# The employment effect of subsidized transit on unemployment assistance recipients

*Full Draft*

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**Abstract**

Policymakers typically use active labor market policies, such as training and job-search assistance, to help the unemployed find work. Nonetheless, these are often costly and have shown modest effects. In this research, I assess the employment effect of a low-cost and straightforward intervention: subsidized public transport for cash-constrained jobseekers. In particular, I exploit a natural experiment in Catalonia in 2012 that reduced transit costs for unemployment assistance recipients. Using three complementary empirical approaches (difference-in-differences, the synthetic control method, and the synthetic difference-in-differences method), I find that the transport subsidy offered in Catalonia brought meaningful employment gains *concentrated* on younger assistance recipients. These gains ranged from 18% to 25% of their estimated counterfactual outcome, three to twenty-four months after entering unemployment. Finally, I also find suggestive evidence that these employment gains did not come at the expense of lower earnings.

## 1 Introduction

Policymakers typically rely on "active labor market policies", such as training and job-search assistance, to help the unemployed find work. Nonetheless, these traditional activation policies are often costly and have had limited effects (Crépon and Van Den Berg, 2016; McKenzie, 2017; Vooren et al., 2019). Furthermore, their design and implementation have long strained local resources and institutional capacity in many economies. Indeed, training programs poorly address labor demand, and public employment services face severe staff shortages in many poor and rich countries alike (e.g. Angel-Urdinola et al., 2012; Escudero et al., 2016; OECD, 2018a,b; EU, 2019a,b; Martin, 2022).

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In this research, I focus on a simple and low-cost intervention that entails neither high operating costs nor bigger or better public employment services. In particular, I examine the employment effect of subsidizing public transport for cash-constrained and disadvantaged unemployed jobseekers, namely, means-tested unemployment assistance recipients. Targeting subsidized transit for these unemployed workers is likely to prove cost-effective for two reasons. Firstly, it is a simple-to-implement scalable activation policy that can build on the *existing* resources of *local* employment services and transport authorities. Secondly, cheaper transit may boost unemployed workers' job-finding rate and match quality. Indeed, a large body of empirical literature in urban economics shows that better access to transport enhances labor market outcomes (see Gobillon et al. (2007) for a recent review of this "spatial mismatch" literature following the seminal work of Kain (1968)).[1] Different mechanisms may explain this relationship, two of which are relevant to this research. First, lower transport costs may encourage workers to search farther away from their neighborhoods. Second, while search efficiency may initially decrease with distance, cheaper transport is likely to encourage workers to travel more often to distant areas, allowing them to learn about local job markets. These mechanisms are particularly relevant for liquidity-constrained unemployed jobseekers with poor social networks (Picard and Zenou, 2018) and reliant on public transport (Patacchini and Zenou, 2005) and low-skill services jobs, for which employers often use local recruiting methods, e.g., want ads (Gobillon and Selod, 2021).

To assess the employment effect of subsidized transit, I exploit a natural experiment in the Spanish region of Catalonia in 2012 targeting unemployment assistance recipients and the long-term unemployed. Since 2012, these jobseekers have been eligible to buy a 10-euro monthly travel pass to use *any* means of public transport within their *province* of residence. This subsidized pass may allow them to save between 40 and 130 euros per month (i.e., between 10 and 30 percent of unemployment assistance benefits), depending on the number of transport zones where their pass is valid.[2] To better capture the impact of this transit pass on the unemployed, I focus on treated individuals living in *Barcelona province*, where public transport has much better coverage and availability than in other Catalan provinces. Specifically, using Spanish social security records, I compare the employment trajectory of unemployment assistance entrants in Barcelona to those in other control provinces, weighting the latter using three alternative approaches: difference-in-differences, the synthetic control method, and the synthetic difference-in-differences. The key difference between these methods lies in the procedure used to assign weights to each available control unit (here, *province* unit). While the standard practice when applying difference-in-differences is to use an *ad-hoc* vector of unit weights, the synthetic control and synthetic difference-in-differences rely on a *data-driven* procedure to estimate them. More precisely, they

[1] This literature studies public and private transport individually and job accessibility more broadly defined. Some examples are Bastiaanssen et al. (2022); Brandtner et al. (2019); Tyndall (2017); Di Paolo et al. (2017); Rotger and Nielsen (2015); Holzer et al. (2003) on public transport; Baum (2009); Raphael et al. (2001) on private transport; and Andersson et al. (2018); Gobillon et al. (2011); Korsu and Wenglenski (2010) on a broader measure of job accesibility. Finally, in the context of the Spanish labor market, Di Paolo et al. (2017); Matas et al. (2010) suggest that better access to jobs through public transport improves the labor market outcomes of workers living in the Barcelona and Madrid metropolitan areas.

[2] Section 2 expands on the details of this activation policy.

are chosen such that the resulting weighted average for the control units best mimics the pre-treatment outcomes for the treated unit.

The group of workers I focus on in this research is particularly relevant to understand the potential role of subsidized transit in enhancing the job-search process of the unemployed. Indeed, I study jobseekers entering unemployment assistance with a monthly income below 75% of the minimum wage in Spain and a critically unfavorable employment history. Over three-quarters of them had accumulated more than twelve months in unemployment over the previous two years, above 90% had a temporary contract, and about 60% relied on low-skill services jobs. These are thus cash-constrained jobseekers who are likely to respond to price cuts in public transport to broaden their geographical search radius and benefit from local recruiting methods. The labor market outcome I examine is the number of cumulative days in employment over the first three, six, twelve, and twenty-four months after entering unemployment, capturing both job finding and match quality in the short and long run.

My results suggest that subsidized transit may be an effective, low-cost policy to boost employment for cash-constrained jobseekers most reliant on mass transport. More precisely, I find relatively large employment gains for *younger* unemployment assistance entrants (aged 27-35), both in the short and long term. In particular, using the synthetic control method, I estimate that they gained 2 and 6 cumulative days in employment over the first three and six months after entering unemployment, respectively, and 21 and 49 days one and two years after. These are economically and statistically significant treatment effects representing between 18% and 25% of their estimated counterfactual outcome. Nonetheless, treatment effects for older cohorts and the overall sample are small and statistically insignificant. This treatment effect heterogeneity by age is bound to reflect that younger workers tend to rely more on *public* transport and are thus more likely to take up the transit subsidy. Moreover, they also tend to rely more on low-skill services jobs, the types of jobs where the returns to *spatial* job search are likely highest.

This research relates to the literature on active labor market policies in developed countries (see Crépon and Van Den Berg, 2016; Vooren et al., 2019; Card et al., 2018, for a review). 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. Moreover, I focus on a Southern European economy with persistent tight resource constraints and one of Europe's highest unemployment and long-term unemployment rates. In contrast, previous studies have mainly focused on countries with relatively adequate resources and institutional capacity to activate the unemployed and better labor market indicators, such as Germany, Austria, Norway, Denmark, and Sweden.[3] Yet, with these caveats in mind, Goller et al. (2021) recently assess the effectiveness of traditional activation programs on unemployment assistance recipients in Germany. A salient feature in their study is that the implementing job centers had considerable freedom in designing programs to meet jobseekers' needs, thus likely boosting their

[3] According to a report by the European Commission, in 2016, the ratio of caseworkers to unemployed clients in Spain was 1:596, whereas, in Germany, Austria, and Sweden, it ranged from 1:26 to 1:85. No data were available for Norway and Denmark (see EU, 2016). Additionally, between 2012 and 2017, the average unemployment (long-term unemployment) rate in Spain was 22% (11%), while in Germany, Austria, Norway, Denmark, and Sweden it ranged from 4% to 8% (1% to 2%). Despite these striking differences, these countries account for more than 80% of studies on active labor market policies, as reviewed by Vooren et al. (2019).

impact on beneficiaries. Indeed, the authors estimate significant employment gains from job training and placement services up to three years after treatment.

More closely related to this research is the empirical literature studying the effects of low-cost interventions to enhance job search. Two recent papers with a similar scope are those by Altmann et al. (2018) and Belot et al. (2019). In particular, Altmann et al. (2018) study the labor market effects of motivating and informing unemployed jobseekers in Germany about crucial aspects of the job-search process and the consequences of unemployment. The authors find positive effects on earnings and (cumulative) days in employment, concentrated on jobseekers at risk of long-term unemployment. Belot et al. (2019) evaluate the effect of providing jobseekers with online tailored advice to search for alternative occupations to their preferred one. Their results show positive effects on jobseekers' occupational breadth of search and applications and their total number of interviews, especially for those who initially searched narrowly and had been unemployed for more than two months. Nonetheless, they cannot provide evidence of their intervention boosting employment outcomes, likely due to both lack of power and a relatively low conversion rate from interviews to job offers for broader occupations. This paper adds to this literature by assessing a potentially *complementary* cost-effective intervention to foster the labor market outcomes of the unemployed, especially of those with low income and more likely to benefit from cheaper spatial job search.

This research also builds on and contributes to the literature in urban economics assessing the effect of transport subsidies for job search on the unemployed. 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 example, Abebe et al. (2021) and Franklin (2018) assess the employment effect of a public transport subsidy on young unemployed jobseekers in Africa, while Phillips (2014) on African-American unemployed jobseekers from economically disadvantaged neighborhoods in Washington DC.[4] These authors show that public transport subsidies help the unemployed find jobs, though this effect is short-lived. Furthermore, Le Gallo et al. (2017) study the employment effect of providing young unemployed individuals with a 1000-euro voucher for driving lessons. The authors find that this subsidy reduced their search effort during training and, only two years after treatment, helped them find jobs, albeit only temporary ones. They thus advise against using such a subsidy to reduce search costs. Moreover, even if subsidizing private transport boosted the employment prospects of the unemployed, it could still prove to be an inefficient policy given its negative externalities on traffic congestion, especially in metropolitan areas.

This paper proceeds as follows. Section 2 describes the policy change and explains the characteristics and eligibility criteria of the transit subsidy I study. Section 3 outlines the three alternative approaches I follow to estimate the causal effect of subsidized transit on unemployed jobseekers. Section 4 provides information about my data source and estimation sample. Section 5 shows the results and includes placebo checks and a back-of-the-envelope cost-benefit analysis. Section 6 concludes.

[4]The sample of participants in the experimental study of Phillips (2014) were clients of a non-profit organization serving not only low-income individuals receiving public assistance but also those re-entering the labor force after incarceration or recovering from substance abuse.

## 2 The Policy Change

On 12 March 2012, Catalonia became the first Spanish region where the unemployed could get a subsidy to use *any* means of public transport within their *province* of residence. While there are a few similar subsidies in other Spanish regions, these are offered by municipal rather than province-level authorities. Thus, they only allow for journeys within a particular city (e.g., Malaga, Sevilla, and Bilbao), mainly using a single mode of public transport (city buses). Until now, Catalonia is still the only Spanish region where unemployed jobseekers are eligible for a province-level subsidy for all means of public transport.

To be eligible for this subsidy, an unemployed jobseeker must: 1) receive unemployment assistance benefits or 2) have been registered as unemployed at the regional employment service for at least 12 months over the last two years. A national or regional employment service certificate is required to prove either of these eligibility conditions.[5]

Eligible jobseekers can buy a monthly travel pass for a subsidized *flat* price of about 10 euros.[6] This pass allows them to travel within one or more transport zones in their province, depending on their unemployment history over the last two years before they apply for it. In particular, jobseekers registered as unemployed for a minimum of 12 months are entitled to a pass for up to all zones, while all other eligible individuals are entitled to a pass for one zone only. As a result, jobseekers may save from 40 to 130 euros per monthly pass, depending on the number of zones where it is valid. This saving ranges from 10 to 30% of unemployment assistance benefits.[7]

While all four Catalan provinces have implemented the transit subsidy similarly, their public transport networks vary widely in size and quality. Barcelona province indeed offers a network with much better coverage and availability. I thus focus on this province, where cash-constrained jobseekers are more likely to react to subsidized public transport to search for work.[8] Figure 1 shows gross estimates of the take-up rate of the subsidy in Barcelona and Girona, the two provinces with available

[5]Since 2014, recipients of social assistance benefits are also eligible for the subsidy. To prove their eligibility, they need to submit a certificate from their local municipality validating their status as unemployed jobseekers receiving only social assistance benefits.

