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The Effect of Unemployment Insurance Benefits on (Self-) Employment: Two Sides of the Same Coin?

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The Effect of Unemployment Insurance Benefits on (Self-)Employment: Two Sides of the Same Coin? *

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Abstract

Although a relevant share of firms is created out of unemployment and current active labor market policies in Europe often subsidize unemployed individuals to start their own businesses, little is known about the role of unemployment insurance (UI) generosity for self-employment. By using Spanish administrative data including so far inaccessible information on self-employment, we exploit a reform-driven exogenous cut in UI benefits to identify its causal effect on *general employment* and decompose it into the effects on self-employment and re-employment. Exploiting a discontinuity in the UI benefit schedule which changed as a result of the 2012 Spanish labor market reform, we estimate the causal reform effects on the extensive margin of (self-)employment and on unemployment duration. We find heterogeneous effects on the extensive margin: while the job-finding rate increases, the startup rate decreases. Over different time horizons, the negative effect on self-employment (35-50%) outweighs the positive effect on employment (5-33%). Therefore, omitting self-employment as a counterfactual outcome might lead to overestimate *general employment* effects. Our UI benefit duration elasticity estimates indicate that reduced UI benefits extend unemployment duration for individuals transitioning into self-employment but shorten unemployment for individuals finding re-employment. These results might be relevant for the (optimal) design of UI systems.

JEL-Classification: H75, J64, J65, J68, L26

Keywords: *Social Insurance*, *Self-Employment*, *Spain*, *Unemployment Insurance*

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1 Introduction

Reducing unemployment is a common public policy goal which becomes especially important during a period of economic crisis. For this reason, Unemployment Insurance (UI) policies aim to provide a social safety net while limiting moral hazard in order to promote re-employment and to reduce unemployment duration. In this context, most studies analyze how the generosity of UI systems in terms of Potential Benefit Duration (PBD) or UI benefit levels affects re-employment outcomes (e.g. Solon, 1985; Katz & Meyer, 1990; Card & Levine, 2000; Kolsrud, Landais, Nilsson, & Spinnewijn, 2018 and Atkinson and Micklewright (1991) for a critical literature review). However, little is known about the relationship between UI generosity and self-employment[1]. The channel from unemployment to self-employment is economically relevant, particularly in Spain where more than a quarter of all new firms is started out of unemployment each year.[2] Given the potential of successful startups to create additional employment or to boost innovation, and because self-employment is a common trajectory for individuals to exit unemployment, current active labor market policies in Europe often subsidize unemployed individuals to start their own businesses.[3] Thus, it is important to conduct research on understanding the role of UI benefits in the transition from unemployment to self-employment. More knowledge about this channel would also complete the picture of how the design of UI benefits affects all relevant post-unemployment outcomes – not only employment – and may lead to more efficient unemployment policies.

Our paper aims to shed light on this issue by analyzing the heterogeneous effects of UI benefit level changes on self-employment and employment in Spain. By exploiting reform-driven exogenous variation in UI benefit levels, we are the first to estimate the causal effects of a cut in UI benefits (holding PBD fixed) on the probability of exiting from unemployment into self-employment or employment, i.e. the union of both exit states, and decompose the overall effect into distinct causal effects on the self-employment probability (startup rate) and the employment probability (job-finding rate). We investigate the causal effects on the extensive margin and on the quality of post-unemployment labor market states, and compute unemployment duration elasticities for each potential exit state. Since most other studies investigate increases in UI generosity, our focus on analyzing a reduction in UI benefit levels is also novel within this field of research.

From a theoretical point of view, it is ex-ante unclear whether there is a differential effect of a change in UI benefit levels on self-employment as compared to employment. According to *standard search theory*, a cut in UI benefit levels will lower the reservation wage, and thus the opportunity costs

[1] Regarding the labor market status *self-employment*, the term *founder* refers to the person starting a firm which covers both firms with and without employees. The term *entrepreneur* is used to focus on a founder who continues to run a firm after having started it. The term *startup* refers to the act of starting a firm and is a synonym for *new firm*.

[2] Self-employment accounts for 10-15% of the labor force in the member countries of the Organization of Economic Co-operation and Development (OECD). Spain's self-employment rate is among the highest in the European Union (EU) - it varied between 16.4% and 17.9% during the 2010s (OECD, 2018). We find that between 2005 and 2018, 30-50% of founders have been unemployed before starting their firms in Spain. In Germany, about one quarter of startups emerged out of unemployment between 2005 and 2015 (Camarero Garcia & Murmann, 2020).

[3] In Spain, such policies have been used to address the high (youth) unemployment rates after the economic crisis of 2007/2008. For instance, in 2013 the Spanish government launched the *Strategy of Entrepreneurship and Youth Employment 2013-2016*. This program aimed at promoting self-employment among the unemployed youth through reductions in social security contributions (González Menéndez & Cueto, 2015). Garcia-Cabo and Madera (2019) provide a good overview of self-employment options in rigid labor markets like Spain.

of searching will decline which should increase search intensity. Consequently, the probability of exiting unemployment will increase and the actual unemployment duration will decrease (Mortensen, 1977; Schmieder, von Wachter, & Bender, 2016). In other words, the job-finding rate is expected to rise in response to a cut in UI benefit levels (see, e.g. Rebollo-Sanz and Rodríguez-Planas (2020) for evidence from Spain). However, a cut in UI benefits may also alter an individual's decision to become self-employed. If searching for business opportunities works exactly the same way as searching for regular employment, *standard search theory* would also predict an increase in the startup rate. Nevertheless, a different scenario is also possible: by taking general equilibrium effects into account, as the reservation wage for employment decreases and labor becomes cheaper, the number of job vacancies will increase. In this instance, we would again expect a higher job-finding rate, but also a relative decrease in the startup rate to occur, as more job vacancies do not affect the latter and only increase re-employment options. Taking both scenarios from *standard search theory* into account, the effect of a cut in UI benefits on the startup rate is ambiguous.

In the *entrepreneurial choice model*, individuals compare their expected returns from employment and self-employment, and choose the labor market status with a larger expected net income (Lucas, 1978; Kihlstrom & Laffont, 1979; Evans & Jovanovic, 1989). The basic versions of this model focus on the effect of personal characteristics[4] on the entrepreneurial choice problem. Alba-Ramirez (1994) expands the model with regard to unemployment.[5] The model argues that due to the cut in benefits, actual UI duration decreases for similar reasons as in the *standard search model*. Hence, shorter unemployment duration implies less negative unemployment duration dependence (e.g. less human capital depreciation or stigma effects as illustrated by Jarosch and Pilossoph (2019)), and thus, relatively better employment prospects compared to a setting without a cut in UI benefits. Consequently, the expected employment income remains relatively higher, suggesting that the job-finding rate should increase. With regard to the expected self-employment income, shorter actual UI benefit duration, however, implies a shorter period of learning regarding market opportunities, which might lower the expected quality of business ideas and thus potential returns from self-employment. According to this model, a cut in UI benefits could therefore negatively affect entrepreneurial success, which would consequently lead to worse self-employment prospects. Thus, the *entrepreneurial choice model* predicts rather a decrease in the startup rate, while the job-finding rate is expected to increase.

Both theories predict a positive effect on the job-finding rate, but also ambiguous effects on the startup rate. In this study, we provide empirical clarification. We are aware that we cannot fully identify the right model because there is within-model ambiguity, and our results might be partially in line with both theories. Nevertheless, we provide new evidence on which future models can base themselves.

[4]These characteristics include entrepreneurial skills (Lucas, 1978; Evans & Jovanovic, 1989), risk preferences (Kihlstrom & Laffont, 1979), and capital constraints (Evans & Jovanovic, 1989).

[5]Alba-Ramirez (1994) estimates the determinants of the self-employment probability in a sample of previously employed workers and UI recipients. Given the individual was a former UI recipient, the author finds that the probability for self-employment significantly increases with longer UI spell duration. Unfortunately, his estimates may suffer from selection bias as individuals who remain recipients of UI benefits are not taken into account.

We focus on the Spanish UI system and use comprehensive administrative data from the Continuous Working Life Sample - *Muestra Continua de Vidas Laborales* (MCVL). We extract the MCVL's information on self-employment which is mostly inaccessible when it comes to similar data of other countries. In our descriptive analysis, we validate the new information on self-employment against official data. Our main analysis exploits a Spanish labor market reform in 2012 which led to a sharp change in UI benefits: with the reform, the net replacement rate for the time after 180 days of benefit receipt decreased by 10 percentage points (from a replacement rate of 60% to one of 50%). Only individuals entitled to more than 180 days of UI benefits receipt can be affected by this reform. This quasi-experimental set-up allows us to exploit exogenous variation in UI benefit levels, in order to estimate the causal effect of a cut in UI benefits on (self-)employment. We use a sample of UI recipients who were displaced from regular employment and apply a Regression Discontinuity Design (RDD), which relies on the running variable being the time interval between the UI entry date and the sharp reform cutoff date, to estimate our causal effects.

First, we estimate the causal effect of UI benefits on the probability of exiting from unemployment into self-employment or employment, i.e. the union of both exit states. We then decompose the overall effect into distinct causal effects on the startup rate and the job-finding rate. When estimating the effect on self-employment, we consider unemployment and employment as counterfactual outcomes (vice versa for the effect on employment) and, thus, take all possible labor market states[6] into account. Second, we estimate the causal reform effect on the unemployment spell duration for individuals who become self-employed and those who get re-employed and calculate distinct UI benefit level duration elasticities. Third, we analyze the causal relationship between UI benefits and the quality of post-unemployment labor market states to infer potential welfare implications.

Our findings show that in response to the cut in UI benefit levels the self-employment probability declines within the first 180 days of unemployment (short term). This negative effect gets stronger if the considered time frame is increased up to 360 or 720 days (medium and long term, respectively). On the contrary, the probability of finding a job is positively affected in the short term whilst attenuating in the medium and long term. The effect on the union of both exit states is slightly positive on a short-term basis but attenuates towards zero in the medium and long term, suggesting that the positive effect on employment and the negative effect on self-employment cancel out each other. These results clearly show a behavioral response of treated individuals. They increase their search intensity to find employment before UI benefits drop after 180 days, which explains the (decrease) increase in the short-term (self-)employment probability. In terms of effect size our results show notable differences to a study conducted by Rebollo-Sanz and Rodríguez-Planas (2020)[7]: from the overall analysis, their RDD estimates point towards a local average treatment effect on the job-finding rate of 26%, while our estimates are placed in a range between 17% and 19%. Additionally, we find that the negative effect on self-employment (35-50%) is consistently

[6] We distinguish between unemployment, employment and self-employment. The unemployment state includes spells with benefit receipt but also spells unregistered (out of labor force) with social security authorities.

[7] Rebollo-Sanz and Rodríguez-Planas (2020) exploit the same Spanish labor market reform in 2012. They find that the benefit reduction shortens the mean expected unemployment duration by 14% and increases the job-finding rate by 26% compared to workers unaffected by the reform, using a RDD. Different to Rebollo-Sanz and Rodríguez-Planas (2020), we take all possible unemployment exit states into account and consider also long-term effects.

stronger than the positive effect on employment (5-33%) over different time horizons. Although Rebollo-Sanz and Rodríguez-Planas (2020) use the same dataset with access to information on self-employment, they exclude self-employed individuals from their sample. Our findings show that through the exclusion of individuals who transition from unemployment to self-employment, the estimated reform effect on the job-finding rate could potentially be upward biased, especially in the very short term (within the first 90 days of unemployment). This suggests that the exclusion of data on self-employment is not an innocuous sample selection criterion. Together with the presence of heterogeneity, the reform's general employment effect (on both self-employment and employment) could be substantially overestimated if self-employed workers are excluded from the sample.

We find that UI benefit levels affect the actual unemployment duration of unemployed individuals no matter whether they become re-employed or self-employed, but in opposing directions. We estimate a positive UI benefit duration elasticity of approximately 0.6-0.9 for those who become re-employed. Our estimate is higher compared to findings in other studies which usually estimate this elasticity based on reforms that extend UI generosity instead of reducing it, suggesting the existence of asymmetric effects depending on the direction of changes in UI generosity. Interestingly, we find a negative UI benefit duration elasticity for those transitioning from unemployment to self-employment (between -0.2 and -1.5). This finding could be explained through liquidity constraints, imposed by the cut in UI benefits which impact potential founders more so than individuals who search for regular jobs, due to the fact that those who decide to set up a business may need more time to collect necessary funding. Moreover, our estimated reform effect is stronger for self-employment than re-employment, i.e. the cut in UI benefit levels increases actual UI duration more for those transitioning to self-employment than it decreases UI duration for those transitioning to employment (joint elasticity is therefore 0.3-0.6). Nevertheless, we are cautious in interpreting our elasticity results, which are only barely statistically significant with respect to self-employment but mostly significant with regard to re-employment.

Finally, our analysis concludes that there is mixed evidence of the reform's effect on the quality of post-unemployment labor market states. The cut in UI benefits did not significantly affect the quality of post-unemployment startups or jobs. While re-employment wages appear to stagnate, our findings suggest that our proxy for self-employment income increases in response to the reform. Altogether, UI benefits affect the extensive margin of transitions into (self-)employment but not so much the quality of post-unemployment labor market states.

