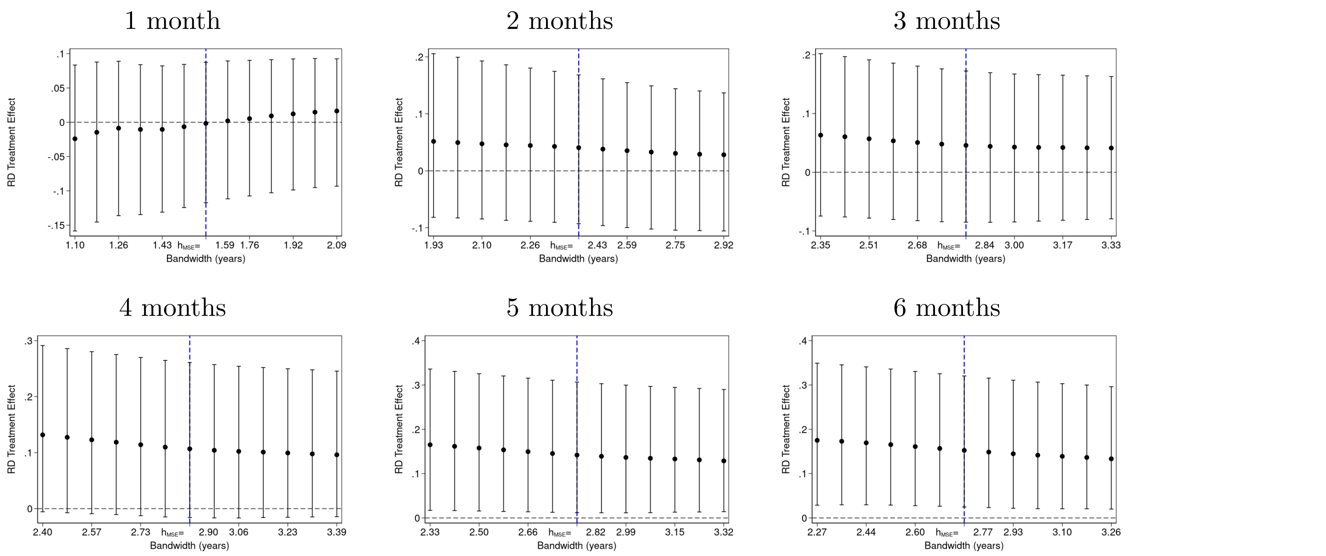
Cumulative earnings (2016 euros)



Notes: This figure plots RD estimates and the associated 95% robust bias-corrected confidence intervals for the earnings effects of the youth transit pass using different bandwidths around the cutoff. The bandwidths used are ± 30, 60, 90, 120, 150, and 180 days relative to the benchmark mean squared error (MSE)-optimal bandwidth reported in Panel C of Table 2. Estimates are obtained using Equation 1. The vertical blue line indicates the MSE-optimal bandwidth.

Figure A2: Bandwidth Sensitivity Check — Mobility Effects

Job mobility across outer and inner areas



Notes: This figure plots RD estimates and the associated 95% robust bias-corrected confidence intervals for the mobility effects of the youth transit pass using different bandwidths around the cutoff. The bandwidths used are ± 30, 60, 90, 120, 150, and 180 days relative to the benchmark mean squared error (MSE)-optimal bandwidth reported in Panel A of Table 3. Estimates are obtained using Equation 1. The vertical blue line indicates the MSE-optimal bandwidth.