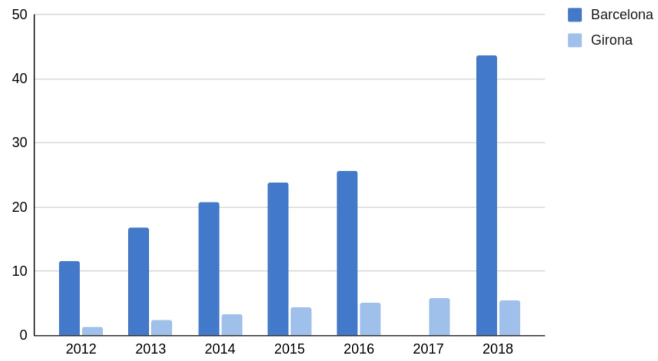
[6]This activation policy has been implemented slightly differently in each Catalan province. In Barcelona, jobseekers were initially offered a 30-euro *quarterly* pass and, from 2017 onwards, a 10-euro *monthly* pass. In contrast, in Girona, Lleida, and Tarragona, they were offered a monthly pass since the onset of the policy, for 9 to 11 euros.

[7]The actual saving for jobseekers depends on their province of residence and baseline price for a monthly transit pass. The previous saving calculation pertains to jobseekers living in Barcelona, who otherwise would face the *regular* (full) price. Nevertheless, saving for groups *already* benefiting from subsidized transit is notably lower, ranging from 2 to 24%. These groups include youths under 25 and individuals in special family categories (e.g., families with a single parent or more than three dependent children). However, these individuals will likely play a minor role in the results of this study. Indeed, youths under 25 are not included in the estimation sample for identification reasons discussed in Section 4. Additionally, the share of individuals belonging to special family categories is likely small. According to Eurostat data, during the sample period 2006-2017, only 3% of households in Spain were headed by a single parent, and 2% by two adults with three or more dependent children.

[8]According to the last Daily Mobility Survey in Catalonia in 2006, 24% of all commutes in the metropolitan area of Barcelona use public transport, whereas only 7% in the rest of Catalonia.

data. This figure shows a marked difference in their take-up rates, in line with the hypothesis that liquidity-constrained jobseekers are more likely to react to mass transport subsidies in areas with better public transport services.

Figure 1: Estimate of the take-up rate of the transit subsidy (%)




*Notes:* These take-up estimates are based on the number of subsidized passes sold and the number of unemployment assistance recipients living in each province. Source: ATM Barcelona, ATM Girona, and MITES, Spain. No data are available for Tarragona and Lleida.

## 3 Empirical Strategy

To assess the causal effect of the transit subsidy on the unemployed in Barcelona province, I exploit the time and geographical dimensions of the policy change.

As a first exploratory analysis, I use a difference-in-differences approach comparing the employment outcomes of unemployment assistance (UA) entrants in Barcelona province (treated) and Madrid Community (control) before and after the treatment.[9] These two provinces have a population size and density that stand out in sharp contrast to the rest of peninsular Spain and have similar GDP per capita and unemployment rates.[10] Moreover, they both have a well-integrated network of public transport offering good coverage and availability. Indeed, people in Barcelona and Madrid rely more heavily on public transport than in any other Spanish metropolitan area.[11]

While this Barcelona versus Madrid comparison is a reasonably intuitive first exercise, it poses some critical concerns. More precisely, difference-in-differences applications have well-known caveats on statistical inference (Conley and Taber, 2011). More importantly, they leaves us with much uncertainty about how well our chosen control unit actually reproduces the counterfactual of the treated unit.

[9]Madrid Community is a Spanish region with a single province. For simplicity, from now on, I shall refer to it as “Madrid” and to Barcelona *province* as “Barcelona”.

[10]During the pre-treatment period 2006-2011, Barcelona and Madrid had a population size and density of 5-6 million and 700-780 inhabitants/km$^2$, respectively. Meanwhile, the rest of peninsular Spain had a median of only 0.6 million and 55 inhabitants/km$^2$. In addition, Barcelona’s and Madrid’s GDP per capita and unemployment rate were 27-31 thousand euros and 11-12 percent, respectively. The rest of peninsular Spain had a corresponding median of 20 thousand euros and 13 percent.

[11]Public transport accounts for more than 30% of commutes in Barcelona and Madrid. Whereas, in other Spanish metropolitan areas, the median is 10% (Monzón et al., 2019).

Taking this first analysis one step further, I thus implement the data-driven procedure proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015) to construct an alternative comparison unit for Barcelona. This *"synthetic control unit"* is a weighted average of different control units (provinces), with weights chosen to best match the pre-treatment outcome levels of the treated unit. The basic idea behind this approach is that a combination of different control units likely provides a better comparison to the treated unit than any control unit *alone* (Abadie and Cattaneo, 2018).

Additionally, as a robustness check, I implement the *"synthetic difference-in-differences"* (Arkhangelsky et al., 2021), which builds on the pros of both previous methods. In particular, it let us construct a control unit for Barcelona without searching for the best fit between the levels but simply the *trends* of pre-treatment outcomes.

This section is organized as follows. Subsection 3.1 introduces basic notation and the structure of the panel data I build to estimate treatment effects. Subsection 3.2 presents the three identification methods I implement, and Subsection 3.3 outlines the inference approach I follow. It is important to bear in mind that the effects I am able to estimate in this paper capture *intention-to-treat* (ITT) effects. Specifically, they capture the effect of being eligible for subsidized spatial job search.

## 3.1 The setting

Let's consider a balanced panel of $J+1$ provinces over $T$ years, where $Y_{pt}$ denotes the average employment outcome in province $p$ and year $t$. Moreover, let province $p=1$ be the (only) treated unit and provinces $p=2$ to $p=J+1$, $J$ available control units. The treated unit (Barcelona) is exposed to the treatment (subsidized public transport) from year $T_0+1$ (2012) onwards.[12] $T_0$ denotes the number of pre-treatment years and $T_1$, the number of post-treatment years, thus $T=T_0+T_1$.

## 3.2 Model specification

### 3.2.1 *Difference-in-differences* [DID]

I estimate a difference-in-differences model on the average employment outcomes of UA entrants in Barcelona ($p=1$) and Madrid ($p=2$). Access to treatment is determined by their province of residence and the year when they entered UA. The cohort of entry into UA thus captures the time dimension of the policy change. The pre-treatment cohort comprises UA entrants between 2006 and 2011 ($t=1,\ldots,T_0$), while the post-treatment cohort, those between 2012 and 2017 ($t=T_0+1,\ldots,T$). In particular, for each outcome I study, I estimate the following regression:

$$Y_{pt}=\gamma_p+\lambda_t+\boldsymbol{\tau}^{\boldsymbol{did}}TREAT_p\cdot POST_t+\epsilon_{pt} \tag{1}$$
$$p\in\{1,2\}$$
$$t\in\{1,\ldots,T\}$$

[12] More precisely, Barcelona is exposed to the treatment from 12 March 2012, not 1 January 2012. Nonethelss, I abstract from this defered start and define the post-treatment period to begin from 1 January 2012. This abstraction is, however, likely to prove inconsequential for the results. Indeed, Figure 1, showing gross estimates of the subsidy's take-up rate in Barcelona, suggests that (Intention-to-Treat) effects will start off relatively low and increase gradually over time.

where $Y_{pt}$ is the average employment outcome for UA entrants in year $t$ and province $p$; $\gamma_p$ and $\lambda_t$ are province and time fixed-effects, respectively; $TREAT_p$, a dummy indicating the treated province (Barcelona); and $POST_t$, a dummy for post-treatment cohorts (UA entrants from 2012 onwards, when the transport subsidy became available).

Under the assumption that the average outcome in Barcelona and Madrid would have evolved equally in the absence of the treatment, the Ordinary Least Squares (OLS) estimator of $\tau^{did}$ captures the (average) causal effect of the transport subsidy on UA entrants in Barcelona. This DID estimator of the treatment effect, averaged out across all post-treatment cohorts, can be expressed as:

$$\hat{\tau}^{did} = \hat{\delta}_1^{did} - \hat{\delta}_2^{did} \tag{2}$$

where,

$$\hat{\delta}_p^{did} \equiv \frac{1}{T_1} \sum_{t=T_0+1}^{T} Y_{pt} - \frac{1}{T_0} \sum_{t=1}^{T_0} Y_{pt} \quad \forall p \in \{1, 2\}$$

### 3.2.2 *Synthetic control method* [SC]

Considering that a combination of control provinces may better reproduce the counterfactual outcomes for Barcelona than Madrid alone, I construct a "synthetic Barcelona" by taking a weighted average of other provinces in Spain.

More formally, let $J$ be the number of available Spanish control provinces; and $\omega$ a $(J \times 1)$ vector of non-negative weights summing to one, where $\omega_j$ $(j = 2, ..., J+1)$ is the weight we assign to province $j$. Each value for $\omega$ represents a potential different "synthetic control" for Barcelona.

Building on Abadie and Gardeazabal (2003), Abadie et al. (2010, 2015) and Arkhangelsky et al. (2021), I estimate $\omega$ so that the resulting synthetic control best fits the outcomes of all pre-treatment cohorts in Barcelona.[13] To be precise, let $Y_{1t}$ $(t = 1, ..., T_0)$ be the employment outcome of pre-treatment cohort $t$ in Barcelona and $Y_{jt}$ that in control province $j$. Then, for each outcome I study, I choose $\hat{\omega}^{sc}$ by solving the following optimization problem:

$$\hat{\omega}^{sc} = \underset{\omega \in \Omega}{\arg\min} \, f(\omega) \tag{3}$$

where,

$$f(\boldsymbol{\omega}) \equiv \sum_{t=1}^{T_0} \left( Y_{1t} - \sum_j \boldsymbol{\omega_j} Y_{jt} \right)^2 + \zeta^2 T_0 \sum_j \boldsymbol{\omega}_j^2$$

$$\Omega \equiv \left\{ \boldsymbol{\omega} \in \mathbb{R}_+^J : \sum_j \boldsymbol{\omega_j} = 1 \right\}$$

$$j \in \{2, ..., J+1\}$$

$$t \in \{1, ..., T_0\}$$

[13] For brevity, I shall use pre(post)treatment outcomes to refer to those for pre(post)treatment *cohorts*, i.e. job seekers entering UA between 2006 and 2011 (from 2012 onwards).

The regularization parameter $\zeta$ is set as in Arkhangelsky et al. (2021).

The SC estimator of the effect of the treatment in Barcelona, averaged out across all post-treatment cohorts, is then:

$$\hat{\tau}^{sc} = \hat{\delta}_1^{sc} - \sum_j \hat{\omega}_j^{sc} \hat{\delta}_j^{sc} \tag{4}$$

where,

$$\hat{\delta}_p^{sc} \equiv \frac{1}{T_1} \sum_{t=T_0+1}^{T} Y_{pt} \quad \forall p \in \{1, ..., J+1\}$$

### 3.2.3 *Synthetic difference-in-differences* [SDID]

As the SC method, the SDID (Arkhangelsky et al., 2021) builds on the premise that a combination of different control units likely provides a better counterfactual for the treated than any single unit alone. Nevertheless, SDID unit (province) weights aim to make pre-treatment outcomes *parallel* rather than necessarily equal. In addition, under the SDID method, we also estimate time weights so that, for all control units, pre-treatment years balance post-treatment ones, up to a constant.

Formally, as before, let $Y_{1t}$ be the average outcome in Barcelona for cohort $t$ of UA entrants and $Y_{jt}$, that in control province $j$ ($j = 2, ..., J+1$). Furthermore, let $T_0$ and $T_1$ be the number of pre and post-treatment cohorts, respectively. Then, the vector of SDID *unit* weights ($\hat{\omega}^{sdid}$) solves the following optimization problem:

$$(\hat{\omega}^{sdid}, \hat{\omega}_0) = \underset{\omega \in \Omega, \omega_0 \in \mathbb{R}}{\arg\min} \ f(\boldsymbol{\omega}, \omega_0) \tag{5}$$

where,

$$f(\boldsymbol{\omega}, \omega_0) \equiv \sum_{t=1}^{T_0} \left( Y_{1t} - \sum_j \boldsymbol{\omega}_j Y_{jt} - \omega_0 \right)^2 + \zeta^2 T_0 \sum_j \boldsymbol{\omega}_j^2$$

$$\Omega \equiv \left\{ \boldsymbol{\omega} \in \mathbb{R}_+^J : \sum_j \boldsymbol{\omega}_j = 1 \right\}$$

$$j \in \{2, ..., J+1\}$$

$$t \in \{1, ..., T_0\}$$

The regularization parameter $\zeta$ is set according to equation (5) in Arkhangelsky et al. (2021).