Our study relates to three strands of the literature. First, we contribute to the entrepreneurship literature (e.g. Evans & Leighton, 1989; Levine & Rubinstein, 2017) by providing evidence on the role of UI benefits for entrepreneurship in terms of the extensive margin and composition effects. Hombert, Schoar, Sraer, and Thesmar (2020) exploit a French reform in 2002 which lowered the downside risk of establishing a business and find that more self-employment is created when more social security is provided. We complement this finding by analyzing the causal effect of providing less security (less UI benefits) on self-employment. Second, our research adds to the optimal unemployment insurance literature which analyzes the optimal level of benefits and PBD (e.g. Schmieder, von Wachter, & Bender, 2012; Schmieder et al., 2016; Schmieder & von Wachter, 2016;

Kolsrud et al., 2018). So far, the focus has been on investigating effects on actual unemployment duration and subsequent employment outcomes. The effects of PBD extensions are disputed. For instance, Nekoei and Weber (2017) argue that longer PBD can either induce delay in job acceptance and thus simply subsidize leisure (disincentive effect), or improve job opportunities through promoting a longer search that results in job matches of higher quality. While Nekoei and Weber (2017) find that the latter positive effect is dominant in Austria, Schmieder et al. (2016) report negative effects of unemployment duration on re-employment wages in Germany. The literature agrees upon the disincentive effect with regard to UI benefit levels, i.e. an increase in benefit levels leads to an increase in actual unemployment duration and to a decrease in the job-finding rate (e.g., Rebollo-Sanz & Rodríguez-Planas, 2020; Meyer & Mok, 2014; Lalive, Van Ours, & Zweimüller, 2006). However, to the best of our knowledge, self-employment is usually ignored when these effects are estimated due to a lack of good data. Our study is the first to investigate the effect of UI benefits on both the job-finding and the startup rate. We show that the path from unemployment into self-employment is important and should be considered for the optimal design of UI systems. Third, we contribute to the literature on (un)intended consequences of economic crisis politics considering UI generosity changes which has mostly focused on the US (e.g., Farber, Rothstein, & Valletta, 2015; Card, Johnston, Leung, Mas, & Pei, 2015). As a matter of fact, the labor market reform that we analyze was one of the policies to deal with the aftermath of the Great Recession and was supposed to reduce unemployment under the pressure of fiscal consolidation. Rebollo-Sanz and Rodríguez-Planas (2020) and Doris, O'Neill, and Sweetman (2020) find that a non-standard response of cutting UI benefits in a crisis period increases the job-finding rate and reduces actual unemployment duration. We complement their findings by also estimating the effect on self-employment.

During the 2010s and also this present day, in light of the economic crisis that follows the COVID-19 pandemic, many European countries suffer from high unemployment rates which policymakers often aim to mitigate by easing the transition into self-employment.[8] This demonstrates why our research questions are highly relevant. Moreover, our research enables us to learn about the bias created in studies which ignore self-employment in their analysis and solely focus on employment. We believe that Spain is an interesting country to learn from because it allows us to investigate a policy (in times of crisis) with good internal validity and high data quality. Thus, we can contribute to the big picture of how UI generosity affects (self-)employment outcomes, which may be relevant for countries with similar economic conditions.

The paper proceeds as follows: Section 2 describes our data and provides a descriptive analysis of the Spanish labor market and all labor market flows over time (2005-2018). Section 3 explains the institutional background of social security in Spain and the labor market reform on which our identification strategy relies. Section 4 explains our estimation methodology and its underlying assumptions. Section 5 presents our results. Finally, Section 6 discusses our results and concludes.

[8] Laffineur, Barbosa, Fayolle, and Nziali (2017) find that such active labor market policies have a positive impact on the rate of *necessity*-driven entrepreneurship but no significant effect on the rate of *opportunity*-driven entrepreneurship.

2 Data and Descriptive Analysis

In this section, we describe our dataset and demonstrate its representativeness by illustrating to which extent we can match relevant labor market stocks and flows with official statistics. In this context, we provide a descriptive analysis of the Spanish labor market, with a particular focus on the transitions between unemployment, self-employment, and employment over time (during the time period 2005-2018). Thus, this section illustrates the relevance of our research questions, establishes important facts for future research and provides insights into how the data can be used.

2.1 MCVL Data

For our analysis we use Spain's Continuous Working Life Sample - *Muestra Continua de Vidas Laborales* (MCVL). It contains administrative information on individual socio-economic characteristics and longitudinal information on labor market statuses and job characteristics for a four percent non-stratified random sample of Spain's population. The MCVL takes into account individuals who were registered with the social security at any point between 2005 and 2018, but it also entails reliable employment histories retrospectively since the 1980s. MCVL data was released in 14 waves, the first occurring in 2005 and the most recent in 2018. As the anonymized identifiers are maintained, all MCVL editions can be combined. This allows a representative dataset to be created in which, as opposed to survey data, there is no problem concerning sample attrition.

MCVL data identifies five different labor market spells: 1) employment; 2) self-employment; 3) UI benefits receipt; 4) UA benefits receipt; and 5) inactivity. The retrospective nature of the data enables an individual's different labor market spells over an entire labor market history to be tracked. Starting from the point when the individual joined a social security scheme for the first time, the labor market trajectory can be tracked up until 2018. Naturally, the forthcoming spells after 2018 are right-censored with the exception of individuals who passed away beforehand. The spells 1) - 4) imply that the individuals are actively registered with the social security authorities, whereas individuals in spell 5) are unregistered. In addition to the labor market trajectories, MCVL data also contains job characteristics. For each employment spell, it provides information on sector, occupation, skill level required for this job, contract type (temporary vs. permanent, part-time vs. full-time), contribution basis, reason for dismissal, firm ownership (private vs. public), and the firm's location. As individual spell entry/exit dates can be observed, (self-)employment experience can be calculated as well.[9] The socio-economic characteristics entail an individual's age, sex, date of birth/death, country of birth, nationality, and formal education. From the province of residence, we can infer where each UI recipient is currently registered. Appendix E.1 provides more information on the MCVL data.

Restrictions. For the construction of the quarterly dataset which we use to obtain the relevant descriptive statistics, we limit our sample to individuals of working age, i.e. 18 years or older, who are included in the social security files from 2005 to 2018. However, some additional restrictions are

[9]Following the definition of De La Roca and Puga (2017), we compute experience as accumulated time spent in employment, starting from the first job in an individual's life.

necessary in order to carry out the RDD estimations. Details on the RDD sample are explained in Section 4.1. Additionally, we give an overview of our variables and data construction in Appendix E. Procedures to replicate our datasets and results can be gathered from our data documentations.

2.2 Other Data

While processing the MCVL data, the nominal contribution basis was deflated using the Consumer Price Index (CPI) with 2015 as a base year. Furthermore, some other macroeconomic indicators of interest for the data description and the analysis have been obtained from official sources. For instance, the local unemployment rate at the province level is used as a control variable in our regressions. Similarly, the annual unemployment rate and labor market data, such as the self-employment rate or labor force participation, have been extracted and used to generate the descriptive statistics shown in Section 2.3. Our indicators are drawn from the *Selected indicators for Spain* of the OECD (2018)[10] and the INE (2018)[11]. Official statistics on the number of beneficiaries and benefit levels have been extracted from Spain's Ministry of Labor (2020)[12].

2.3 Descriptives - Matching Labor Market Flows

This section documents how the main labor market states evolve in the period 2005-2018 in Spain, thereby confirming our accuracy in constructing the dataset by showing that we are able to match key labor market facts as provided by official bodies such as OECD or the Spanish National Statistics Institution (*Instituto Nacional de Estadística (INE)*). For clarity purposes, the terms Self-Employment (SE), Employment (E), Unemployment (U), and Out of Labor Force (OL) are abbreviated in our graphs.

Labor Force. The composition of the labor force is plotted in Figure A.1. The largest section of the labor force consists of employed workers. In 2005, their share was 78% of the labor force which subsequently declined due to the financial crisis from 2008 onwards until a share of approximately 60% was reached. This drop of 18 percentage points (p.p.) was absorbed by the unemployed individuals' share which increased after the crisis by an equivalent amount. The share of self-employed individuals remains roughly constant at 18%. A slight increase in the self-employment share is observable from 2013 onwards. When analyzing the age distribution of the labor force, Figure A.2 reveals that self-employment is more relevant for the older individuals (age groups over 40) than for younger individuals. The share of self-employed as percentage of the labor force is only around 10-15% for those younger than 40, whereas it ranges between 20-24% for those in the age groups above 40. A closer look at Spain's labor force in the OECD data reveals that a four percent sample should equal on average 913,000 individuals across the sample period (OECD, 2018).[13]

[10] OECD data for Spain can be retrieved from: `https://data.oecd.org/spain.htm`

[11] INE data for Spain can be retrieved from: `https://www.ine.es/dyngs/INEbase/en/listaoperaciones.html`

[12] Unemployment benefits statistics from Spain's Ministry of Labor (2020) can be retrieved from: `http://www.mitramiss.gob.es/estadisticas/PRD/welcome.htm`

[13] The Spanish average labor force level from 2005 until 2015 was approximately 22,817,000 individuals per year (OECD, 2018). Thus, a four percent sample should result in $0.04 \cdot 22,817,000 \approx 913,000$ individuals.

Evolution of the Spanish Labor Market. In Appendix Figure A.3, Spain's annual unemployment rate using MCVL and OECD data is illustrated for the sample period. The unemployment rates from both sources are based on individuals of working age, including all sectors and all social security schemes, such that they are comparable. It is important to note that the OECD restricts the working age population to individuals between 15 and 64 years old, whilst the INE's Working Conditions Survey focuses on individuals older than 16 years of age. We restrict our descriptive sample to individuals who are 18 years or older.[14] In spite of these differences, the computed unemployment rate using MCVL data is very similar to the quarterly unemployment rate reported by INE (left panel figure) and also matches OECD's annual unemployment rate (right panel figure). Concerning the self-employment rate, measured in terms of total employment, Figure A.4 confirms that our data cleaning process and the construction of our dataset from the MCVL data enable us to match quarterly statistics from INE (left-hand panel), as well as annual statistics from OECD data (right-hand panel). Specifically, Figure A.4 shows that self-employment has been slowly rising until reaching its peak in 2014 at nearly 20% and then declining again.

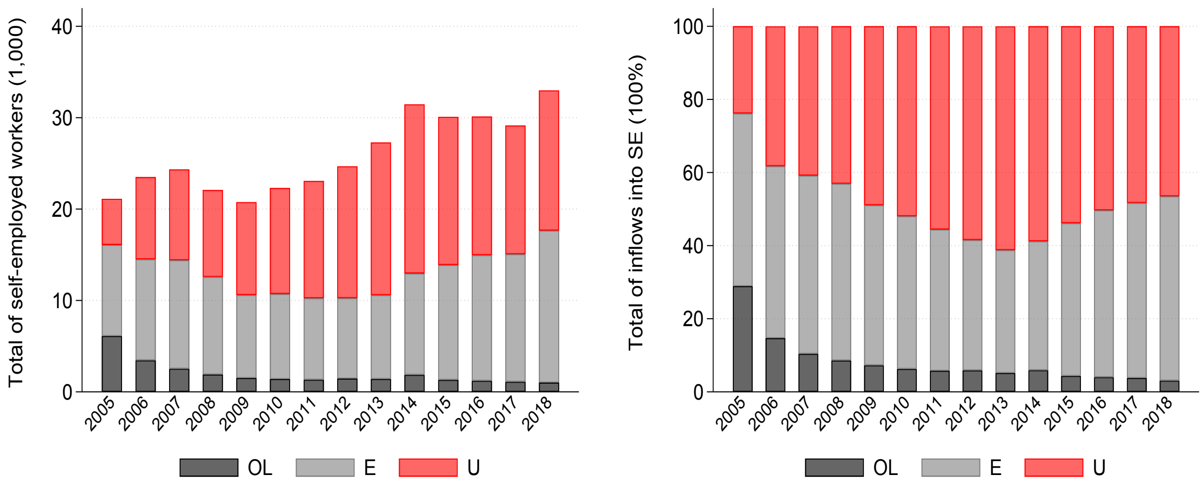
For completeness, Figure A.5 illustrates part-time and Figure A.6 temporary employment rates in Spain. Again, we compare our calculated data with both official statistics from Spain (quarterly INE data in the left-hand panels) and OECD data (annual data in the right-hand panels). While the part-time rate has continuously increased from 10% in 2005 to 15% by 2018, the temporary contract rate reflects an U-shape evolution. This is in line with the observation that during an economic crisis temporary contracts are not renewed, and therefore this group of workers is among the first to be laid-off (as can be seen from the drop of around 27% to 20% in the temporary employment contract rate during the crisis). In contrast, when the recovery started (in Spain at the end of 2013) temporary employment recovered first and surpassed pre-crisis levels in 2017.

Labor Market Flows. Figure 1 shows that the inflow into self-employment is considerably dominated by flows from unemployment. In other words, a relevant share of founders in Spain has been previously unemployed, and the inflow from unemployment into self-employment is important. This inflow from unemployment makes up 30-50% of all new self-employed individuals every year.[15] Moreover, the composition of inflows into self-employment exhibits counter-cyclical patterns, especially from 2010 onwards. While the share of inflows from previously employed workers decreases, the share of inflows from previously unemployed individuals increases during a crisis. Although outflows from unemployment to self-employment might only reflect 5% of the whole unemployment stock (Figure A.12), there are usually job spillovers, i.e. most founders have employees. Consequently, the economic significance of our object of interest is a multiple of the outflow statistics from unemployment to self-employment and is therefore quantitatively important. Thus, startups can be engines for economic growth. It is worth noting that the role of

[14] For a summary of the main sample characteristics, on a person basis, see the last two columns in Table B.1.

[15] In the Appendix, we also show the same figures including the stocks of self-employed. Looking at stocks of self-employed individuals in our representative sample shows that around 80% of the self-employed remain in self-employment in the following year (less during the crisis period): Figure A.7 shows the yearly inflows including the self-employment stock dimension, Figure A.8 shows the same for outflows from self-employment excluding the self-employment stock dimension. The graphs confirm that new inflows into self-employment are mainly composed out of new self-employed individuals who were previously unemployed or employed. In particular, the share of new inflows to self-employment out of unemployment increases until around 2013.