Additionally, the vector of SDID *time* weights ($\hat{\lambda}^{sdid}$) solves:

$$(\hat{\lambda}^{sdid}, \hat{\lambda}_0) = \underset{\lambda \in \Lambda, \lambda_0 \in \mathbb{R}}{\arg\min} \ g(\boldsymbol{\lambda}, \lambda_0) \tag{6}$$

where,

$$g(\boldsymbol{\lambda}, \lambda) \equiv \sum_j \left( \frac{1}{T_1} \sum_{t=T_0+1}^{T} Y_{jt} - \sum_{t=1}^{T_0} \boldsymbol{\lambda}_t Y_{jt} - \lambda_0 \right)^2$$

$$\Lambda \equiv \left\{ \boldsymbol{\lambda} \in \mathbb{R}_{+}^{T_0} : \sum_{t=1}^{T_0} \boldsymbol{\lambda_t} = \mathbf{1} \right\}$$

$$j \in \{2, ..., J+1\}$$

$$t \in \{1, ..., T\}$$

Similarly to the SC approach, I estimate unit and time weights for each employment outcome.

The SDID estimator of the effect of the treatment in Barcelona, averaged out across all post-treatment cohorts, is then:

$$\hat{\tau}^{sdid} = \hat{\delta}_1^{sdid} - \sum_j \hat{\omega}_j^{sdid} \hat{\delta}_j^{sdid} \tag{7}$$

where,

$$\hat{\delta}_p^{sdid} \equiv \frac{1}{T_1} \sum_{t=T_0+1}^{T} Y_{pt} - \sum_{t=1}^{T_0} \hat{\lambda}_t^{sdid} Y_{pt} \quad \forall p \in \{1, ..., J+1\}$$

### 3.3 Inference

Standard errors often reported for regression estimates only capture sampling variance. If we had data on the entire population of UA entrants in Barcelona and our $J$ control provinces, regression standard errors would shrink to zero. Nevertheless, we would still face uncertainty about the validity of our control unit, that is, over its ability to reproduce the fate of UA entrants in Barcelona *in the absence of the treatment*.

Placebo analyses offer a way to measure this uncertainty by substituting the unit that implemented the treatment (Barcelona) with units that did not (our $J$ control provinces). In particular, following Arkhangelsky et al. (2021), I iteratively compute the treatment effect estimator using every control province as if it had been treated, leaving Barcelona out. I then estimate the standard error of the treatment effect for Barcelona by computing the standard deviation of the resulting placebo effects.[14]

## 4 Data

I use data from the Continuous Sample of Working Lives with Tax Records, *"Muestra Continua de Vidas Laborales con Datos Fiscales"*. This data is available yearly starting from 2006, and allows tracking the entire employment history of a 4% random sample of all individuals in Spain who pay into or receive social security over a given reference year. These include employed workers and recipients of unemployment benefits. Notably, we can construct a dataset on the future labor market outcomes of the inflow of unemployed jobseekers each year.

[14] I apply this algorithm separately to the three methods I use for identification (DID, SC, and SDID). To estimate the standard error of the DID treatment effect for Barcelona, with Madrid as a single ad hoc control unit, I iteratively estimate a DID model on Madrid and every other control province coded as treated. The standard error estimator is the standard deviation of the resulting $J-1$ placebo effects.

I focus on newly unemployed workers entering unemployment assistance between 2006 and 2017, at ages 27 to 50.[15] The basic unit of observation in the sampled data is an unemployment assistance spell for a given individual. The sample I focus on comprises 75,046 spells of unemployment assistance from 40,193 jobseekers living in different Spanish provinces. I use this individual micro-data to estimate *aggregate* employment outcomes at the *province-year* cell, using the province of residence and the year when jobseekers entered unemployment assistance.

Synthetic controls may produce largely biased treatment effect estimates if the pool of control units is too large or if this pool comprises units that are too different from the treated unit. I thus focus only on *highly urban* control provinces in Spain since Barcelona —the treated province— is heavily and densely populated.[16] In alphabetical order, these control provinces are: Alacant, Bizkaia, Cádiz, Córdoba, Madrid, Málaga, Murcia, Sevilla, València, and Zaragoza.[17]

The main estimation sample is then a panel of 11 highly urban Spanish provinces over 12 years (from 2006 to 2017). The labour market outcome I examine is the *average* cumulative days in employment three, six, twelve and twenty-four months after entry into unemployment assistance.[18] This outcome accounts for work hours.[19]

Table 1 shows descriptive statistics for Barcelona and each comparison unit I construct to match its pre-treatment outcomes —*three months* after entry into UA.[20] These comparison units are weighted averages of 10 urban Spanish provinces chosen as described in the previous section. Overall, they resemble fairly well the pre-treatment characteristics and outcomes of UA entrants in Barcelona.[21]

[15] I exclude UA entrants under 27 because some (those aged 23-26) benefited from a similar intervention to the one I examine but launched in Madrid in November 2015. In addition, I exclude UA entrants over 50 since most of them had at most two years —my observation window of interest— to become eligible for *unlimited* UA benefits. Specifically, until 2012 (between 2012 and 2019), UA recipients over 52 (55) were eligible for unlimited benefits until retirement. Thus, these recipients had potentially little economic incentives to search for work. Similarly, I also exclude individuals having any part-time job when they entered 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. Finally, I exclude UA entrants who were recalled by their last employer within three months.

[16] My results are, nonetheless, robust to using a larger pool of control units comprising all provinces above half a million inhabitants in peninsular Spain. This larger pool includes highly and medium urban provinces but not rural ones.

[17] Spain has 52 provinces. I exclude those outside peninsular Spain, i.e., Ceuta and Melilla, and the Balearic and Canary Islands. According to the Eurostat urban-rural taxonomy, there are 13 highly urban provinces in peninsular Spain. I exclude those with a sample size below 30 in any given province-year cell (thus, dropping Araba and Gipuzkoa in the Basque Country).

[18] Job losers must claim UA benefits within 15 days after displacement. If they do it later, their potential benefit duration decreases by the number of days elapsed between when they claimed benefits and lost their job. I exclude job losers claiming UA benefits more than one month later. Thus, for simplicity, I may use interchangeably “entry into *unemployment assistance*” and “entry into *unemployment*”.

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

[20] As mentioned in the previous section, I construct different SC and SDID control units for *each* outcome I study: the average number of cumulative days in employment *three*, *six*, *twelve*, and *twenty-four* months after entry into UA. For the sake of brevity, Table 1 only presents the pre-treatment balance between Barcelona and its controls when targeting the first outcome.

[21] A notable exception is the discrepancy between the share of foreign UA entrants in Barcelona and its two synthetic controls (SC and SDID).

Table 1: Pre-treatment descriptive statistics (mean)

| | Barcelona | Control | | |
|---|---|---|---|---|
| | | DID | SC | SDID |
| *Individual characteristics* | | | | |
| Age 27-35 | 0.44 | 0.43 | 0.43 | 0.42 |
| Age 36-44 | 0.39 | 0.39 | 0.39 | 0.39 |
| Age 45-50 | 0.17 | 0.18 | 0.18 | 0.18 |
| Female | 0.44 | 0.50 | 0.51 | 0.51 |
| Foreign | 0.35 | 0.37 | 0.20 | 0.19 |
| High scool | 0.29 | 0.33 | 0.24 | 0.22 |
| *Unemployment history (over last 2 years)* | | | | |
| Unemployed < 6 months | 0.04 | 0.03 | 0.03 | 0.04 |
| Unemployed 6-12 months | 0.19 | 0.23 | 0.22 | 0.22 |
| Unemployed 12+ months | 0.78 | 0.73 | 0.75 | 0.75 |
| *Last job characteristics* | | | | |
| Temporary | 0.95 | 0.94 | 0.96 | 0.97 |
| Part-time | 0.31 | 0.29 | 0.29 | 0.29 |
| Low-skill occupation | 0.88 | 0.85 | 0.88 | 0.88 |
| Services | 0.75 | 0.71 | 0.70 | 0.69 |
| Construction | 0.19 | 0.25 | 0.23 | 0.22 |
| Industry | 0.07 | 0.04 | 0.08 | 0.09 |
| *Outcomes* | | | | |
| Cum. days in work 3 months after | 8.66 | 9.15 | 9.53 | 9.13 |
| Cum. days in work 6 months after | 27.94 | 30.18 | 30.69 | 29.63 |
| Cum. days in work 12 months after | 81.21 | 86.06 | 86.59 | 85.66 |
| Cum. days in work 24 months after | 188.71 | 200.79 | 198.11 | 197.29 |

[1]**SC province weights:** Cádiz 27.4%, Madrid 24%, València 23%, Zaragoza 16.3%, Bizkaia 4.8%, and Córdoba 4.5%.

[2]**SDID province weights:** Zaragoza 14.2%, València 13.6%, Madrid 12.1%, Cádiz 10.7%, Bizkaia 10.2%, Alacant 8.8%, Sevilla 8.6%, Córdoba 7.9%, Málaga 7.4%, and Murcia 6.4%.

# 5 Results

This section presents my results on the employment effect of the transit subsidy launched in Barcelona in 2012 on unemployment assistance (UA) entrants. As described previously, the employment outcome I examine is their number of cumulative days in employment—over the first three, six, twelve, and twenty-four months after they entered UA. I first show results on the overall sample of UA entrants and then break them down by age. Finally, I provide placebo checks to assess the credibility of my findings.

## 5.1 Overall treatment effects

Figures 2a and 2b show my results on the subsidy's short- and long-run employment effects, respectively. Panels in these figures have three plots comparing the actual outcome for UA entrants in Barcelona (in red) and each of its three alternative control units (in blue). In particular, from left to right, this control unit is Madrid (DID), Barcelona's synthetic control (SC), and Barcelona's synthetic difference-in-differences control (SDID).[22] The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA.

Figures 2a and 2b convey two key messages. First, compared to DID, the SC and SDID methods generally track pre-treatment Barcelona more closely.[23] Second, all three methods point to small, albeit positive, overall treatment effects in the short and long run. Their sign and magnitude are indicated by the black arrows in each plot. *On average*, the three methods estimate treatment effects of 1 and 2 cumulative days in employment three and six months after UA entry, respectively (Panel a of Table 2); and 5 and 14 days one and two years after (Panel b of Table 2). In relative terms, the first two represent an increase of 14% and 8% in the estimated counterfactual days in employment, while the last two of 5% and 6%. Nevertheless, none of these effects is statistically different from zero at any conventional significance level.

While overall treatment effects are small and statistically insignificant, point estimates are comparable to those reported by Altmann et al. (2018) for another low-cost intervention. Altmann et al. (2018) conduct a randomized experiment to examine the effect of sending the unemployed a brochure tackling potential information barriers and lack of motivation. The authors find that treated jobseekers at risk of long-term unemployment gained about 4% in cumulative days in employment one year after treatment.[24] This treatment effect is of the same order of magnitude as the one I estimate on the overall sample of UA entrants in Barcelona, ranging from 2 to 8%, depending on the identification method (see Panel b of Table 2). This comparison thus suggests that *overall* treatment effects from subsidized spatial job search may parallel those of other low-cost activation policies. Furthermore, this parallelism lends credence to the results of this study.

[22]These two versions of *synthetic* Barcelona are constructed using the province weights reported in Figures A1.a and A1.b in the Appendix.

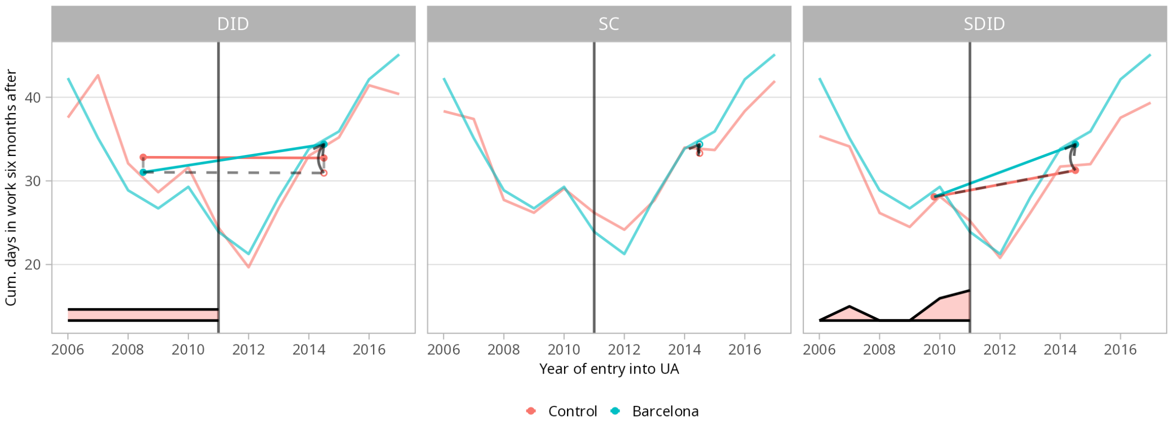
[23]The largest difference in pre-treatment fit emerges for Barcelona's long-run outcome, two years after UA entry (see bottom panel in Figure 2b).

[24]Workers at risk of long-term unemployment serve as a more reasonably comparable group to UA recipients —the jobseekers I study— than the overall pool of unemployed workers.

Figure 2a: DD, SC, and SDID treatment effects on employment — Full sample

*Cumulative days in work three months after*

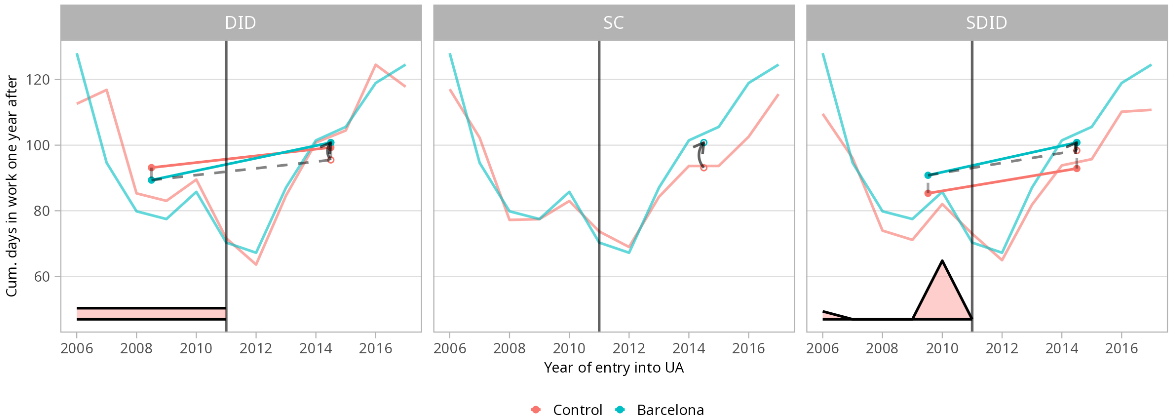

*Cumulative days in work six months after*



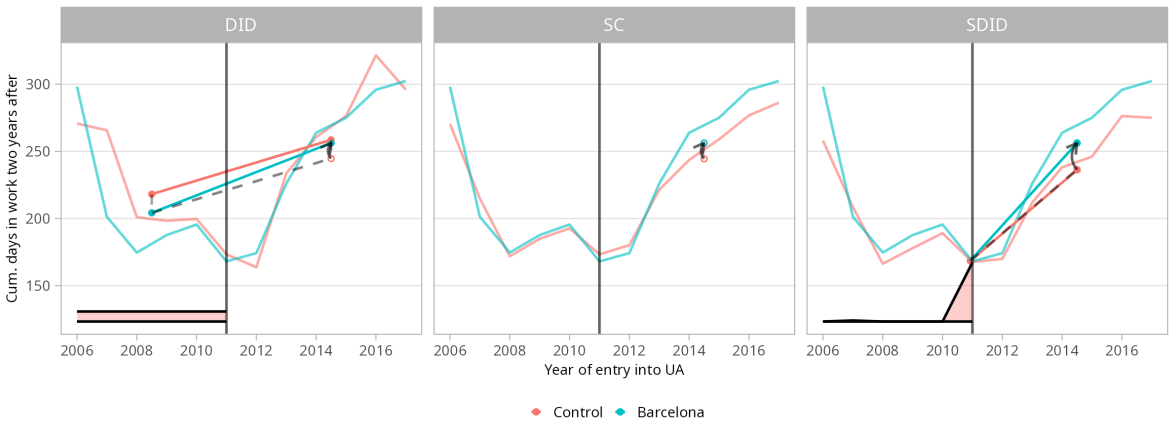

*Notes:* This figure shows the observed outcome for UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Figure 2b: DD, SC, and SDID treatment effects on employment — Full sample

*Cumulative days in work one year after*




*Cumulative days in work two years after*




*Notes:* This figure shows the observed outcome for UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Table 2: DID, SC and SDID treatment effects on employment — Full sample

*(a) Cumulative days in work three and six months after*

| | Three months after | | | Six months after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | 1.33 | 0.63 | 0.73 | 3.44 | 1.08 | 3.11 |
| Standard error | (1.25) | (1.48) | (1.57) | (3.46) | (4.44) | (4.55) |
| Counterfactual | 9.44 | 10.14 | 10.03 | 30.94 | 33.30 | 31.27 |
| Percentage | 14.09 | 6.21 | 7.28 | 11.12 | 3.24 | 9.95 |

*(b) Cumulative days in work one and two years after*

| | One year after | | | Two years after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | 5.31 | 7.70 | 2.43 | 11.55 | 11.72 | 19.78 |
| Standard error | (10.69) | (10.81) | (9.49) | (25.83) | (25.47) | (16.18) |
| Counterfactual | 95.50 | 93.10 | 98.37 | 244.68 | 244.51 | 236.44 |
| Percentage | 5.56 | 8.27 | 2.47 | 4.72 | 4.80 | 8.37 |

*Notes:* This table reports treatment effect estimates on UA entrants under the three methods outlined in Section 3. The outcome of interest is their average number of days in employment over a given period of time after entering UA. Standard errors are estimated using the placebo approach laid out in Section 3.3. The table also reports the estimated counterfactual outcome for the treated and treatment effects as a percentage of the latter.

## 5.2 Treatment effect heterogeneity by age

The previous findings suggest that the transit subsidy in Barcelona had no significant effect on the overall population of UA entrants, as measured by their cumulative days in employment after job loss. Nonetheless, these results likely mask fundamental heterogeneous effects as jobseekers differ in their potential gains from *spatial* job search and their reliance on *public* transportation. To examine this heterogeneity, I split my sample of UA entrants into two groups: those aged between 27 and 35 ("younger") and those over 35 ("older"). Younger jobseekers are more likely to rely on low-skill services jobs. These are jobs for which employers often use local recruiting methods, such as wanted signs, and hence for which spatial job search may prove most productive. Moreover, the young are also more likely to rely heavily on public transport and thus take up the subsidy.

### 5.2.1 Younger UA entrants

Figures 3a and 3b present my results on the employment effect of subsidized transit on *younger* UA entrants. Similarly to Figures 2a and 2b, they compare the actual outcome for Barcelona and each of its alternative control units before and after treatment.[25]

[25] Figures A2.a and A2.b in the Appendix report the province weights used to construct Barcelona's synthetic (SC) and synthetic difference-in-differences (SDID) controls for the sample of younger UA entrants.

The figures show positive and relatively large employment effects on younger entrants using either of the three methods I implement. Reassuringly, the effect of subsidized transit emerges only gradually, resembling the increasing take-up rate shown in Section 2. *On average*, the three methods suggest that the subsidy helped younger assistance recipients gain 3 and 6 days in employment three and six months after UA entry, respectively (Panel a of Table 3), and 16 and 36 days one and two years after (Panel b of Table 3). The first two estimates represent an increase of 33% and 21% in the estimated counterfactual outcome and the last two of 18% and 16%. While these are economically significant gains in employment, only the synthetic control treatment effects are precisely estimated for all time horizons after UA entry.

To assess the importance of these statistically and economically significant employment gains, I compare them to those found by Goller et al. (2021) for *traditional* activation programs. The authors evaluate the effectiveness of job training and placement services on UA recipients in Germany. A salient feature in their study is that the implementing job centers had considerable freedom in designing programs, thus likely boosting their impact on beneficiaries. Goller et al. (2021) estimate a *job training* effect of 17 cumulative work days two years after treatment and a *placement service* effect of 36 days.[26] Moreover, they find that, by and large, treatment effects for younger UA recipients are not greater than those for the overall sample. In relative terms, these effects correspond to employment gains of 16% for job training and 31% for placement services. These are sizeable treatment effects against which those of subsidized spatial job search —a relatively low labor-intensive intervention— still hold relevance. In particular, synthetic control estimates for younger UA entrants in Barcelona suggest employment gains of 22% (49 days) from subsidized transit —Panel b of Table 3.

While the previous comparison with Goller et al. (2021) is policy-relevant, it is not without caveats. Indeed, differences in treatment effect estimates across studies likely reflect differences in sample composition, labor market conditions, and institutions, and not only in program type. Nevertheless, with these caveats in mind, such comparison suggests that transit subsidies may bring meaningful employment gains for low-income, young unemployed jobseekers.

#### 5.2.2 Older UA entrants

In contrast to the young, *older* UA entrants do not seem to benefit from subsidized transit. Indeed, all three methods (DID, SC, and SDID) estimate substantially smaller and statistically insignificant treatment effects on their cumulative work days in the short and long run (see Figures 4a–4b and Table 4).[27] As mentioned before, these results are likely due to a relatively low take-up rate and more limited returns on

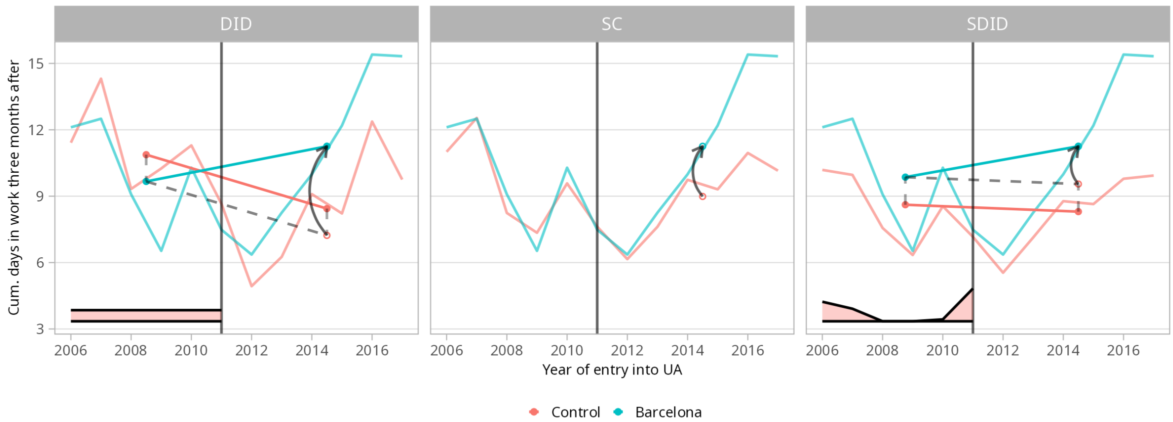
[26] I estimate these *overall* treatment effects by taking the simple average of the effects for men and women. Moreover, I compute treatment effects *two* years after treatment by taking those three years after and subtracting treatment effects between months 25 and 36. These are reported in Panels A and B of Table 3 in Goller et al. (2021). The authors also evaluate a third intervention, "reducing impediments", focused on "individual skills and employability". Treatment effects estimates for this intervention equal 19 cumulative employment days (18%), falling between those for job training and placement services.

[27] Figures A3.a and A3.b in the Appendix report the province weights used to construct Barcelona's synthetic (SC) and synthetic difference-in-differences (SDID) controls for the sample of older UA entrants.

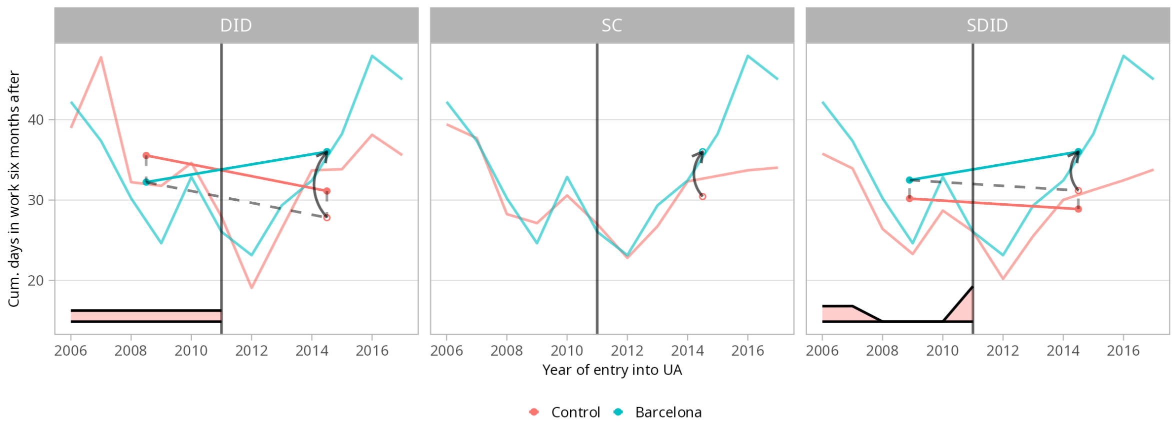
spatial job search. In particular, older jobseekers are less likely to rely on mass transport. More generally, they may face less severe cash constraints since they have more work experience and, thus, potentially higher wages and savings. Finally, they are less likely to rely on low-skill services jobs, the types of jobs where the returns on *spatial* job search may be highest.