Figure 1: Composition of Inflows into Self-Employment



Notes: These figures illustrate the yearly inflows to self-employment in Spain, in both absolute (left-hand side) and relative (right-hand side) terms. The sample consists of all individuals who are 18 years of age or older. We distinguish inflows of individuals from the relevant states: Out of Labor Force (OL), Employment (E), and Unemployment (U). See Figures A.7 to A.12 for a representation of inflows to and outflows from other statuses.
Source: Authors' calculations are based on MCVL 2005-2018 data.

self-employment for the inflows into employment (Figure A.9) appears not to change much over time. This is also true for the outflows from employment to self-employment (Figure A.10), but is different to the patterns observed when analyzing the outflows from unemployment.

Figure A.12 illustrates that the share of individuals who transition from unemployment to self-employment remains relatively stable during the years surrounding the reform, even though they are relatively larger than at the beginning of the sample period. However, the outflows from unemployment are clearly dominated by employment, especially during the years 2012 and 2013. Moreover, Figure A.11 shows a similar pattern regarding the inflows into unemployment. The relative destruction of employment increases until 2013, when the economic recovery changes the trend. The inflow into unemployment from employment starts to decline thereafter.

Self-Employment Characteristics. Table B.1 compares the main characteristics of employed versus self-employed individuals. Regarding their socioeconomic features, we observe a gender gap in the group of self-employed individuals: while 47% of employed individuals are female, only 35% of self-employed individuals are women. The average age of the self-employed individuals (44 years) is higher than the average age of employed individuals (37 years). Moreover, the distribution of education levels differs to a certain extent: for example, the share of highly educated workers is larger for employed (15%) than for self-employed individuals (13%). This might be due to the fact that the trade and agricultural sectors are more relevant for self-employment. Moreover, the share of migrant founders (15%) is smaller than the share of immigrants among employees (26%).

Additionally, Figure A.13 illustrates the composition of self-employment with regard to the sector in which the business has been started. Our findings indicate that self-employment is important in the construction sector. The share of founders in that sector increases until 2008, when it begins to decrease in favor of other sectors like trade (retail and tourism), education, health, social, auxiliary, information, communication, insurance, and scientific services.[16]

Earnings. Figure A.14 compares the evolution of average annual real earnings from tax and social security data.[17] Both move parallel to one another: average earnings increased until 2009 but declined during the crisis period. They have only started to recover since 2014 but are still below pre-crisis levels at around 21,000 euros. The evolution of earnings follows the previously described patterns of the unemployment rate. In this context Bonhomme and Hospido (2017) document that earnings inequality (between 2004 and 2010) also appears to have evolved in line with the evolution of unemployment rates using similar social security data. Figure A.16 shows that the distribution of average monthly earnings is skewed to the left with a large dispersion across top incomes. Thus, most citizens in Spain earn an income that is below the mean.

3 Institutional Framework and Reform

Spain provides social security protection which covers healthcare, professional care for illnesses or accidents, and benefits for (temporary) disability, maternity, paternity, death, retirement, and job loss (SEPE, 2019). In the following section we will only focus on benefits in case of job loss. For details on the institutional background, we refer to Appendix F.1.

3.1 Unemployment Benefits in Spain

Unemployment Insurance (UI) Benefits. To get UI benefits, an individual must be legally unemployed, 16-65 years old, must have contributed to social security for at least 360 days within the last six years, and the reason of unemployment must be involuntary dismissal (SEPE, 2019).

The monthly UI benefit amount is computed from the regulatory base, which is an approximation of the average labor income over the 180 days preceding the unemployment spell, multiplied by the replacement rate. For the first 180 days of UI benefit receipt, a replacement rate of 70% is applied. If the individual is entitled to more than 180 days of UI benefits, another replacement rate is valid from day 181 onward. This second rate corresponded to 60% in the period before the reform of July 2012 took place. The reform reduced the second replacement rate to 50% of the regulatory base. According to the SEPE (2019), the monthly UI benefit amount is subject to a floor of 80% of the Public Income Index - *Indicador Público de Renta de Efectos Múltiples* (IPREM)[18] - and a ceiling of 225% of the IPREM. It is increased by one sixth of the monthly benefit amount conditional on the number of dependent children. Details on the calculation of UI benefits can be inferred from Table B.2 in the Appendix B. Moreover, the bounds of UI (and of unemployment assistance) benefit amounts were kept constant between 2010 and 2016, when the IPREM was frozen. In other words, during the period of our analysis, all relevant social security

[16] According to the classification of the Bank of Spain (García & Román, 2019), the construction sector decreased in favor of transport, tourism and retail, but also professional, scientific, administrative, and auxiliary services.

[17] We compare average monthly and daily real earnings from tax and social security data in Figure A.15.

[18] The IPREM serves as a reference to calculate social security benefits and is revised on an annual basis. Since June 25 2004 the IPREM replaced the minimum wage which was previously used to calculate social benefit amounts.

benefit levels were kept nominally constant in Spain. An overview of the evolution of UI benefit levels is provided in Figure A.17 in Appendix A.

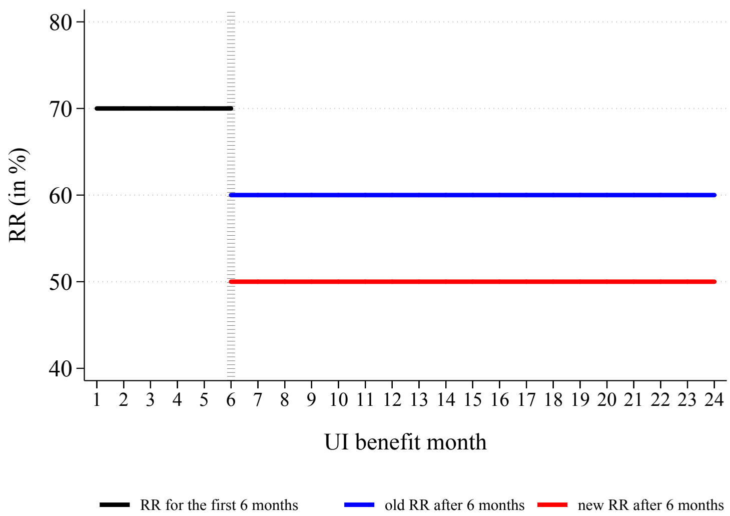
The duration of entitlement to UI benefits depends on the contribution period. Appendix Table B.3 shows that the Potential Benefit Duration (PBD) starts from a minimum of 120 days given a contribution period of at least 360 days. It increases gradually by 60 days conditional on the respective length of the contribution period. The maximum possible PBD is 720 days (SEPE, 2019). For more details on the Spanish UI system, we refer to Appendix F.2.

Unemployment Assistance (UA) Benefits. Under certain conditions registered job seekers are eligible for Unemployment Assistance (UA) benefits: the individual must be ineligible for UI benefits (or exhausted them) and the monthly gross income must be less than 75% of Spain's minimum wage. Additional information on the UA system is provided in Appendix F.3. Moreover, the evolution of the number of UI/UA beneficiaries is shown in Figure A.18.

3.2 Labor Market Reform in 2012

We focus on a Spanish labor market reform which was publicly announced on July 13, 2012.[19] On this day, Spain's vice president explained that all recipients entitled to more than 180 days of UI benefits who start their UI spell after July 14 2012 would experience a reduced Replacement Rate (RR) of 50% after their first 180 days of UI benefits receipt. Thus, this reform decreased UI benefits by approximately 16.67% in comparison to the previous RR of 60%. This new RR is marked by the red line in Figure 2. For all UI recipients who entered the UI system before July 15 2012 the old rate (blue line) remained valid from day 181 of the benefit period onward. As illustrated by the black line, the RR of 70% for the first 180 days of the UI PBD remained unchanged.

Figure 2: Replacement Rate before and after the Reform



Notes: This figure shows the drop in the Replacement Rate (RR) of Unemployment Insurance (UI) benefits before and after the reform.
Source: Authors' illustration of the reform.

[19] By virtue of the Royal Decree-Law 20/2012, this reform aimed to ensure budgetary stability and competitiveness.

Rebollo-Sanz and Rodríguez-Planas (2020) note that the reform's consequences for UI benefits became quickly known publicly as the new law received broad media attention. Nonetheless, a displaced worker's decision to claim benefits should not have been affected by the reform because for the first 180 days of benefit receipt the RR stayed the same. As the benefit cut kicks in 180 days after the UI spell entry, we can investigate individuals' responses in job search behavior before and after the actual drop in the net RR takes place.

According to Rebollo-Sanz and Rodríguez-Planas (2020), strategic lay-offs caused by the new law have been fairly improbable because the reform had already been implemented two days after its announcement. They also show that trends of monthly inflows into the UI system were similar during 2011 and 2012. As we discuss in Section 4.2, our analysis confirms that strategic manipulation around the reform cutoff date is not an issue, and thus the reform can be exploited as a quasi-experiment. Since Spain's unemployment rate reached its zenith of 26.1% in 2013 (OECD, 2018), the implementation of this reform affected a large portion of the Spanish labor force and was fairly unexpected in times when the economy was unlikely to improve for many months to come. Consequently, it cannot be argued that its implementation was endogenous to an anticipated recovery of the economy (Rebollo-Sanz & Rodríguez-Planas, 2020).

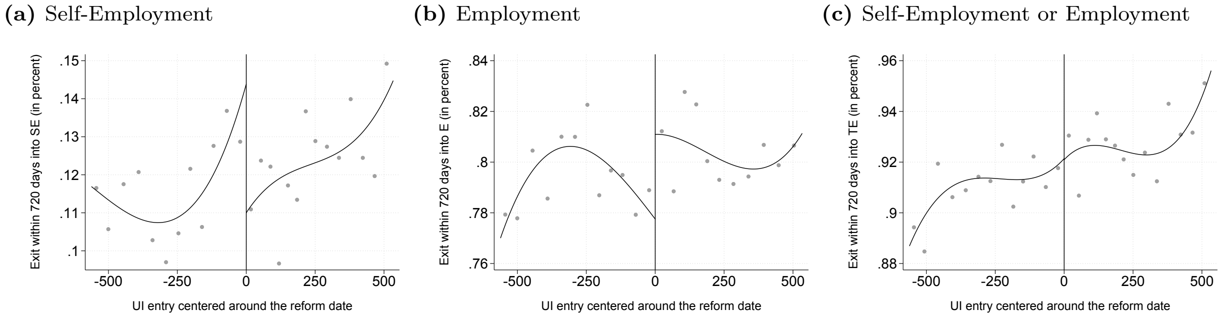
Besides the reduction in the RR, the reform also changed labor market rules for part-time workers and workers older than 52 years of age. Moreover, reforms adopted in 2013 had the goal of promoting self-employment among young workers. In Appendix F.6.3, we show that these self-employment reforms do not influence our results. A detailed overview of all reforms is given in Appendix F.6.

4 Empirical Strategy

The aim of this paper is to investigate the heterogeneous treatment effects of (reducing) UI benefit levels on (self-)employment in Spain. A decrease in UI benefit levels was implemented as part of Spain's 2012 labor market reform. The new law lowered the RR after the first 180 days of an individual's UI benefit spell by about 16.67% (cf. Section 3.2). Since only the individuals entitled to more than 180 days of UI benefits who entered their benefit spell after July 14 2012 are affected by the reform, we can exploit this quasi-experimental set-up to identify causal reform effects using an RDD. Our estimation sample consists of UI recipients who were displaced from a full-time job.[20] We follow each individual until he or she chooses to accept a job, becomes self-employed or until the end of 2018 in case he or she remains unemployed (or out of the labor force). For individuals who become self-employed, the counterfactual outcome would be to find a job or to stay unemployed. For individuals who become employed, the counterfactual would be to become self-employed or to stay unemployed. Our sample includes the whole set of possible exit states from unemployment. Thus, we can avoid the potential bias that emerges through ignoring self-employment and only focusing on employment.

[20]We exclude individuals who were self-employed right before they received cease-of-activity benefits (analogous to UI benefits) because their eligibility rules deviate from the UI eligibility criteria of regularly employed individuals (compare Appendix F.4). This does not, however, necessarily mean that individuals in our sample have no self-employment experience. It could be the case that they have previously been self-employed at an earlier stage of their employment history.

Figure 3: Reform Effects on the Extensive Margin from the Raw Data



Notes: These figures illustrate the reform effect on the probability of exiting unemployment into self-employment, employment, or either of them within the first 720 days of the UI spell from the raw data. We apply the IMSE-optimal number of quantile-spaced bins using a cubic polynomial (linear and quadratic versions are presented in Figure C.1). Our sample includes individuals who are 25-52 years old, entitled to more than 180 days of UI benefits, and who entered their UI benefit spell between January 1 2011 and December 31 2013, after having been laid-off from a full-time employment spell in a private firm (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

In Figure 3 we plot the effect of the UI benefit cut in 2012 on the probability of exiting from unemployment into (a) self-employment, (b) employment or (c) either of them (the union of both exit states) within the first 720 days of the unemployment spell from the raw data using a cubic polynomial. As suggested by Cattaneo, Idrobo, and Titiunik (2019), we apply the integrated mean squared error (IMSE) optimal number of quantile-spaced bins.[21] We find evidence of a negative reform effect on the startup rate and a positive reform effect on the job-finding rate, regardless of the polynomial order used (compare Figure C.1). It is worth noting that the scale is different for each exit state because more individuals transition into employment rather than self-employment. The effect on the union of self-employment and employment appears to be rather small if we use a quadratic polynomial (in Figure C.1) and vanishes if we use a cubic one. Consequently, using different functional forms to verify robustness of our results seems highly relevant. Overall, the raw data imply that the reform effects on (self-)employment may point in different directions.