Figure 3a: DD, SC, and SDID treatment effects on employment — Younger UA entrants

*Cumulative days in work three months after*



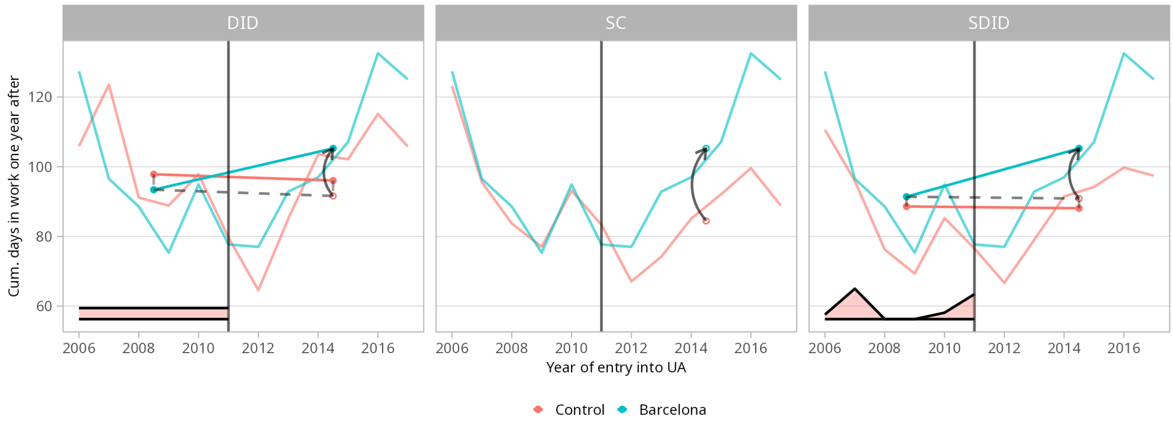

*Cumulative days in work six months after*




*Notes:* This figure shows the observed outcome for *younger* UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Figure 3b: DD, SC, and SDID treatment effecs on employment — Younger UA entrants

*Cumulative days in work one year after*




*Cumulative days in work two years after*


*Notes:* This figure shows the observed outcome for *younger* UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Table 3: DID, SC and SDID treatment effects on employment — Younger UA entrants

*(a) Cumulative days in work three and six months after*

| | Three months after | | | Six months after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | 4.03 | 2.27 | 1.70 | 8.21 | 5.58 | 4.84 |
| Standard error | (1.15) | (1.06) | (2.23) | (3.82) | (1.97) | (4.69) |
| Counterfactual | 7.23 | 8.99 | 9.55 | 27.81 | 30.44 | 31.18 |
| Percentage | 55.74 | 25.25 | 17.80 | 29.52 | 18.33 | 15.52 |

*(b) Cumulative days in work one and two years after*

| | One year after | | | Two years after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | 13.64 | 20.79 | 14.38 | 30.78 | 48.80 | 30.21 |
| Standard error | (10.73) | (7.22) | (16.51) | (25.76) | (19.36) | (25.07) |
| Counterfactual | 91.61 | 84.46 | 90.86 | 234.69 | 216.67 | 235.24 |
| Percentage | 14.89 | 24.60 | 15.83 | 13.12 | 22.52 | 12.85 |

*Notes:* This table reports treatment effect estimates on *younger* UA entrants under the three methods outlined in Section 3. The outcome of interest is their average number of days in employment over a given period of time after entering UA. Standard errors are estimated using the placebo approach laid out in Section 3.3. The table also reports the estimated counterfactual outcome for the treated and treatment effects as a percentage of the latter.

Figure 4a: DD, SC, and SDID treatment effects on employment — Older UA entrants

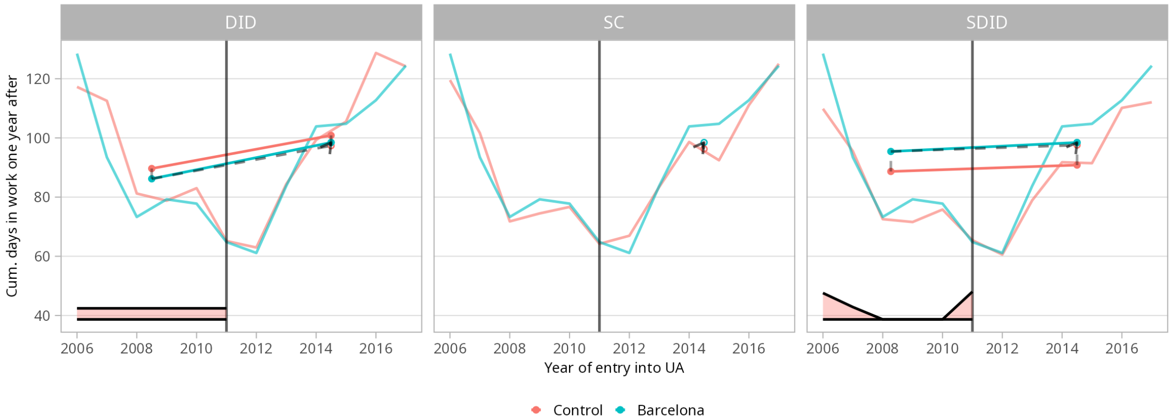
*Cumulative days in work three months after*


*Cumulative days in work six months after*

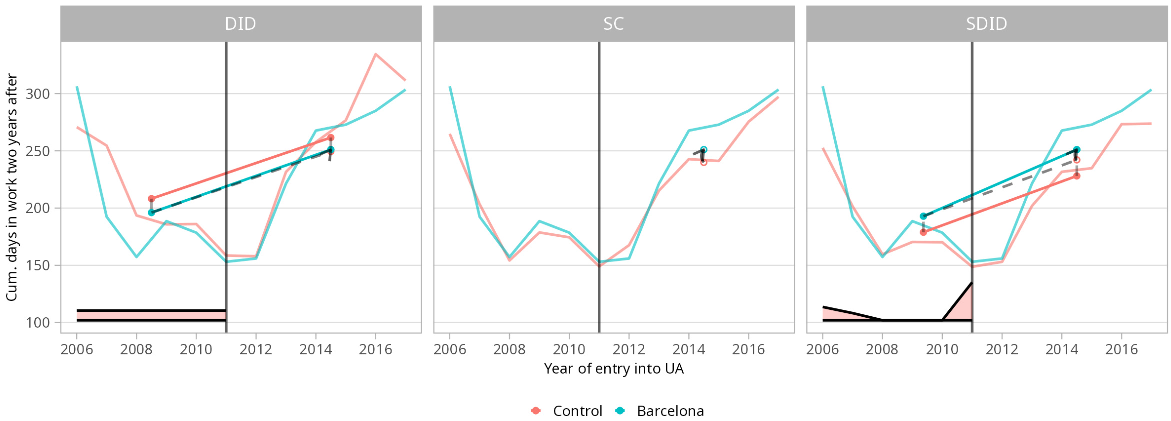

*Notes:* This figure shows the observed outcome for *older* UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Figure 4b: DD, SC, and SDID treatment effects on employment — Older UA entrants

*Cumulative days in work one year after*




*Cumulative days in work two years after*




*Notes:* This figure shows the observed outcome for *older* UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Table 4: DID, SC and SDID treatment effects on employment — Older UA entrants

*(a) Cumulative days in work three and six months after*

| | Three months after | | | Six months after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | -0.26 | 0.41 | -0.04 | 0.72 | 1.58 | 3.14 |
| Standard error | (1.71) | (1.67) | (1.80) | (3.61) | (3.62) | (4.51) |
| Counterfactual | 10.79 | 10.13 | 10.57 | 32.82 | 31.96 | 30.40 |
| Percentage | -2.41 | 3.95 | -0.38 | 2.19 | 4.94 | 10.33 |

*(b) Cumulative days in work one and two years after*

| | One year after | | | Two years after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | 1.08 | 2.18 | 0.87 | 1.63 | 11.18 | 8.99 |
| Standard error | (8.49) | (7.75) | (12.39) | (20.60) | (24.36) | (22.34) |
| Counterfactual | 97.37 | 96.27 | 97.58 | 249.44 | 239.90 | 242.08 |
| Percentage | 1.11 | 2.26 | 0.89 | 0.65 | 4.66 | 3.72 |

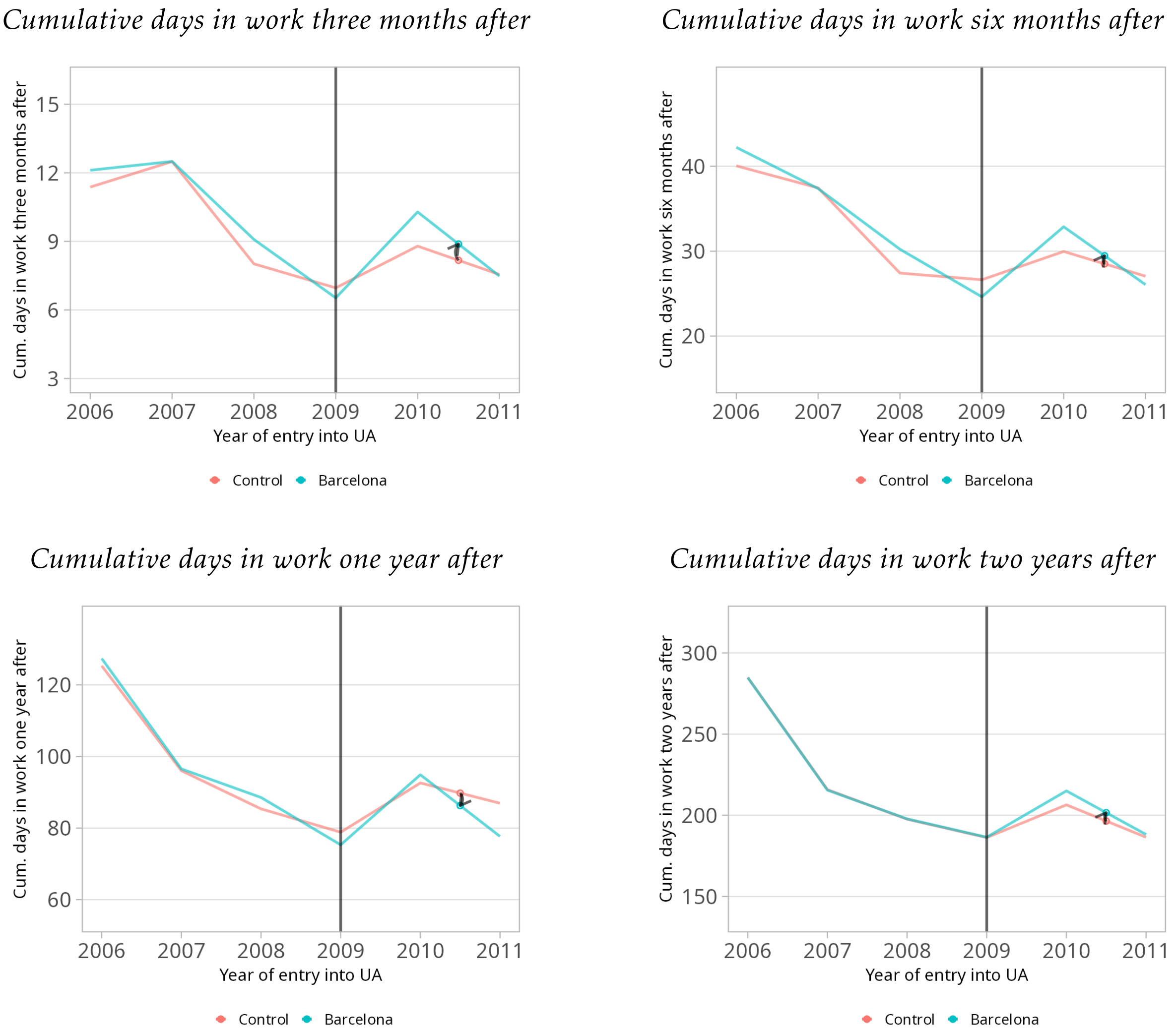
*Notes:* This table reports treatment effect estimates on *older* UA entrants under the three methods outlined in Section 3. The outcome of interest is their average number of days in employment over a given period of time after entering UA. Standard errors are estimated using the placebo approach laid out in Section 3.3. The table also reports the estimated counterfactual outcome for the treated and treatment effects as a percentage of the latter.

## 5.3 In-time placebo study

As previously shown, the synthetic control method suggests that the transit subsidy in Barcelona boosted employment among younger UA entrants. To assess the credibility of this finding, I artificially re-assign the start of the subsidy to 2010, two years before its actual start, and recalculate synthetic control weights, solving equation 3. I then estimate placebo effects on the young for the *remaining* pre-treatment years. In the spirit of Abadie et al. (2015), large placebo effects would undermine the credibility of treatment effect estimates as reflecting causality and not potentially poor predictive power of the synthetic control.

Figure 5 displays these in-time placebo effects. For comparability, I use the same corresponding scale in Figures 3a and 3b showing *treatment* effects. Similarly to this figure, the blue and red lines depict the observed outcome for Barcelona and its synthetic control, respectively, but with the latter using pre-treatment years *2006-2009 only*. Figure 5 shows in-time placebo effects that are substantially smaller than the treatment effects I estimate when using the *actual* start year of the transit subsidy in Barcelona (Figures 3a and 3b). These placebo effects thus reassure our confidence in the potential employment gains from subsidized spatial job search for younger UA recipients.

Figure 5: Synthetic control placebo effects on employment — Younger UA entrants




*Notes:* This figure shows the outcome for *younger* UA entrants in actual and synthetic Barcelona *before* the true start of treatment. The x-axis indicates the year of entry into UA, with the black vertical line artificially separating pre- and "post" treatment cohorts. The y-axis shows their average number of days in employment over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the placebo effect estimate.

## 5.4 Effect on earnings

To assess whether the *employment* gains for the young are paralleled by gains in *earnings*, I focus on the subsample of youths with wage data over a given observation window after entering UA. The larger this window, the higher the percentage of individuals with missing (cumulative) earnings. Therefore, I only estimate *short-run* effects on this outcome three and six months after entry into UA.[28] Figure 6 shows these results, comparing observed earnings for young UA entrants in Barcelona (treated) and each of its alternative control units. Similarly to Figure 3a, from left to right, these controls are Madrid (DID), Barcelona's synthetic control (SC), and

[28]The percentage of observations with missing cumulative earnings three and six months after UA entry is 5% and 9%, respectively, while 18% and 32%, one and two years after.

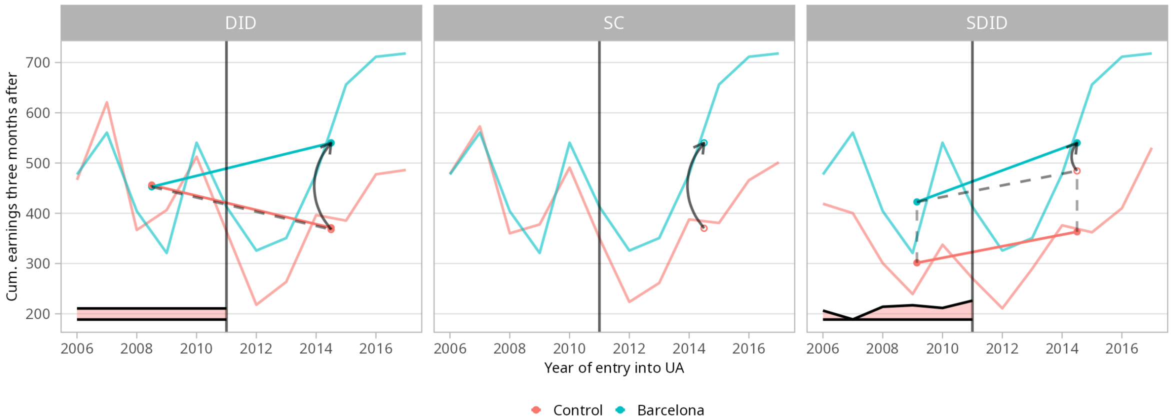
Barcelona's synthetic difference-in-differences (SDID) control.[29] Overall, Figure 6 suggests that the employment gains brought by the transit subsidy were *not at the expense* of lower earnings. While the three methods estimate *positive* effects on earnings three and six months after UA entry, there are two reasons to take them with some caution. First, as Table 5 shows, estimates vary considerably across methods: from 56 to 172 euros three months after UA entry and from 68 to 384 euros six months after. In relative terms, these effects range from 11% to 47% and from 4% to 30%, respectively. Second, the two methods suggesting economically and statistically significant gains in earnings (DID and SC) have a relatively poor pre-treatment fit. In fact, while Barcelona's synthetic control achieves a good pre-treatment fit for cumulative *employment* (Figure 3a), it provides a relatively poor fit for earnings. This lower quality in pre-treatment fit is likely due to earnings including more random noise since they capture both cumulative employment and average wages and are estimated on a smaller sample.[30]

[29]Figure A4 in the Appendix reports the province weights used to construct Barcelona's synthetic (SC) and synthetic difference-in-differences (SDID) controls for the sample of younger UA entrants with wage data three and six months after entering UA.

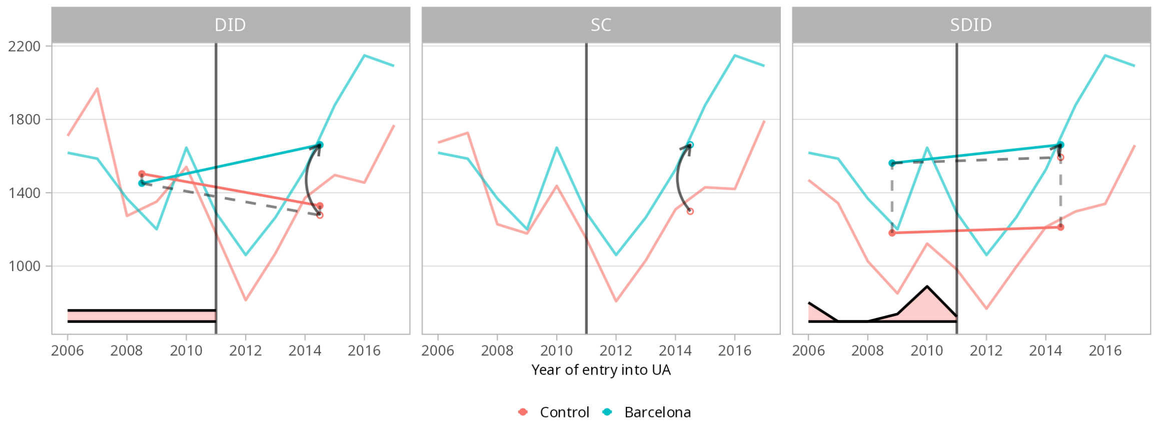
[30]Treatment effect estimates on youth employment (three and six months after UA entry) are qualitatively similar if we use this smaller sample with earnings data rather than the *full* sample. The only estimate that changes substantially is the SDID effect six months after UA entry. Table A1 in the Appendix reports employment estimates on both samples.

Figure 6: DD, SC, and SDID treatment effects on earnings — Younger UA entrants

*Cumulative earnings three months after (in 2016 euros)*




*Cumulative earnings six months after (in 2016 euros)*




*Notes:* This figure shows the observed outcome for *younger* UA entrants in Barcelona and each alternative control unit constructed under the three methods outlined in Section 3. In particular, from left to right, the control unit is Madrid (DID), Barcelona's synthetic control (SC), and synthetic difference-in-differences (SDID) control. The x-axis indicates the year of entry into UA, with the black vertical line separating pre- and post-treatment cohorts. The y-axis shows their average cumulative earnings (in 2016 euros) over a given period of time after entering UA. Black arrows indicate the sign and magnitude of the treatment effect estimate of each method, as specified in equations (2), (4), and (7). The shaded red areas in the DID and SDID plots show the relative weights assigned to each pre-treatment year when estimating treatment effects. Under the DID method, all pre-treatment years are assigned equal weights by construction. Nevertheless, under the SDID method, they are reweighted to best match the average post-treatment outcome for all control provinces, as in (6).

Table 5: DID, SC and SDID treatment effects on earnings — Younger UA entrants

| | Cumulative earnings three months after (in 2016 euros) | | | Cumulative earnings six months year after (in 2016 euros) | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| Estimate | 172.37 | 169.99 | 55.76 | 384.38 | 363.08 | 68.59 |
| Standard error | (50.46) | (76.56) | (163.32) | (122.31) | (164.97) | (301.34) |
| Counterfactual | 367.75 | 370.13 | 484.35 | 1277.30 | 1298.60 | 1593.04 |
| Percentage | 46.87 | 45.92 | 11.51 | 30.09 | 27.96 | 4.31 |

*Notes:* This table reports treatment effect estimates on *younger* UA entrants under the three methods outlined in Section 3. The outcome of interest is their average cumulative earnings (in 2016 euros) over a given period of time after entering UA. Standard errors are estimated using the placebo approach laid out in Section 3.3. The table also reports the estimated counterfactual outcome for the treated and treatment effects as a percentage of the latter.

## 5.5 Cost-benefit analysis

In this section, I provide a rough back-of-the-envelope cost-benefit analysis of the transit subsidy for younger UA claimants. As shown previously, the results of this paper are consistent with the hypothesis that younger jobseekers rely more heavily on mass transport and low-skill services jobs, the types of jobs where spatial job search may be most productive. The results thus highlight the importance of targeted subsidized spatial job search.

To assess the cost-effectiveness of this targeted policy, I consider the benefits and costs from the government's perspective. While a societal perspective would be more informative, the available data precludes a more comprehensive analysis, particularly the limited wage data to precisely estimate treatment effects on earnings.

In this simple cost-benefit analysis, I focus on the longest time horizon I examine, two years after entry into UA. The benefits I consider are the reduced UA payments and social security contributions that result from employment gains. Reduced UA payments are straightforward to estimate since UA benefits are flat (427 euros per month). In contrast, social security contributions vary with earnings and type of job contract. For simplicity, I assume that individuals earn the minimum wage and work under a temporary contract. In this case, contributions amount to 38% of the gross minimum wage in Spain (288 euros per month).[31] Total estimated benefits per younger claimant, therefore, equal 1168 euros.[32] On the cost side, I consider the total amount of the transit subsidy borne by the government. This amount depends on two variables, the subsidy level per transit pass and the duration of the subsidy. The first ranges from 40 to 130 euros per month, depending on the number of transport zones where the pass is valid. The second hinges on the time younger jobseekers need to realize employment gains from subsidized spatial search. For simplicity, I assume that they take up the subsidy for as long as they remain unemployed.[33] Considering

[31] I consider the average minimum wage during the post-treament sample period.

[32] Specifically, these are estimated by adding up reduced UA payments and gains in social security contributions (427+288=715 euros), and multiplying this sum by the synthetic control treatment effect on employment, expressed in months (49 days/30=1.63 months, see Panel b of Table 3).

[33] More precisely, for as long as they remain in non-employment until their first job transition.

these cost variables, I calculate the *break-even time exposure* to the subsidy, measuring the time a jobseeker may use subsidized transit before its costs surpass its benefits. Moreover, I compute this break-even point for each number of transport zones where the transit pass may be valid.

Considering the observed distribution of unemployment duration for younger UA claimants in Barcelona, the previous cost-benefit analysis suggests that the transit subsidy pays for itself for a sizeable share of them.[34] In particular, the subsidy more than breaks even for nearly three-quarters of them, regardless of the number of transport zones covered by the transit pass.

# 6 Conclusion

In this paper, I exploit a natural experiment in Catalonia in 2012 to study the employment effect of subsidizing public transport on unemployment assistance (UA) recipients. Using difference-in-differences and synthetic control methods, I find that subsidized transit may create meaningful employment gains for *younger* UA entrants in the short and long run. More precisely, synthetic control estimates indicate employment gains for the young ranging from 18% to 25% of their estimated counterfactual outcome, three to twenty-four months after entering UA. Meanwhile, employment gains for older UA entrants or the overall sample are small and statistically insignificant. This treatment effect heterogeneity is consistent with the hypothesis that younger jobseekers rely more heavily on public transport and low-skill services jobs, the types of jobs where spatial job search may be most productive. In addition, I find suggestive evidence that youth employment gains brought by subsidized transit may not come at the expense of lower earnings.