Our plots only depict the overall effects within the first 720 days of unemployment. The results of Rebollo-Sanz and Rodríguez-Planas (2020) suggest heterogeneous treatment effects depending on the length of the actual unemployment spell duration. In their RDD set-up they find that the cut in UI benefits increases the job-finding rate on average by 26%, but only in the short term before the actual RR drop takes place (anticipation effect). We expect that the cut in UI benefits may not only affect employment in a heterogeneous manner, but also self-employment.

4.1 Methodology

Being affected by the reform is a deterministic and discontinuous function of time. Our RDD approach exploits the sharp treatment discontinuity introduced by the reform. When taking only individuals entitled to more than 180 days of UI benefits into consideration, those who enter their UI benefit spell after July 14 2012 are directly affected by the benefit cut (treatment group), whereas

[21]Bins contain approximately the same number of observations but their length may differ (Cattaneo et al., 2019).

those who still entered into UI before that date represent a valid counterfactual (control group). If nothing changed around the cutoff other than the treatment discontinuity induced by the reform, this setting allows us to identify the causal reform effect. Thus, identification relies on individuals' UI entry being a smooth function around the cutoff date which cannot be *precisely* manipulated.

Estimation sample. We restrict our sample to individuals entitled to more than 180 days[22] of UI benefits who entered their benefit spell between January 1 2011 and December 31 2013 after being laid-off from a full-time employment spell in a private[23] firm. We exclude individuals who contributed to a social security scheme[24] different from the general scheme right before they became unemployed, as well as disabled persons. Moreover, we restrict our sample to individuals between the ages of 25 and 52. As the new law from 2012 also changed labor market rules for workers older than 52 years of age, this seems to be a reasonable maximum age restriction to avoid bias from other parts of the reform. We exclude individuals not affected by the reform because their benefits either hit the ceiling or the floor of the UI benefit amount both before and after the RR drop (see Section 3.1 and Appendix Table B.2 for more details). In line with Fernandez-Navia (2020) we exclude UI benefit spells for which individuals potentially use the *option right*.[25] In case of multiple UI entries of the same person within our time period of interest, we keep one random observation. We drop a handful of individuals who leave the sample (due to death or emigration) to avoid potential sample selection bias (Fernandez-Navia, 2020).

Appendix Table C.1 shows mean values and standard deviations of pre-displacement characteristics of individuals in our RDD sample. We distinguish between individuals who exit from unemployment into self-employment (4,132) and employment (27,630) within the first 720 days of their unemployment spell. The total sample column additionally includes individuals who stay unemployed or whose exit states are censored (2,819). Similar to the comparison regarding our descriptive analysis (Table B.1), the share of women and immigrants is higher for individuals who exit into employment as compared to those who exit into self-employment. The characteristics of self-employed and employed workers differ in our RDD sample regarding the presence of children, education, skill level, earnings, PBD, and other variables. Individuals who exit into self-employment tend to be more educated and have worked in an occupation with a higher skill level in comparison to those who exit into employment.[26] The self-employed workers earn, on average, only slightly higher monthly real incomes and are entitled to only slightly more PBD.[27] In general, differences between both groups are less severe as compared to the sample that we used for our descriptive analysis (Table B.1), which is due to our sample restrictions that make treatment and control groups comparable.

[22]As the RR drop kicks in after the first 180 days of benefit receipt, individuals entitled to a maximum of 180 days of UI benefits are not affected by the reform.

[23]Shortly after the reform's implementation, we find an increase in dismissals of public firm workers in the data. We also find evidence of imbalanced covariates if we include public firm workers, which is why we decided to exclude them from our sample. In fact, austerity policy led to a decline in public sector workers in 2012 (see Appendix F.5).

[24]In addition to the *general scheme*, special schemes also exist for sea workers, etc. (see also Appendix F.1).

[25]If individuals use the *option right*, we cannot be sure whether they use up their old entitlement based on the rules from the pre-reform period or entitlements based on the rules valid after the cutoff date (see also Appendix F.2).

[26]This might be due to the exclusion of individuals who contributed to special social security schemes (i.e. marine scheme, agricultural scheme, etc.) who are characterized by lower education and skill level. Since these schemes are particularly important for the self-employed, this group experiences the largest changes.

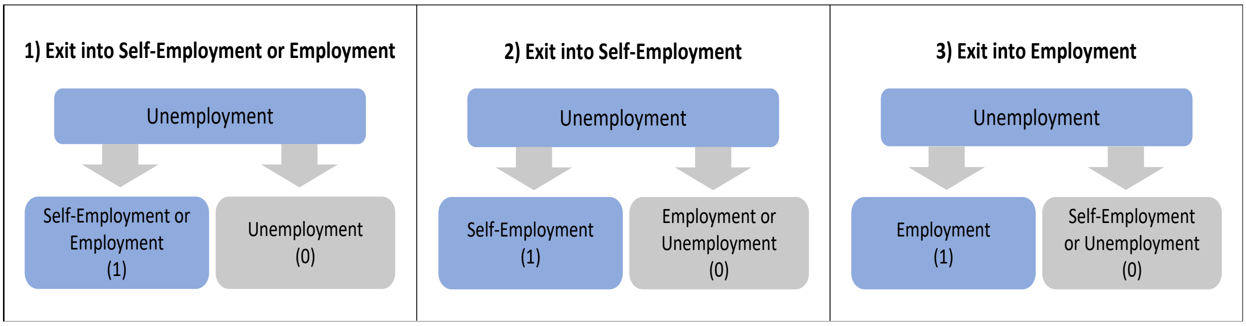
[27]This might be due to the exclusion of individuals entitled to no more than 180 days of UI benefits.

Estimation equation. We employ a non-parametric local polynomial estimation framework using a triangular kernel function, different polynomials as a sensitivity check, and a bandwidth that optimizes the mean squared error (MSE) as recommended by Cattaneo et al. (2019). Standard errors are clustered at UI entry date level to account for potential correlation in unobservable characteristics (Lee & Card, 2008). Our estimation equation can be illustrated as follows:

$$Y_i = \alpha + \beta \cdot \mathbf{1}(t_i \geq 0) + \delta(t_i) + \theta X_i + \epsilon_i \tag{1}$$

We use three different sets of outcome variables, Y_i. With our first set of outcome variables we intend to measure extensive margin effects. In this case, our dependent variable Y_i is a binary outcome which takes the value of one if individual i exits from unemployment into the state of interest (self-employment, employment or the union of both as shown in Figure 4, where each exit state of interest is highlighted in blue) within the first 90, 180, 360, and 720 days of being unemployed. It takes the value zero if the individual remains unemployed or exits into the counterfactual state (Figure 4, highlighted in gray). This measure can also be interpreted as a cumulative hazard rate because the probability of exiting from unemployment into the state of interest is accumulated over time.[28] Summary statistics of our extensive margin outcomes are shown in Appendix Table C.2.

Figure 4: Illustration of Extensive Margin Outcome Variables



Notes: Alongside UI spells, unemployment also includes UA spells and unregistered spells which essentially means the individual is unemployed without receiving any kind of benefits (out of labor force).
Source: Authors' own illustration.

The second set of outcome variables measures the unemployment spell duration in months such that we can compute duration elasticities. We distinguish between the general Unemployment (UE) spell duration (including UI, UA, and unemployment spells without benefit receipt) and UI spell duration (excluding periods without UI benefit receipt). We run unemployment duration regressions for different subsamples: for individuals who transition from unemployment into self-employment, employment, and the union of both within the first 360 and 720 days of their unemployment spell. We then calculate distinct duration elasticities for each of these subsamples, i.e. we divide the percentage change in UI or UE duration (relative to the pre-reform average duration, as illustrated

[28]Example: If individual i exits into self-employment within the first 90 days of unemployment, the same individual also exits within 180, 360, etc. days into self-employment. For this particular individual the *self-employment* (and *self-employment or employment*) outcome variables will always take the value one and the *employment* outcome variables will always take the value zero.

in Table C.17) by the percentage change in the RR due to the reform (approximately 16.67%):

$$\eta = \frac{\textit{\% change in UI or UE duration}}{\textit{\% change in RR}} \tag{2}$$

Our third set of outcome variables consists of quality measures regarding the unemployment exit states to assess the reform's potential welfare implications. Namely, (self-)employment spell duration[29] (in months), logarithm of the real average social security contribution basis[30], a dummy variable indicating whether the individual earned an income above the median[31] before he or she became unemployed, and eight sector dummy variables[32]. Regarding the employment quality measures we additionally include a permanent contract dummy. We take potentially heterogeneous reform effects on (self-)employment quality into account by restricting our sample to individuals who transition into a (self-)employment spell within the first 360 and 720 days of unemployment.

Our running variable, t_i, is the UI entry date of individual i, normalized to zero at the cutoff date (July 15 2012). The treatment dummy variable is represented by the indicator function $\mathbf{1}(t_i \geq 0)$ which equals one if individual i enters the UI benefit spell after July 14 2012 ($t_i \geq 0$) and zero if the individual enters before that date ($t_i < 0$). We control for the smooth relationship between the running variable and Y_i using the function $\delta(.)$ which allows a different slope before and after the reform cutoff date. The effect of the running variable on the outcome variable may therefore be different before the cutoff than after the cutoff date. We use a linear, quadratic, and a cubic spline to test sensitivity of results. By adding different sets of predetermined covariates, X_{ij}, this enables us to investigate the sensitivity of our results. If our point estimates change considerably due to the inclusion of additional covariates, the identification assumption might be violated.

Our predetermined covariates are measured at an individual's UI spell entry and include socio-economic, pre-displacement job, and unemployment characteristics. The socio-economic characteristics refer to a female dummy, age and age squared (in years), educational level dummies (lower, medium, and higher education), a dummy for the presence of children in the household, and an immigrant[33] dummy. The pre-displacement job characteristics refer to an individual's employment

[29] We observe individuals' spells until the end of 2018. Consequently, individuals who switch into an UI spell by the end of 2013 can be observed until a maximum of five years. We guarantee that pre- and post-reform period spells potentially have the same duration maximum by artificially right-censoring exit states' duration after five years.

[30] The contribution basis corresponds to the real earnings with regard to individuals who exit into employment. Unfortunately, we have no information on self-employment income, but we use the contribution basis as best available proxy. Self-employed individuals must choose a contribution base within existing legal bounds which are legally determined each year. The minimum and maximum basis from which the self-employed can choose depends on personal and occupational characteristics. Starting from the legal minimum contribution base, they have to pay a higher percentage of their income as social security contributions if they choose a higher protection level. We can only approximately infer the income of self-employed individuals.

[31] Median monthly real wage from social security data: 1,471.63 euros. We define workers being of high quality if they received a pre-unemployment monthly real wage above the median. If the probability that individuals who become (self-)employed are high quality workers increases due to the reform, this may indicate an increase in the (self-)employment quality.

[32] Sector 1: Agriculture, extraction, primary manufacturing; Sector 2: Manufacturing and utilities; Sector 3: Construction; Sector 4: Trade; Sector 5: Transport and storage; Sector 6: Accommodation and food services; Sector 7: Information, communication, finance, insurance, real estate, and scientific services; Sector 8: Education, health, social, auxiliary, and other services.

[33] We define an immigrant as a person with a different birth country than that of Spain. Our results are robust to

experience (in months), self-employment experience dummy, logarithm of real monthly average earnings, permanent contract dummy, eight sector dummy variables, and occupational skill level (high, medium, low skilled). Ultimately, unemployment characteristics include the PBD (in months) and the quarterly unemployment rate of the province the individual lived in during UI entry. Summary statistics of pre- and post-reform period means are presented in Appendix Table C.3. The variables' detailed definitions can be inferred from Appendix E.3.

In the following sections we focus on the estimated treatment effect $\hat{\beta}$. As our estimation technique relies on a local approach, we estimate the *Local Average Treatment Effect (LATE)* of the cut in UI levels for workers who switch into an UI spell in the vicinity of the cutoff date. Due to limited space we only show the main results in the text. More detailed results are presented in Appendix C.

4.2 Identification

As assignment into the treatment group is solely determined by each individual's UI entry date, identification of the causal LATE hinges on the assumption that individuals cannot *precisely* manipulate this date. In other words, the running variable must be continuous around the cutoff. Given that the reform was already implemented two days after being announced, it seems plausible that this assumption holds. Additionally, an individual is only entitled to receive UI benefits if the reason of dismissal is involuntarily and the employer has to inform the worker about the dismissal two weeks in advance - facts which limit the possibility of *precise* manipulation tremendously. Appendix Figure C.2 shows the histogram of our running variable. It plots the number of UI entrants at each date, centered around the reform cutoff. In line with the findings of Fernandez-Navia (2020), our descriptive evidence shows that most UI entrants systematically occur at the beginning of each month due to administrative reasons. Nonetheless, there is no suspicious peak or drop close to the cutoff, such that we find no visual evidence for *precise* manipulation.

Even though logical reasoning and visual inspection speak in favor of our identification assumption, we also test its validity empirically. As suggested by Cattaneo et al. (2018), a non-parametric local polynomial approach should be used to estimate the density of the running variable below and above the cutoff, respectively. According to Cattaneo et al. (2019) this sort of manipulation test has better power properties than other manipulation tests and does not require pre-binning of the data. Figure 5a plots the resulting density of the running variable and its 95% confidence intervals.[34] On both sides of the cutoff the confidence intervals clearly overlap, indicating continuity of the running variable around the cutoff. We estimate a t-statistic of 0.2657 with a p-value of 0.7905 which confirms the visual impression. Additionally, we run a more typical density test based on McCrary (2008) to verify continuity around the cutoff.[35] We plot the estimated density in Figure 5b using a bin size of three (results remain robust if different bin sizes are used). According to the estimated test statistic of -0.0032 with a standard error of 0.0417, the null hypothesis of continuity around the cutoff cannot be rejected which, again, speaks in favor of our identification assumption.

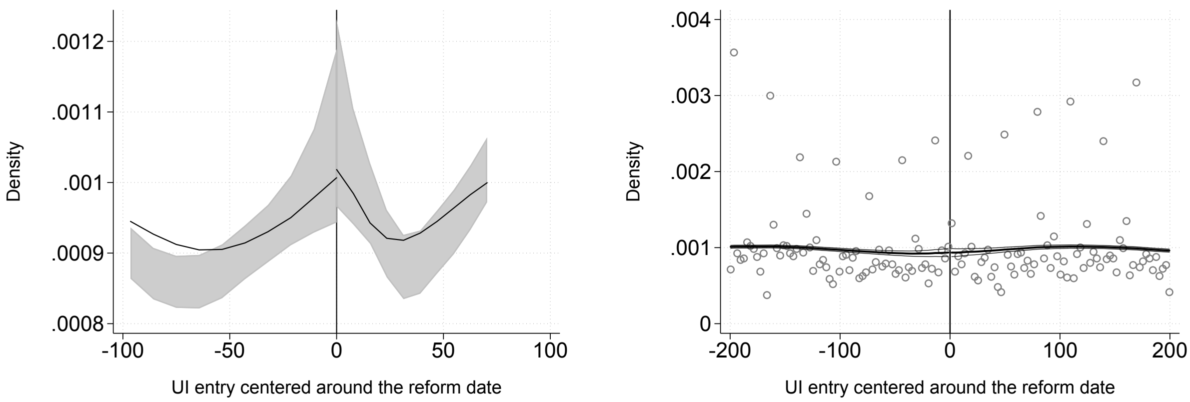
the inclusion of an immigrant variable defined by a person's nationality.

[34] We use the `rddensity` routine in Stata to run the RD manipulation test (Cattaneo et al., 2018).

[35] We use the `DCdensity` routine in Stata to run the McCrary test.

Figure 5: Continuity of the Running Variable

(a) RD Manipulation Test **(b)** McCrary Test



Notes: Figure (a) depicts the density of the running variable and its 95% confidence intervals using non-parametric local polynomial density estimation as suggested by Cattaneo, Jansson, and Ma (2018). We estimate a t-statistic of 0.2657 with a p-value of 0.7905. Figure (b) plots the density of the running variable based on the approach suggested by McCrary (2008). Using a bin size of three and the default bandwidth calculation (bandwidth = 170) we estimate a log difference in height of -0.0032 (0.0417) with standard errors in parentheses. According to both tests, the null hypothesis of a continuous running variable cannot be rejected, which is evidence in favor of our identification assumption. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Validity of our approach is not guaranteed through continuity around the cutoff of the running variable. We require that nothing else changes except for the treatment assignment (and potentially our outcome variables). More precisely, it is necessary that any other determinant of our outcome variables correlated with the running variable is continuous in the vicinity of the cutoff. Thanks to the RDD we can directly test the balancing assumption of our covariates by estimating equation 1 and putting each of the covariates on the left-hand side.

Appendix C.3 shows the estimated reform effects on the covariates and their corresponding balancing plots in detail. We estimate a linear and a quadratic version of the running variable and include the remaining covariates on the right-hand side.[36] Regardless of the functional form used, most of the estimated coefficients are close to zero and insignificant. There are only two exceptions. The reform effect on the immigrant dummy variable is estimated to be significantly different from zero at the 5% level in both the linear and the quadratic set up. However, the estimates we present in the next section remain robust if we exclude immigrants. Hence, this slight imbalance does not affect our results.[37] Another exception is the low-skilled occupation dummy variable which seems to be significantly positively affected by the reform but only at the 10% significance level in the quadratic set-up. The remaining 22 covariates are perfectly balanced which may also be inferred from the balancing plots (Appendix Figures C.3 to C.6). Overall, manipulation and balancing tests support the validity of our identification assumption.

[36] The results remain robust if the basic RDD set-up without covariates is used.

[37] We decided to include immigrants in our main set-up because they make up an important share (16.5%) of self-employed individuals (Appendix C.1).

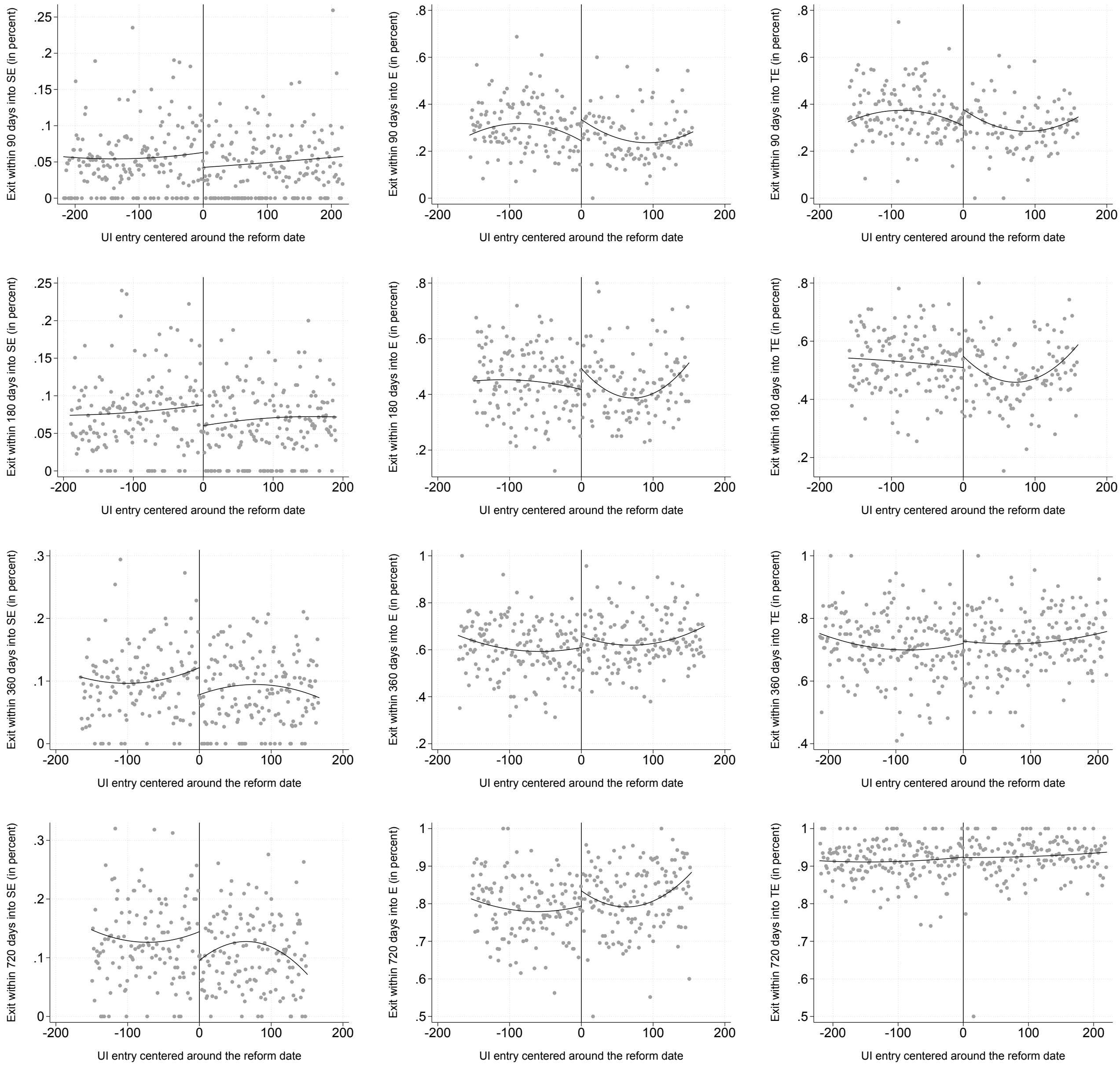
5 Results

5.1 Reform Effects on the Extensive Margin

Our baseline results from local quadratic regressions without covariates are visualized in Figure 6. It plots the (discontinuous) average exit probabilities before and after the cutoff date. The subfigures

Figure 6: RD Plots by UI Exit State (Quadratic)

(a) Self-Employment **(b)** Employment **(c)** Self-Employment or Employment



Notes: These figures illustrate the estimated quadratic reform effect on different UI exit states without covariates using MSE-optimal bandwidths as suggested by Calonico, Cattaneo, and Titiunik (2014). We use the `rdrobust` routine in Stata to select the MSE-optimal bandwidth and the `rdplot` routine to generate the graphs. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). See Figures C.7 to C.8 for the analogous linear and cubic specifications.
Source: Authors' calculations are based on MCVL 2005-2018 data.

depict the effects on the average probability of exiting from unemployment into (a) self-employment, (b) employment, and (c) self-employment or employment. We take potential heterogeneity into account by plotting the effects within different time periods of the unemployment spells. The first row corresponds to exit probabilities within the first 90 days of unemployment, the second row to the exit probabilities within the first 180 days, the third row to those within 360 days, and the last row to the exit probabilities within 720 days of unemployment duration. Thus, we distinguish between short-term (90 or 180 days), medium-term (360 days), and long-term effects (720 days). Moreover, the cumulative unemployment exit probability increases if a longer period of time is taken into consideration. Therefore, the scale of the y-axis increases if the time horizon is extended.

The figure shows that the reform effect on the startup rate is consistently negative and is rather small on a short-term basis but it intensifies over time. By contrast, the job-finding rate is consistently positively affected. The effect is stronger in the short term and decreases over time. The aggregate effect on the probability of exiting into self-employment or employment is slightly positive in the short term but vanishes in the medium and long term. Regardless of the exit state considered, the effect size is very similar if we use a cubic polynomial (see Figure C.8; for the sake of completeness, linear plots are illustrated in Figure C.7). As almost none of the fitted lines appears to be linear, the figures suggest a second or higher order relationship between the running and outcome variable. Therefore, we focus on higher order relationships in the subsequent section.

Overall, we can confirm the visual findings of the raw data when conducting our RDD regression (compare Section 4.1). We find a negative effect on the self-employment probability and a positive effect on the employment probability which cancel out each other if the union of both exit states is taken as an aggregate exit probability. However, the effect intensity varies over time, supporting our presumption of heterogeneous treatment effects.

Table 1 shows our significantly estimated reform effects on the probability of exiting from unemployment into self-employment (SE, panel A) in the medium term and on the probability of exiting into re-employment (E, panel B) in the short term. Panel C refers to the estimated reform effect on the probability of exiting into self-employment or employment (SE or E) in the short term. Alongside point estimates and p-values we indicate the estimated average change in the outcome variable due to the reform relative to its pre-reform average outcome. We also indicate the polynomial of the running variable, whether covariates are added or not, the selected MSE-optimal bandwidth, and the effective number of observations used to the left and the right of the cutoff. Appendix Tables C.4-C.8 contain extensive versions of our results.

Our results show that the lower UI benefits negatively affect the probability of becoming self-employed within the first 360 days of unemployment. The point estimate is significant at the 5% level if we use a local quadratic regression without covariates (panel A, first row). The significance level is only slightly reduced if covariates are added (panel A, second row). The UI benefit cut decreases the self-employment probability in the medium term, on average, by 3.5 percentage points (p.p.). Given a pre-reform self-employment mean probability of 9.6% (Table C.2), this corresponds to a decrease of 36.5%. In the cubic setting the decrease corresponds to 45.8% and is significant at

Table 1: Reform Effects on the Extensive Margin

Outcome Variable	RD Estimate	Rel. Change	s.e.	p-value	Polynom.	Covs.	Bandwidth	N Left	N Right
(A) SE within 360 days	-0.043	-44.8%	0.022	0.028	quadratic		166.751	5,676	5,758
	-0.035	-36.5%	0.021	0.060	quadratic	✓	178.160	5,863	6,171
	-0.048	-50%	0.023	0.018	cubic		264.475	9,105	9,125
	-0.044	-45.8%	0.023	0.030	cubic	✓	239.670	7,973	8,172
(B) E within 180 days	0.076	16.6%	0.042	0.033	quadratic		150.063	4,926	5,244
	0.080	17.5%	0.041	0.027	quadratic	✓	150.738	4,813	5,109
	0.085	18.6%	0.044	0.025	cubic		235.693	8,071	8,292
	0.086	18.8%	0.044	0.024	cubic	✓	234.536	7,829	8,057
(C) SE or E within 90 days	0.070	20.1%	0.043	0.056	quadratic		160.199	5,252	5,626
	0.076	21.8%	0.042	0.039	quadratic	✓	156.880	5,008	5,296
	0.070	20.1%	0.042	0.069	cubic		289.288	10,045	9,718
	0.074	21.2%	0.042	0.057	cubic	✓	284.684	9,427	9,388

Notes: Outcome variables are binary and indicate whether the person transitioned into an (self-)employment spell within the first 90, 180 or 360 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. Relative changes are calculated based on the pre-reform average exit probabilities illustrated in Appendix Table C.2. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). Detailed results for each outcome are provided in Tables C.6 to C.8.
Source: Authors' calculations are based on MCVL 2005-2018 data.

the 5% level even when covariates are added. Regardless of the polynomial order, estimates vary little if we add covariates. Overall, the quadratic and cubic settings of panel A suggest that lower UI benefits reduce the self-employment probability within 360 days of unemployment by 35-50%.

The more extensive results of the estimated reform effects on the self-employment probability in Table C.6 reveal that all point estimates are negative and increase over time in absolute terms. Within the first 90 days of the unemployment spell, the reform effect is small and insignificant (between -2.2 and -1.9 p.p.). It turns significant at the 10% level with a stronger magnitude (between -3.6 and -2.2 p.p.) if we consider the 180 days period. This result is evidence of an anticipation effect. Even if the actual RR drop takes place after 180 days of UI benefit receipt, individuals adjust their behavior from the start of their UI spells. However, the effects on the self-employment probability are stronger (between -3.5 and -5.8 p.p. for the quadratic and cubic polynomials) in the medium and long term. Altogether, our results indicate a negative anticipation effect on the self-employment probability in the short term and an even stronger negative effect on the startup rate in the medium and long term.