Therefore, in light of it being a simple low-cost policy, these findings suggest that targeted subsidized transit may be a cost-effective intervention to help the unemployed, especially those most reliant on public transport and spatial job search. A rough back-of-the-envelope cost-benefit analysis suggests that this targeted intervention may pay for itself for nearly three-quarters of younger UA claimants in Barcelona province. While this analysis is far from comprenhensive, it warrants further research on the labor market effects of subsidized spatial job search. More broadly, this analysis underscores the need for further work on the effects of simple and low-cost active labor market policies.

Finally, due to data limitations, I cannot test the mechanisms through which subsidized transit may help the unemployed. Nonetheless, this empirical analysis is a topic of future work.

[34]In this exercise, I consider the observed distribution of *post-treatment* cohorts.

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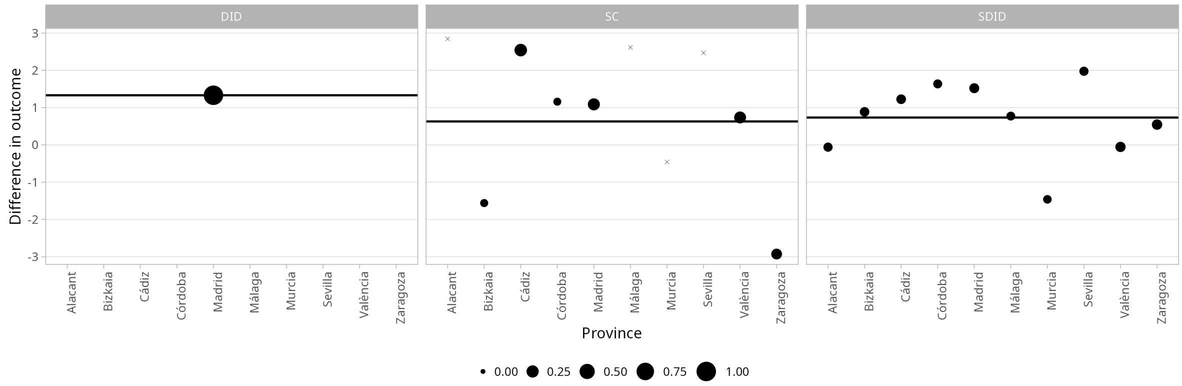
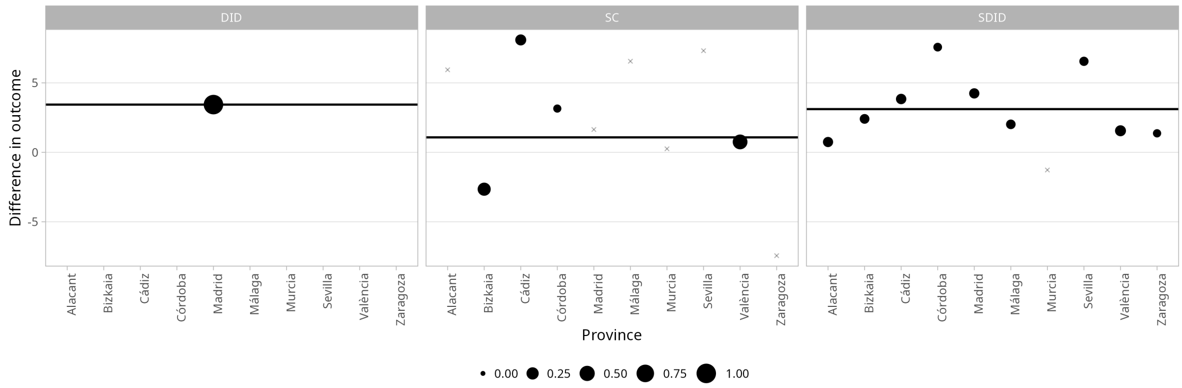
# Appendix

Figure A1.a: DD, SC, and SDID province weights for employment — Full sample

*Cumulative days in work three months after*



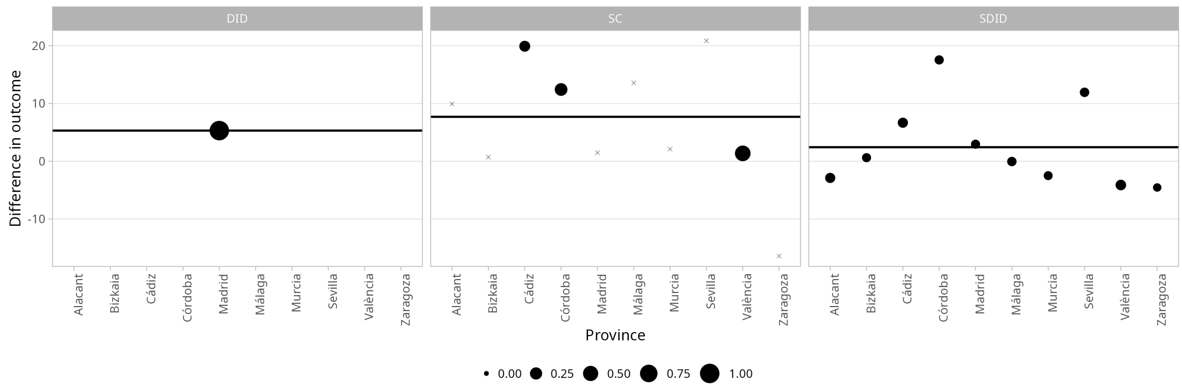

*Cumulative days in work six months after*



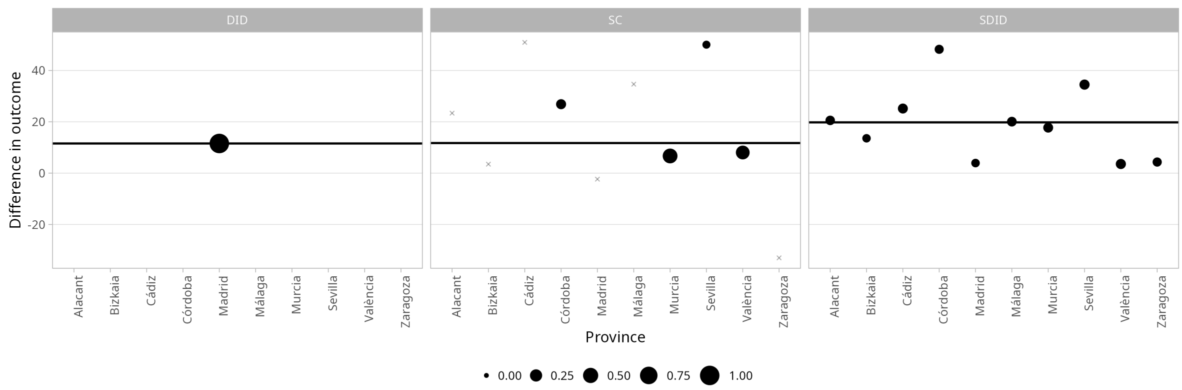

*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p \neq 1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an $\times$ symbol. Provinces are ordered alphabetically.

Figure A1.b: DD, SC, and SDID province weights for employment — Full sample

*Cumulative days in work one year after*



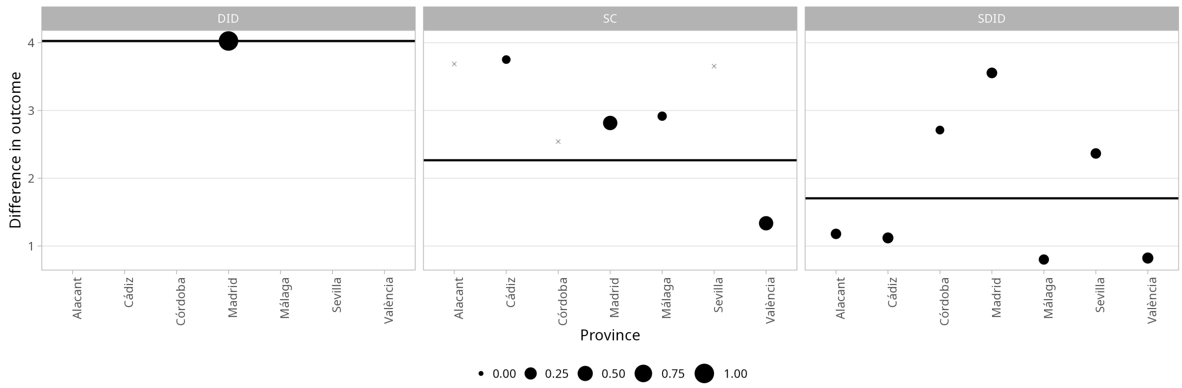

*Cumulative days in work two years after*



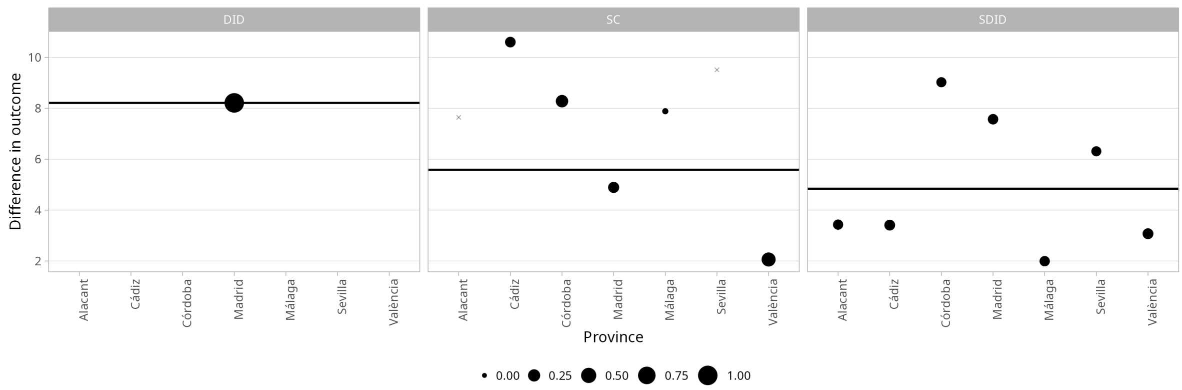

*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p\neq 1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an × symbol. Provinces are ordered alphabetically.

Figure A2.a: DD, SC, and SDID province weights for employment — Younger UA entrants

*Cumulative days in work three months after*



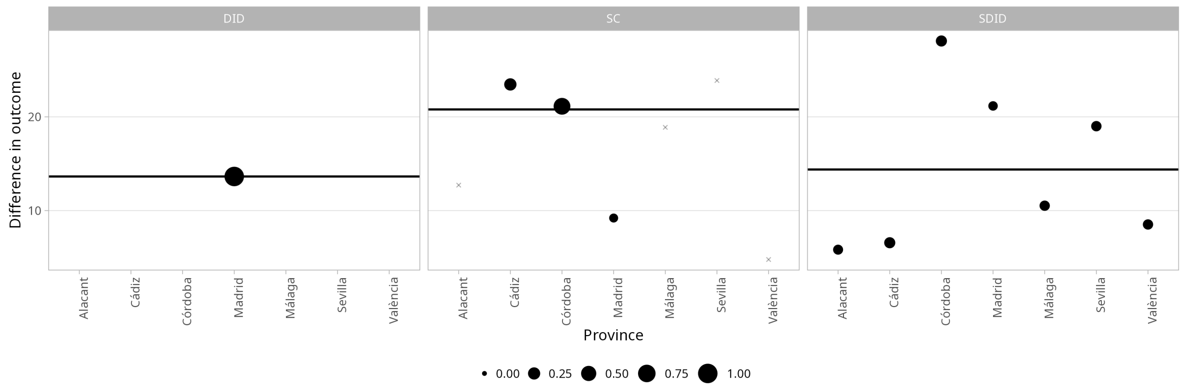

*Cumulative days in work six months after*



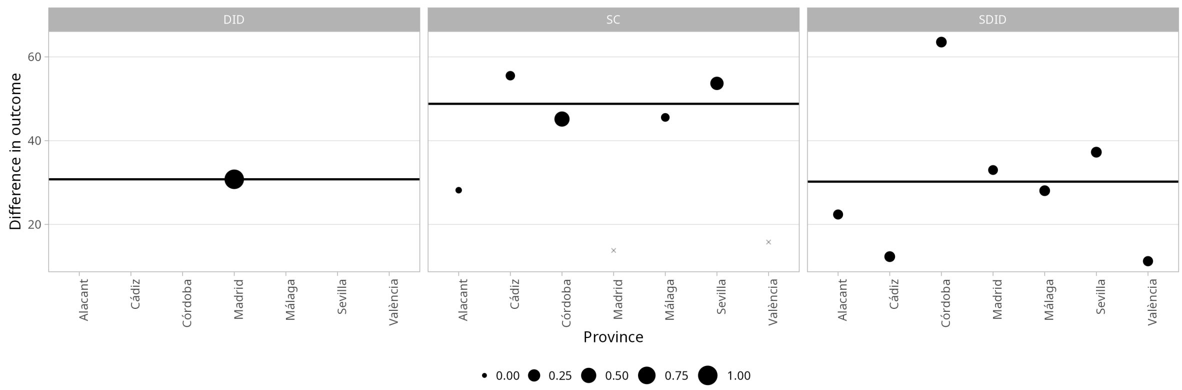

*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p\neq 1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an × symbol. Provinces are ordered alphabetically.