Conversely, we estimate a consistently positive reform effect on the job-finding rate within the first 180 days of unemployment, which is significant at the 5% level in all specifications of panel B. The point estimates indicate an average increase between 7.6 and 8.6 p.p., corresponding to a relative increase between 17% and 19% in the short term. In contrast to the reform effect on the self-employment rate, the effect on the job-finding rate is stronger in the short term than in the medium (7-9%) or long term (5-6%). Appendix Table C.7 shows that these positive effects decrease and turn insignificant if we follow the unemployment spell over a longer time period. In Appendix C.6 we check the sensitivity of our estimates to the exclusion of individuals who exit into self-employment. We show that the estimated reform effect on the very short-term job-finding rate (within the first 90 days of unemployment) is slightly upward biased if self-employment is excluded.

From panel C we infer that the stronger positive anticipation effect on employment surpasses the negative anticipation effect on self-employment, such that the probability of exiting into either of these states is, on average, positively affected in the short term. We estimate a relative increase in the probability of exiting into self-employment or employment of about 20-22% due to the reform. However, this positive effect is only significantly different from zero within the first 90 days of the unemployment spell. Table C.8 shows that the point estimates are halved and turn insignificant if we take the first 180 days of unemployment into consideration. We find insignificant zero effects in the medium and long term. As previously suggested by the raw data in Figure 3, the negative effect on the startup rate and the positive effect on the job-finding rate cancel out each other over time.

Overall, our results are robust to the inclusion of covariates and different polynomials. We can confirm heterogeneous treatment effects on the extensive margin. The effect on the self-employment probability is negative and increases in the medium and long term, whereas the effect on the employment probability is positive and declines throughout the unemployment spell. The probability of exiting into self-employment or employment is positively affected but only within the first 90 days of unemployment. We cannot confirm any medium or long-term effects if we do not differentiate between self-employment and employment (i.e. *general employment*). However, we do find short-term anticipation effects for all exit states. Over different time horizons, the negative effect on self-employment (35-50%) is consistently stronger than the positive effect on employment (5-33%). Our findings suggest that the inclusion of data on self-employment is extremely important to evaluate the reform effect on the probability of exiting unemployment in general. Under heterogeneous treatment effects it is flawed to assume that the positive effect on the job-finding rate represents the reform's *general employment* effect (on both self-employment and employment). It is worth noting that we demonstrate these results more elaborately through the exclusion of self-employment in Appendix C.6. In conclusion, the isolated focus on the job-finding rate does not tell the full story, as the reform's negative effect on the startup rate is not considered.

5.1.1 Placebo Tests

As a robustness check, we analyze our extensive margin results using placebo tests. Appendix Table C.9 shows the results if we use a placebo treatment group of individuals whose RR did not drop after 180 days of UI receipt because they either hit the ceiling or the floor of the UI benefit amount (Appendix Table B.2 shows the minimum/maximum of UI benefits). For this placebo group, we find very few individuals who exit their unemployment spell within 90 or 180 days and therefore can only investigate the effects on the medium and long-term exit probabilities. Regardless of the polynomial order or whether we include control variables or not, we find insignificant placebo reform effects.[38] Consequently, exit state probabilities of our placebo group are not affected, which suggests that our estimated reform effects from Section 5.1 reflect true causal effects and are robust.

[38]The only exception are a handful of significant effects regarding the probability of exiting into self-employment or employment within the first 720 days of the unemployment spell.

Another placebo test we conduct is to artificially change the reform cutoff date to check the robustness of our results and show that they are indeed driven by the reform cutoff date. We use a fictive reform cutoff date one year after the actual reform took place (July 15 2013) to test whether the estimated reform effects are indeed driven by the actual reform and not by other factors such as seasonal effects. We drop observations before the actual cutoff date (July 15 2012) to avoid bias from the true reform effect. Results are presented in Appendix Tables C.10-C.12. We find no evidence of a season driven reform effect, since almost all estimated placebo effects are insignificant.[39] To conclude, placebo tests confirm the robustness of our main results.

5.1.2 Robustness Check - Competing Risks Model

It might be of some concern that our estimation results could potentially be biased, since our local polynomial regression framework does not take the duration structure of the data itself into account. A Competing Risks Regression (CRR) takes care of this issue and therefore could be the more suitable model candidate. In Appendix D we demonstrate that our results do not considerably change if we use a CRR instead, which is why we prefer the more parsimonious model of Section 4.1.

5.1.3 Subgroup Analysis

Besides significant reform effects on the extensive margin of (self-)employment which vary over time, different groups of unemployed individuals might also be heterogeneously affected by the reform. In the following, we conduct a subgroup analysis to investigate which groups are mostly affected. We divide our sample by age (below vs. above median age), gender, contract type (permanent vs. temporary), presence of children in the household, immigration status, education (lower, medium, higher education), and monthly average real earnings (below vs. above logarithm of median wage).[40]

Appendix Table C.15 shows the estimated reform effects on the self-employment probability in the medium term (within 360 days of entering unemployment) when results are divided by subgroups. In general, all point estimates are negative. Reform effects are very similar when it comes to different contract types. We find that, on average, younger individuals, women, those with children, and immigrants experience a significantly stronger drop in their medium-term startup rate when UI benefits decrease. It is not surprising that these vulnerable subgroups are more sensitive to benefit cuts: young people often face liquidity constraints[41] (Alba-Ramirez, 1994) and it is often more difficult for women to successfully compete with male entrepreneurs (depending on the sector of activity). Being a woman is also highly correlated with having children which reduces the probability of becoming self-employed according to our results, perhaps because having children limits entrepreneurial flexibility and increases risk aversion. Immigrants may not only face discrimination but may also be less informed on regulations and procedures which are necessary when it comes to starting up a business. Besides, they may have smaller networks, and therefore

[39] There are only two exceptions: we find significant placebo reform effects for the short-term probability of exiting into self-employment and the medium-term probability of exiting into self-employment or employment. However, we find no significant effects on these outcomes in our actual reform results which is why we take these less seriously.

[40] We used the following median values: median(age)= 36 and the median ln(real monthly average wage)= 7.3.

[41] In fact, this is one of the main reasons why Spain introduced the *Strategy of Entrepreneurship and Youth Employment* in August 2013. For more details on Spanish reforms, we refer to Appendix F.6.

face more obstacles compared to locals. According to González Menéndez and Cueto (2015) and Garcia-Cabo and Madera (2019) business survival rates of younger workers, women, and immigrants are lower than the ones of an average founder. Awareness of this fact might also reduce the motivation to become self-employed within this group, especially when a reduction of the planning period due to higher income pressure arising from a cut in UI benefits might increase the hurdles in starting a business. Moreover, our findings also show that individuals with a medium or higher educational degree and those with pre-displacement income above the median wage tend to be more negatively affected. These potentially relatively smaller subgroups of unemployed individuals most likely have better chances of finding a job, particularly in a crisis period, which could be the reason why their self-employment probability is more strongly reduced.

There is also substantial heterogeneity in the reform effects on the short-term job-finding rate (within 180 days of unemployment). Table C.16 shows that younger individuals experience a stronger positive effect on the job-finding rate, in line with the stronger reduction in their self-employment probability. As opposed to Rebollo-Sanz and Rodríguez-Planas (2020), we find that individuals with a pre-displacement temporary contract and those without children tend to be more positively affected. As this subgroup has fewer duties and a higher flexibility, their chances of finding a suitable job match might be higher. Additionally, men, locals, those educated at a medium level, and individuals with higher pre-displacement earnings drive the positive employment effect in the short term. These subgroups are less subject to prejudices, which may also relatively improve their job-finding opportunities. Our result that the male job-finding rate is more positively affected by the reform corresponds to the findings of Rebollo-Sanz and Rodríguez-Planas (2020).

Altogether, we find not only heterogeneity in the timing of reform effects but also with regard to the socio-economic status and pre-displacement job characteristics of the unemployed individuals affected by the cut in UI benefits.

5.2 Reform Effects on Unemployment Duration

Next, we analyze how UI benefits affect actual unemployment duration. As described in Section 4.1, we use actual UI and UE duration in months as outcome variables and estimate a local polynomial RDD for different subsamples of individuals who transition into self-employment, employment, or the union of both within the first 360 and 720 days of their unemployment spell. We then calculate the duration elasticity for each subsample (η_{SE}, η_E, and $\eta_{SE\ or\ E}$) as illustrated in equation 2.

Table 2 summarizes our duration elasticity results using the quadratic RDD estimation approach for transitions out of unemployment in the medium term (within 360 days of unemployment) and in the long term (within 720 days of unemployment). Panel A shows results using UI and panel B shows results using UE as an outcome variable to measure the reform's effect on actual unemployment duration. Our findings show that the UI duration elasticity for individuals who transition into self-employment is always negative and relatively large in absolute terms (between -0.2 and -1.5 in panel A1). Their UE duration elasticity in panel B1 is slightly smaller in absolute terms, though statistically not significant. Instead, the UI duration elasticity results for those who transition into re-employment (panel A2) are statistically significant and take values between 0.6 and 0.9.

Their UE duration elasticity estimates in panel B2 are slightly smaller yet still positive. Finally, as expected the duration elasticity estimates regarding the union of both exit states (panels A3 and B3) are in between the elasticities with respect to self-employment (A1/B1) and employment (A2/B2), yet always positive (0.3-0.6). Results for linear and cubic polynomials are depicted in Tables C.18 to C.19.

Our duration elasticity results complement the previous probabilities findings by pointing to the following. A reduction in UI benefit levels appears to reduce the actual UI benefit duration (as well as UE duration) of those transitioning from unemployment to re-employment. This is in line with the fact that we find an increase in the job-finding rate in response to the reduction of UI benefits levels (cf. Section 5.1). With increasing search intensity, unemployed individuals find regular employment more quickly, thus, their actual UI/UE duration declines (compare e.g. Marinescu

Table 2: UI and UE Duration Elasticities (360 vs. 720 Days) - Quadratic

Outcome Variable	Duration Elast. (η)	RD Est.	% change in duration	s.e.	p-value	Covs.	N left	N right	Max. days before exit
(A) UI Duration									
(A1) Self-Employment	-0.066	0.034	1.1	0.615	0.907		633	589	360
	-0.200	0.101	3.3	0.616	0.984	✓	584	557	360
	-1.249	1.047	20.8	1.174	0.375		1080	1049	720
	-1.480	1.241	24.7	1.168	0.288	✓	1016	1008	720
(A2) Employment	0.752	-0.491	-12.5	0.334	0.071		3451	3670	360
	0.772	-0.504	-12.9	0.308	0.052	✓	3384	3593	360
	0.830	-0.865	-13.8	0.543	0.063		4881	5269	720
	0.865	-0.901	-14.4	0.551	0.062	✓	5593	5897	720
(A3) Self-Employment or Employment	0.584	-0.370	-9.7	0.311	0.127		4070	4230	360
	0.641	-0.406	-10.7	0.296	0.094	✓	3915	4065	360
	0.528	-0.536	-8.8	0.551	0.239		5954	6252	720
	0.532	-0.541	-8.9	0.559	0.248	✓	6759	7064	720
(B) UE Duration									
(B1) Self-Employment	-0.278	0.154	4.6	0.673	0.975		626	585	360
	-0.475	0.264	7.9	0.665	0.817	✓	595	567	360
	-1.182	1.113	19.7	1.408	0.451		926	879	720
	-1.352	1.274	22.5	1.370	0.350	✓	915	877	720
(B2) Employment	0.783	-0.534	-13.1	0.349	0.061		3313	3641	360
	0.821	-0.559	-13.7	0.317	0.038	✓	3181	3511	360
	0.566	-0.644	-9.4	0.565	0.169		5474	5603	720
	0.584	-0.665	-9.7	0.565	0.163	✓	6073	6255	720
(B3) Self-Employment or Employment	0.615	-0.409	-10.2	0.329	0.111		4018	4186	360
	0.668	-0.444	-11.1	0.306	0.077	✓	3642	3988	360
	0.336	-0.373	-5.6	0.596	0.409		6398	6590	720
	0.358	-0.398	-6.0	0.596	0.397	✓	6818	7114	720

Notes: This table presents our estimated UI (panel A) and UE (panel B) duration regression results. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014), a quadratic specification and a triangular kernel. Standard errors are clustered at the UI entry date level. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) but restricted to individuals who exit into self-employment, employment, or either of them within the first 360 or 720 days of unemployment. The duration elasticity, η, is computed from the percentage change in UI or UE duration (relative to the pre-reform average duration, see Table C.17), divided by the percentage change in the RR due to the reform (approx. 16.67%), as illustrated in equation 2. Detailed results for linear and cubic polynomials are provided in Tables C.18 to C.19.
Source: Authors' calculations are based on MCVL 2005-2018 data.

& Skandalis, 2019). Thereby, in correspondence with Doris et al. (2020) the elasticity estimates regarding employment appear to be larger in absolute terms than common elasticity estimates that are usually based on evaluating more rather than less generosity of the UI system. Our findings confirm the asymmetric nature of the direction of UI generosity changes for UI duration elasticity results.

Moreover, our findings are among the first that provide elasticity estimates concerning self-employment. These estimates show that individuals starting up out of unemployment after a cut in benefits tend to remain longer unemployed (both in terms of UI and UE duration). Given our main results revealing that there is a decline in the probability of becoming self-employed, the results that suggest a negative UI (UE) duration elasticity could be interpreted in two ways. First, individuals who are not able to find a proper employment option may end up feeling pushed to become self-employed. As a result, longer actual unemployment duration may hint to a deterioration of the quality of new startups in response to the cut in benefit levels. In the following section, we explore this aspect in more detail by focusing on the welfare considerations for those transitioning into self-employment. Second, those with an *opportunity-driven* motivation for starting up a business might be less responsive to a change in unemployment benefits and simply take the reform as an opportunity to find, for instance, new employees more easily. In this context, our findings could be also explained through liquidity constraints, imposed by the cut in UI benefits which hit potential founders more than individuals who search for regular jobs, since those who want to set up a business might need more time to collect necessary funding.