Figure A2.b: DD, SC, and SDID province weights for employment — Younger UA entrants

*Cumulative days in work one year after*



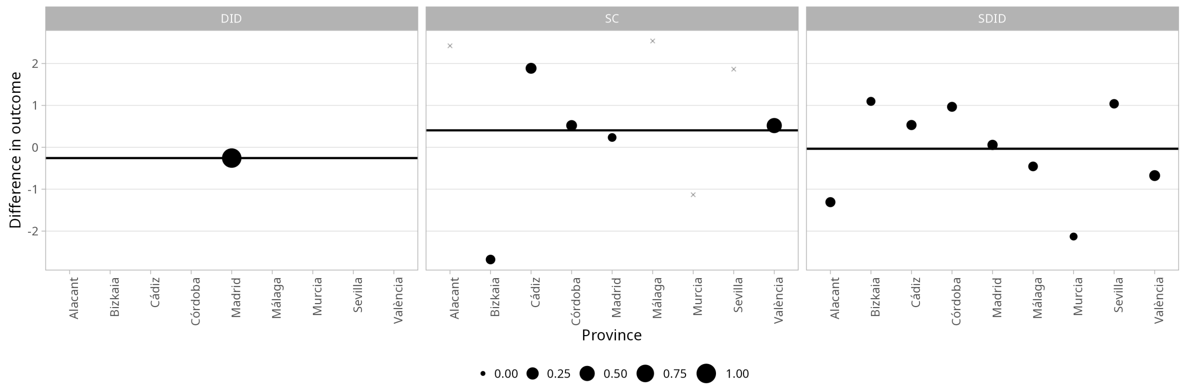

*Cumulative days in work two years after*



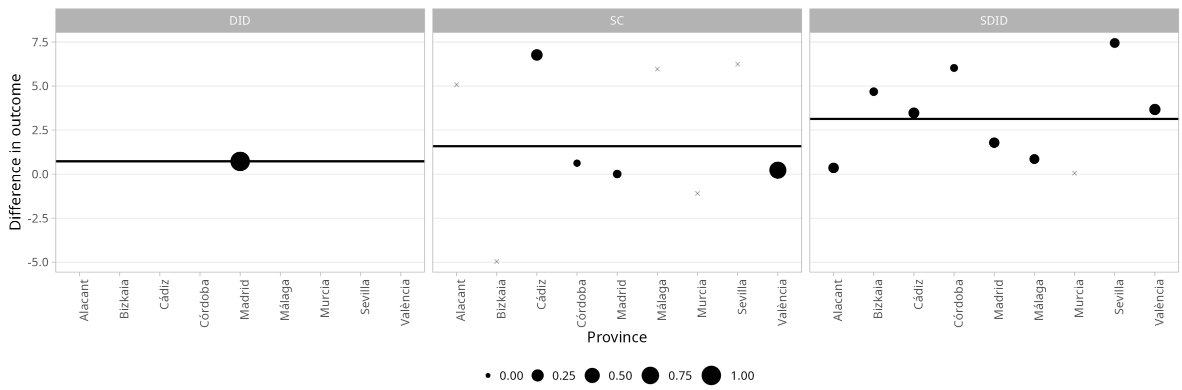

*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p \neq 1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an $\times$ symbol. Provinces are ordered alphabetically.

Figure A3.a: DD, SC, and SDID province weights for employment — Older UA entrants

*Cumulative days in work three months after*

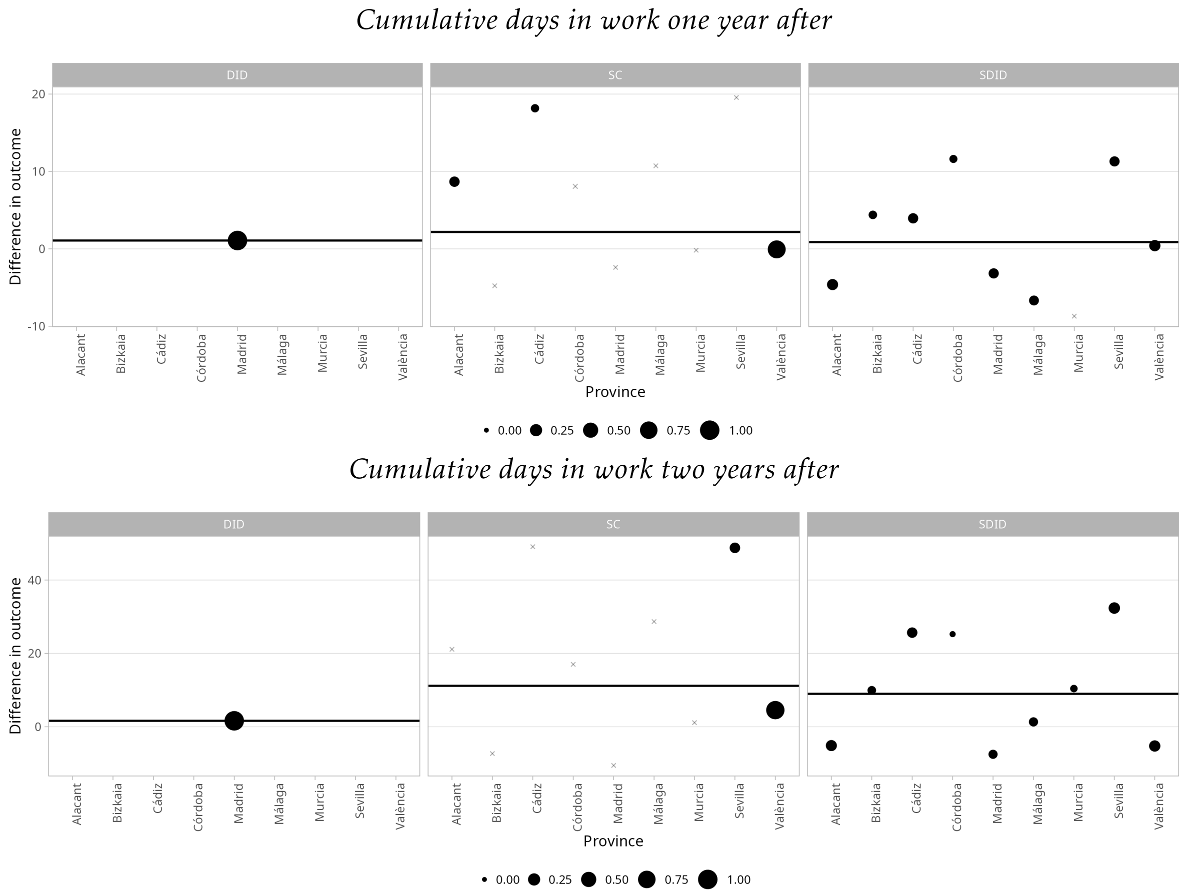



*Cumulative days in work six months after*




*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p \neq 1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an $\times$ symbol. Provinces are ordered alphabetically.

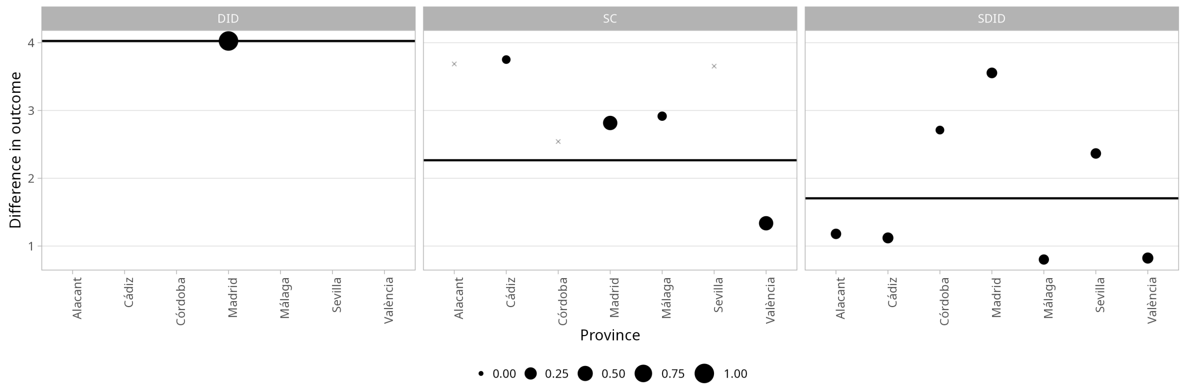
Figure A3.b: DD, SC, and SDID province weights for employment — Older UA entrants



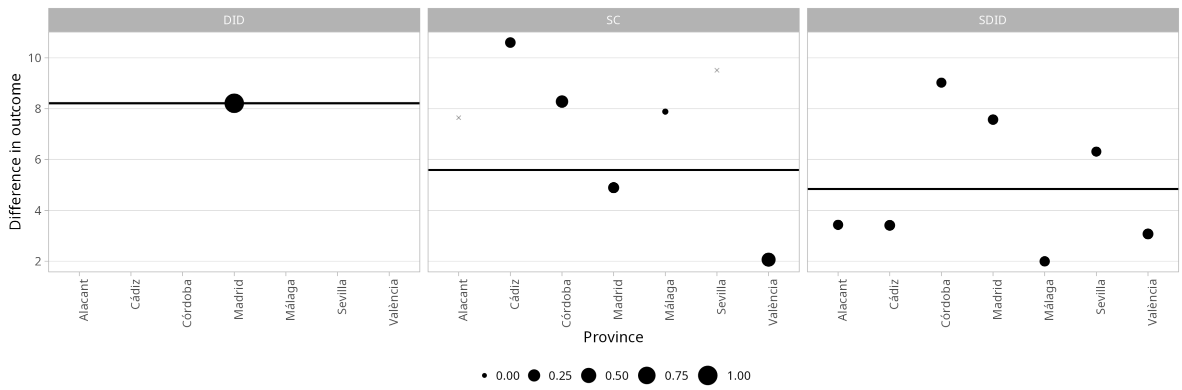

*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p\neq 1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an × symbol. Provinces are ordered alphabetically.

Figure A4: DD, SC, and SDID province weights for earnings — Younger UA entrants

*Cumulative earnings three months after*




*Cumulative earnings six months after*




*Notes:* This figure shows the outcome difference $\hat{\delta}_1 - \hat{\delta}_{p\neq1}$ between Barcelona ($p = 1$) and each available control province $p \neq 1$, as defined in (2), (4) and (7). The weight of each control province is indicated by its dot size. Provinces with zero weight have an × symbol. Provinces are ordered alphabetically.

Table A1: DID, SC and SDID treatment effects on employment — Younger UA entrants

| | Cumulative days in work three months after | | | Cumulative days in work six months year after | | |
|---|---|---|---|---|---|---|
| | DID | SC | SDID | DID | SC | SDID |
| | *Panel A: All younger entrants* | | | | | |
| Estimate | 4.03 | 2.27 | 1.70 | 8.21 | 5.58 | 4.84 |
| Standard error | (1.15) | (1.06) | (2.23) | (3.82) | (1.97) | (4.69) |
| Counterfactual | 7.23 | 8.99 | 9.55 | 27.81 | 30.44 | 31.18 |
| Percentage | 55.74 | 25.25 | 17.80 | 29.52 | 18.33 | 15.52 |
| | *Panel B: Younger entrants with available earnings* | | | | | |
| Estimate | 3.85 | 2.70 | 1.63 | 6.85 | 5.08 | 1.62 |
| Standard error | (0.71) | (0.86) | (2.53) | (2.00) | (2.39) | (5.11) |
| Counterfactual | 6.90 | 8.04 | 9.11 | 26.75 | 28.53 | 31.99 |
| Percentage | 55.80 | 33.58 | 17.89 | 25.61 | 17.77 | 5.06 |

*Notes:* This table reports treatment effect estimates on young UA entrants under the three methods outlined in Section 3. Panel A shows estimates using the entire sample of younger entrants, while Panel B those using the subsample with available earnings six months after UA entry. The outcome of interest is their average number of days in employment over the three and six months after entering UA. Standard errors are estimated using the placebo approach laid out in Section 3.3. The table also reports the estimated counterfactual outcome for the treated and treatment effects as a percentage of the latter.