5.3 Reform Effects on (Self-)Employment Quality

From a policy perspective, it is also relevant to understand whether the quality of (self-)employment has changed due to the reform alongside our extensive margin results of the previous sections. Therefore, we investigate whether the UI benefit level cut affects the composition of (self-)employment and other quality aspects of post-unemployment outcomes. Regarding self-employment, we propose that an increase in the share of *opportunity-driven* rather than *necessity-driven* entrepreneurs indicates a quality improvement. *Opportunity-driven* entrepreneurs are most likely better prepared and consider their business as a destiny rather than a last resort to escape unemployment, as opposed to *necessity-driven* entrepreneurs. Consequently, we can expect that if the composition of self-employment changes, its quality changes as well.

Starting with a descriptive analysis of the effects on self-employment quality, Appendix Figure C.9 shows that the composition of self-employment among different age groups before and after the reform has changed. Older individuals are less often self-employed than before, whereas the opposite can be observed with regard to the younger generation. According to Azoulay, Jones, and Miranda (2020) successful entrepreneurs tend to be middle-aged rather than young. An increasing share of young entrepreneurs may therefore indicate an increase in *necessity-driven* entrepreneurship, i.e. a decrease in startup quality.[42]

[42] However, this is only descriptive evidence and could be caused by other reforms in 2013 which particularly targeted young unemployed individuals to become self-employed. See Appendix F.6.2 and F.6.3 for more details.

Other descriptive evidence can be inferred from mean comparison tests in Appendix Table C.20. We compare two quality measures in the post- and pre-reform period to account for changes in self-employment quality: sector of activity indicators and the real average social security contribution basis as best available proxy for self-employment income. If self-employed individuals choose a higher contribution basis, this may be an indicator of *opportunity-driven* entrepreneurship because they tend to be in a better economic situation than *necessity* entrepreneurs. This enables them to pay higher social security contributions in order to get access to more social insurance. *Opportunity-driven* entrepreneurs may successfully work in any sector, particularly in those with higher growth potential. For instance, an increase in the information and communication (I&C), finance, real estate, and scientific services sector could be interpreted as an average increase of startups' quality. Instead, transitions to activities such as trade could reflect decreases in startup quality because it may entail simple business models with low growth potential. Increases in accommodation and food services which are primarily touristic, and seasonal activities, may also indicate a quality decrease of startups. Table C.20 shows that the real monthly average contribution basis is significantly lower in the post-reform compared to the pre-reform period. This could reflect an increase in *necessity-driven* entrepreneurship due to the reform. Furthermore, there is indeed a significant difference between the sectors which treated individuals worked in before they became unemployed and the new sector in which they start their business. We observe that the share of treated individuals who started a business in the construction sector significantly decreases, while significantly more individuals started a new business in the trade and high-skilled service sector (I&C, finance, real estate, and scientific). It is difficult to exactly decompose changes in sectors of new firms into the share of *necessity-/opportunity-driven* entrepreneurship. However, the significant increase in the high-skilled service sector points to more *opportunity-driven* entrepreneurship, whereas increases in the trade, accommodation and food service sectors indicate that more individuals are also pushed into *necessity-driven* entrepreneurship. Altogether, our descriptive evidence suggests that the dispersion in the quality of startups increased due to the reform.

So far we only considered correlations. In Table 3 we estimate the potential causal relationship between the cut in UI benefits and (self-)employment quality using quality proxies as outcome variables. First, we focus on our findings regarding self-employment quality. We restrict our sample to individuals who transition from unemployment into self-employment within the first 720 days of unemployment (4,132 individuals, as illustrated in Appendix C.1).[43] According to our results, the cut in UI benefits reduces the self-employment spell duration, indicating a quality decrease. However, the reform effect is estimated to be insignificant.[44] We find that the reform slightly increases the social security contribution basis during the self-employment spell, suggesting a quality increase. Nevertheless, this effect is only significant at the 10% level in a linear specification and turns insignificant if we use a quadratic or cubic functional form of the running variable. Our point estimates show that, on average, the reform decreases the probability of being a self-employed individual for those who earned a monthly income above the median before becoming unemployed.

[43] As our results are very similar if we restrict the sample even further to individuals who exit into self-employment within the first 360 days, we refrain from showing them here – also due to space limitations. They are available upon request.

[44] Kyyrä and Pesola (2020) also find insignificant effects of UI benefits on duration of the exit employment spell.

Table 3: Effect on (Self-)Employment Quality - Quadratic

Outcome Variable	RD Est.	s.e.	p-value	Bandwidth	N Left	N Right	Covs.
Duration (monthly)							
Employment	0.035	1.154	0.990	193.009	5138	5480	
	0.199	1.227	0.928	198.022	5321	5434	✓
Self-Employment	-0.893	4.462	0.928	212.747	922	877	
	-1.518	3.824	0.681	263.851	1079	1069	✓
ln(real monthly average contribution basis)							
Employment	-0.005	0.046	0.880	163.718	4251	4618	
	-0.005	0.044	0.944	191.233	4925	5267	✓
Self-Employment	0.024	0.029	0.374	247.499	1025	1008	
	0.038	0.028	0.169	249.093	1014	1007	✓
Above median wage pre UI receipt							
Employment	-0.030	0.047	0.580	241.809	6543	6816	
	-0.019	0.039	0.615	253.983	6666	6894	✓
Self-Employment	-0.076	0.077	0.412	228.250	978	925	
	-0.048	0.075	0.412	182.859	752	732	✓
Agriculture, extraction, primary manufacturing							
Employment	0.036	0.016	0.011	237.905	6436	6743	
	0.037	0.017	0.021	179.551	4652	5033	✓
Self-Employment	-0.010	0.028	0.824	189.985	798	763	
	-0.014	0.027	0.670	188.375	784	749	✓
Manufacturing and utilities							
Employment	-0.030	0.019	0.147	239.718	6493	6775	
	-0.019	0.016	0.280	268.679	7114	7252	✓
Self-Employment	-0.034	0.025	0.242	230.725	984	954	
	-0.035	0.025	0.197	255.110	1028	1026	✓
Construction							
Employment	0.010	0.030	0.558	182.187	4851	5250	
	0.004	0.019	0.673	169.365	4426	4593	✓
Self-Employment	0.052	0.071	0.458	229.972	981	954	
	0.045	0.053	0.408	242.699	994	976	✓
Trade							
Employment	-0.005	0.025	0.665	151.897	3917	4280	
	-0.015	0.021	0.316	147.435	3711	4068	✓
Self-Employment	0.090	0.081	0.158	166.067	711	656	
	0.067	0.072	0.232	152.583	632	597	✓
Transport and storage							
Employment	-0.001	0.013	0.933	205.727	5594	5917	
	-0.011	0.011	0.219	240.461	6337	6626	✓
Self-Employment	-0.022	0.042	0.444	184.396	780	744	
	-0.044	0.037	0.159	146.136	612	581	✓
Accommodation and food services							
Employment	-0.048	0.027	0.073	198.455	5451	5586	
	-0.005	0.017	0.683	241.878	6366	6640	✓
Self-Employment	-0.017	0.053	0.673	192.366	810	769	
	-0.033	0.049	0.385	208.193	890	849	✓
I&C, finance, real estate, and scientific services							
Employment	0.017	0.021	0.442	210.369	5696	6036	
	0.010	0.018	0.534	199.280	5342	5451	✓
Self-Employment	0.016	0.058	0.722	244.172	1015	999	
	0.052	0.045	0.240	232.551	971	949	✓
Education, health, social, and other services							
Employment	0.008	0.030	0.906	209.058	5691	6023	
	-0.014	0.024	0.485	199.106	5342	5451	✓
Self-Employment	-0.030	0.060	0.496	211.189	913	871	
	-0.009	0.048	0.807	230.120	968	940	✓
Permanent contract							
Employment	-0.023	0.042	0.705	182.023	4851	5250	
	-0.013	0.037	0.825	184.551	4785	5141	✓

Notes: In this table, we estimate the causal reform effect on (self-)employment quality. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014), a quadratic specification and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We restrict our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) to individuals who exit into (self-)employment within the first 720 days of unemployment. Results for individuals who exit into (self-)employment within the first 360 days of unemployment are provided in Table C.21. Detailed results for the linear and cubic specifications are available upon request. The median monthly real wage from social security data is EUR 1,471.63.
Source: Authors' calculations are based on MCVL 2005-2018 data.

While this result could indicate a quality decrease, it is again insignificant in all specifications. Additionally, we find no clear effects on the choice of post-unemployment sectors in which new firms are created. Our estimated reform effects are mostly statistically insignificant, except for the effect on the manufacturing and utilities sector in the linear version. However, if we apply different functional forms, the reform effect turns insignificant as well. The decrease in standard errors when covariates are added shows that we can increase precision. In Appendix C.3, we also show that we have no balancing problem which could cause changes in point estimates. Insignificant results could also point to a power issue. Nonetheless, the fact that most of the estimates are rather small and clearly insignificant shows that there are no significant changes in the composition of self-employment due to the reform. Altogether, we find descriptive evidence but no significant causal reform effect on the self-employment quality.

Table 4: Reform Effects on Monthly Earnings for (Self-)Employment within 360 Days – Quadratic

Time Horizon	RD Estimate	s.e.	p-value	Covs.	Bandwidth	N left	N right
12 months after							
Employment	0.031	0.048	0.335		145.690	2861	3219
	0.007	0.034	0.632	✓	166.876	3338	3528
Self-Employment	0.093	0.067	0.112		238.643	796	779
	0.103	0.064	0.076	✓	234.423	776	763
18 months after							
Employment	0.027	0.045	0.369		150.268	2942	3294
	0.014	0.027	0.438	✓	179.923	3520	3887
Self-Employment	0.068	0.074	0.304		199.310	712	611
	0.076	0.074	0.268	✓	189.292	626	587
24 months after							
Employment	0.005	0.055	0.714		175.068	3499	3908
	-0.015	0.035	0.595	✓	263.940	5358	5477
Self-Employment	0.110	0.071	0.097		199.315	702	608
	0.127	0.065	0.035	✓	181.345	591	567
36 months after							
Employment	0.021	0.052	0.497		169.375	3372	3606
	-0.009	0.033	0.710	✓	254.617	5037	5243
Self-Employment	0.023	0.072	0.737		258.245	844	824
	0.026	0.075	0.852	✓	201.639	685	627
48 months after							
Employment	0.021	0.043	0.419		150.893	2850	3193
	-0.022	0.028	0.440	✓	257.730	5159	5205
Self-Employment	0.060	0.086	0.426		228.101	755	709
	0.085	0.085	0.271	✓	170.966	549	526

<u>Notes:</u> In this table, we estimate the causal reform effect on earnings, approximated by the contribution basis in the case of self-employment for those who transition into (self-)employment within 360 days of entering unemployment. The dependent variable corresponds to log monthly earnings after different time periods measured in months after having entered UI. Note that the earnings or contribution basis in Table 3 and Table C.21 correspond to the exit status, i.e. the first status which an individual has after unemployment; whereas the earnings considered here might belong to a status which is different from the exit status, as we are able to trace the individuals' complete labor market trajectories until 2018. We estimate these effects on sub-samples defined by the individuals' exit status: *Employment* and *Self-Employment*. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014), a quadratic specification and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We restrict our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) to individuals who exit into (self-)employment within the first 360 days of unemployment. Results for the linear and cubic specifications are provided in Table C.22 and Table C.23. Detailed results for individuals who exit within the first 720 days of unemployment are provided upon request.
<u>Source:</u> Authors' calculations are based on MCVL 2005-2018 data.

A similar picture emerges if we look at our employment quality regressions in Table 3. When restricting our sample to individuals who exit to regular employment within the first 720 days of unemployment, our sample size is much larger (27,630 individuals, as illustrated in Appendix C.1). Nonetheless, most of our point estimates are insignificantly different from zero and an increase in the job-finding rate is only statistically significant in the primary sector. Thus, we find no evidence for a significant effect on employment quality. However, the signs of the reform effect resemble the observed signs of our descriptive analysis.

Finally, similar to Khoury, Brébion, and Briole (2019), we analyze the effect of UI benefits on post-unemployment re-employment wages and our proxy for self-employment income. Table 4 shows that treated individuals, who find re-employment within 360 days of unemployment, experience nearly no wage increases even four years after UI entry. Instead, our estimated effects on self-employment income are consistently higher, though they also mostly lack statistical significance. To sum up, our analysis reveals that reducing UI benefits may increase the dispersion of the quality of startups created out of unemployment and only slightly worsens the quality of re-employment.

6 Conclusion

This paper addresses how UI benefit levels affect both self-employment and employment. We account for heterogeneity in the timing of these effects and investigate whether our findings are driven by different subgroups. We also estimate the effect on actual unemployment duration and analyze potential welfare implications with regard to the quality of post-unemployment exit states. Finally, we rationalize our findings in the context of related literature in labor and public economics.

While the existing literature has addressed how UI policies affect unemployment duration and re-employment wages when self-employment is ignored in the analysis, we are the first to consider self-employment as an alternative post-unemployment outcome. Since active labor market policies, which incentivize unemployed individuals to start their own businesses, are commonly used policy measures to fight unemployment, understanding the effects of the design of UI policies on self-employment is extremely relevant.

To surpass data limitation on the labor market employment histories of founders, we exhausted Spanish administrative social insurance and labor income tax data to assess all relevant labor market flows over the business cycle (2005-2018). This has enabled us to conduct a descriptive analysis of self-employment in Spain in stock/flow dimension. Our findings show that flows from unemployment to self-employment are important in Spain: 30% of all new firms are created by founders who were previously unemployed. During the crisis, this share increased by up to 50%.

In our causal analyses, we exploit the Spanish labor market reform in 2012 which led to a sharp change in UI benefits: with the reform, the net replacement rate for the time after 180 days of benefit receipt decreased by 10 percentage points (from a replacement rate of 60% to one of 50%). Only individuals entitled to more than 180 days of UI benefits receipt were affected by this reform. This quasi-experimental set-up allows us to exploit reform-driven exogenous variation in UI benefit levels in order to estimate the causal effect of a cut in UI benefits on (self-)employment.

Our results suggest significant LATEs on the extensive margin of both employment and self-employment outcomes (Section 5.1). We find negative reform effects on the self-employment probability, expanding in size throughout the UI spell duration. This reveals heterogeneity in the effect size over time. Regarding the effects on re-employment, similar heterogeneity over time is prevalent, but in the opposite direction: reform effects on the job-finding rate are significantly positive in the short term, although they attenuate and turn rather insignificant in the medium and long term. In correspondence with Rebollo-Sanz and Rodríguez-Planas (2020), we find that individuals already adjust their search intensity before the actual cut in UI benefits takes place. In contrast to Rebollo-Sanz and Rodríguez-Planas (2020), we cannot only confirm an anticipation effect on the job-finding rate but also on the startup rate. In relative terms, the probability of becoming self-employed declines by around 35-50%, whereas the probability of becoming re-employed increases by 16-19%, and thus the first effect (startup rate) dominates the latter (job-finding rate). Moreover, in terms of effect size, our findings differ from their results: our estimated effects on employment are smaller than the estimates provided by Rebollo-Sanz and Rodríguez-Planas (2020). Their RDD results point towards a LATE on the job-finding rate of 26%, while our corresponding estimates are in a range between 16-19%. Additionally, we show that over different time horizons, the negative effect on self-employment (35-50%) is consistently stronger than the positive effect on employment (5-32%). Our findings suggest that the inclusion of data on self-employment is extremely important in order to evaluate the reform effect on the probability of exiting unemployment in general. Under heterogeneous treatment effects it should not be assumed that the positive effect on the job-finding rate represents the *general employment* effect (on both self-employment and employment), as the reform's negative effect on the startup rate is not taken into account (compare also Appendix C.6).

Our subgroup analysis shows that the significant negative effect on the medium-term startup rate is mainly driven by the more vulnerable subgroups (younger individuals, women, those with children, and immigrants). Additionally, better qualified individuals with a higher educational attainment and those with a pre-displacement real income above the median experience stronger decreases in the startup rate. The latter group is also the main driver of the significant positive effect on the job-finding rate. Overall, we find not only heterogeneity in the timing of reform effects but also in relation to the socio-economic status and pre-displacement job characteristics of treated unemployed individuals.

In line with the findings of Doris et al. (2020) and Rebollo-Sanz and Rodríguez-Planas (2020), we confirm that the UI benefit level duration elasticity is larger with regard to UI benefit level cuts rather than increases. We find that the UI duration elasticity is larger in absolute terms for those who exit into self-employment (between -0.2 and -1.5) compared to those who exit into employment (0.6-0.9), implying that the joint elasticity into either exit state is in between (0.3-0.6). Our elasticity results show a clear pattern that complements our findings on the extensive margins.

While we find mixed descriptive evidence for changes in (self-)employment quality due to the reform, we cannot confirm any causal relationship. The reform neither significantly affected the post-unemployment exit state quality of newly self-employed individuals, nor the one of re-employed individuals. Consequently, (self-)employment quality is barely affected by the cut in UI benefits.

Taking stocks also from the results derived in Camarero Garcia and Murmann (2020), both time and money are important when it comes to the effect of UI benefits on self-employment. Camarero Garcia and Murmann (2020) show that a PBD extension prolongs actual unemployment duration of founders. They find that the UI duration elasticity for those transitioning to self-employment is positive and larger than common estimates for those who are re-employed suggest. In summary, PBD changes can affect the quality of startups (Camarero Garcia and Murmann (2020)), whereas UI benefit levels can affect the extensive margin, i.e. the startup rate, as we show in this paper.

Overall, our results show that reducing UI benefit levels does not push unemployed individuals to become self-employed, but rather induces search for employment on the extensive margin. The existing literature offers two different theoretical explanations for this outcome. On the one hand, our findings are in line with the second hypothesis derived from *standard search theory* which suggests that a decrease in benefit levels leads to higher search intensity already at the beginning of the unemployment spell before the actual RR drop takes place. The reservation wage for employment decreases and labor becomes cheaper. Taking general equilibrium effects into account, both the number of job vacancies and labor market tightness increase. Consequently, we expect a higher job-finding rate. As employment increases, self-employment becomes less likely in relative terms and this is exactly what we discover in our results in the short term. On the other hand, we find evidence in favor of the *entrepreneurial choice model*. It predicts that the shortened UI duration, which is caused by the decrease in benefits, leads to less negative unemployment duration dependence (e.g. less human capital depreciation or stigma effects), and thus, better employment prospects compared with an unchanged UI level. Moreover, as there is less time for learning about proper business opportunities, it becomes relatively easier to find a job than starting a business (Alba-Ramirez, 1994). Both the decrease in the startup rate as well as the negative duration elasticity for the self-employed could be explained through liquidity constraints, imposed by the cut in UI benefits which hit potential founders stronger than individuals searching for regular jobs. Liquidity constraints could, on the one hand, prevent affected individuals from establishing a firm independent of the entrepreneurial ability. On the other hand, individuals who decide to start a business despite these constraints may need more time to collect sufficient funding which increases their actual unemployment duration.

As our findings are in line with different theoretical explanations, future research may help to rationalize our results in a theoretical model which takes the extensive margin effects of UI benefit levels on both employment and self-employment into account. For instance, an extended search-matching model including employment and self-employment as matched states alongside unmatched outcomes could rationalize our empirical findings and contribute to the policy debate.

Finally, in light of the crisis following the COVID-19 pandemic a reallocation shock may destroy many jobs (Barrero, Bloom, & Davis, 2020), potentially increasing the importance of transitions from unemployment to self-employment. Therefore, demands for optimizing the design of the UI system could be better addressed by taking the results of this paper into account, and consequently considering the role of UI benefits on both sides of the same coin: employment and self-employment.

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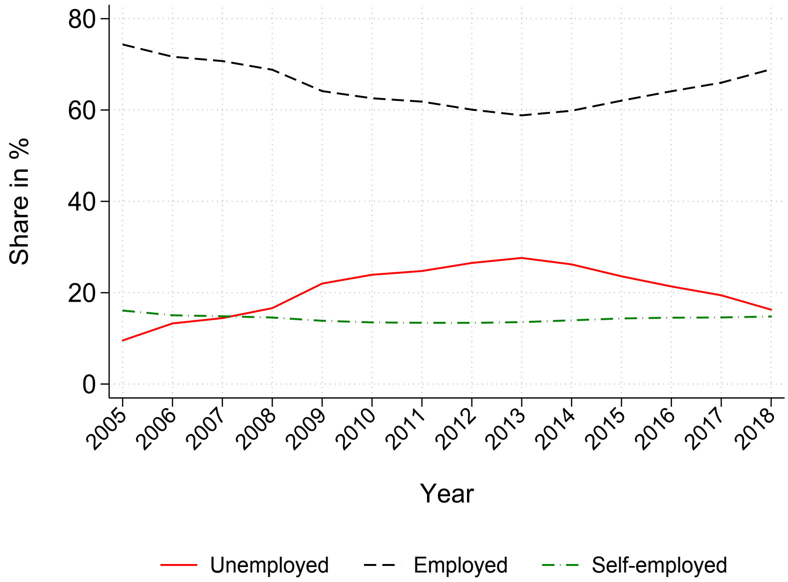
List of Abbreviations

CPI	Consumer Price Index.
CRR	Competing Risks Regression.
E	Employment.
EU	European Union.
INE	Instituto Nacional de Estadística.
IPREM	Public Income Index - *Indicador Público de Renta de Efectos Múltiples*.
LATE	Local Average Treatment Effect.
MCVL	Continuous Working Life Sample - *Muestra Continua de Vidas Laborales*.
OECD	Organization of Economic Co-operation and Development.
OL	Out of Labor Force.
PBD	Potential Benefit Duration.
RDD	Regression Discontinuity Design.
RR	Replacement Rate.
SE	Self-Employment.
SE or E	Self-Employment or Employment.
U	Unemployment.
UA	Unemployment Assistance.
UE	Unemployment.
UI	Unemployment Insurance.

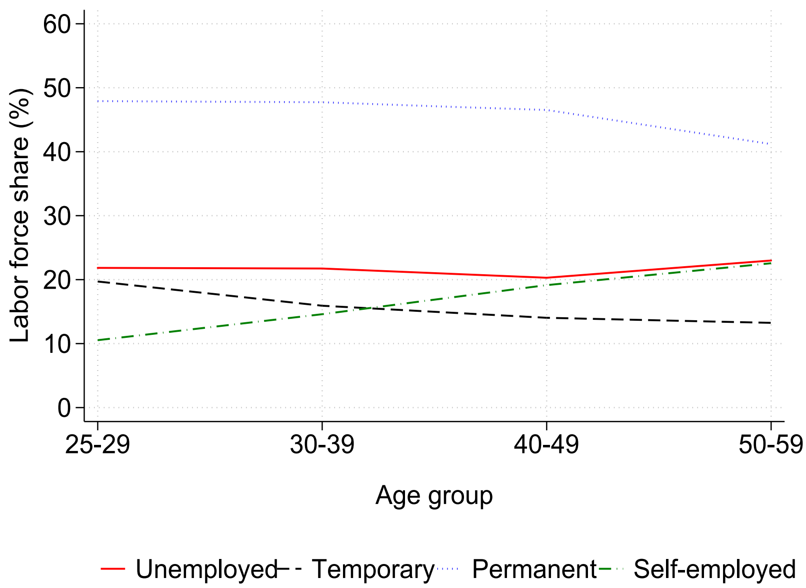
A Appendix: Descriptive Analysis Figures

Figure A.1: Composition of the Labor Force in Spain



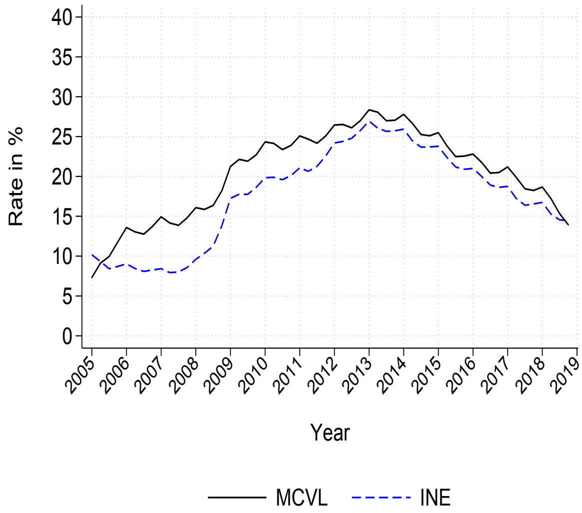
Notes: This figure illustrates the composition of the Spanish labor force between 2005 and 2018. It shows the percentage of individuals of working age (18 years of age or older) distinguishing Unemployment, Employment and Self-Employment.
Source: Authors' calculations are based on the MCVL 2005-2018 data.

Figure A.2: Distribution of Workers Across Employment States and Age Groups

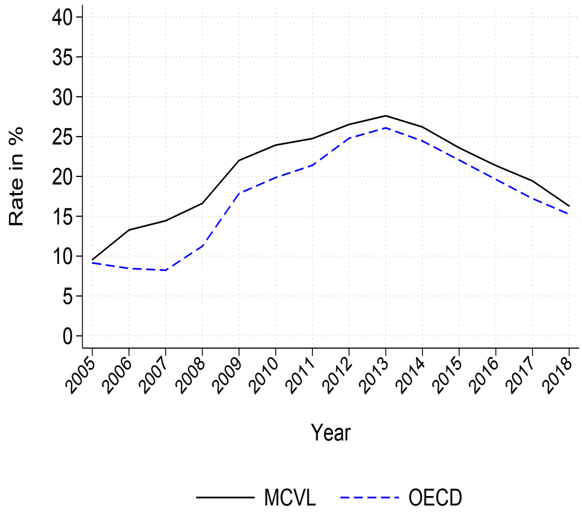


Notes: This figure illustrates the distribution of workers across the different employment states, including Unemployment, Temporary Employment, Permanent Employment and Self-Employment, with respect to their age group, as a percentage of the Spanish labor force.
Source: Authors' calculations are based on the MCVL 2005-2018 data.

Figure A.3: Unemployment Rate



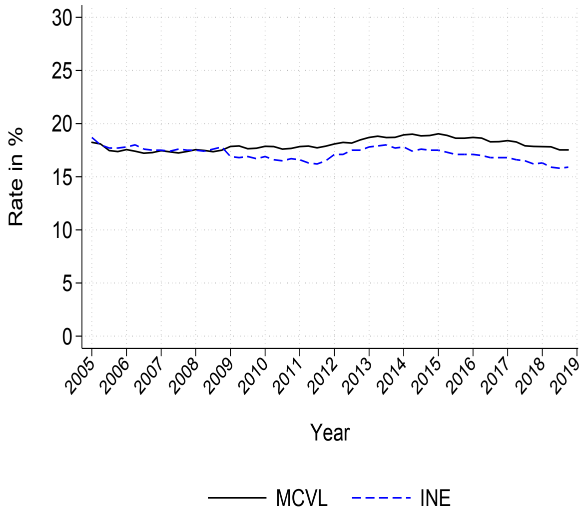
(a) Quarterly



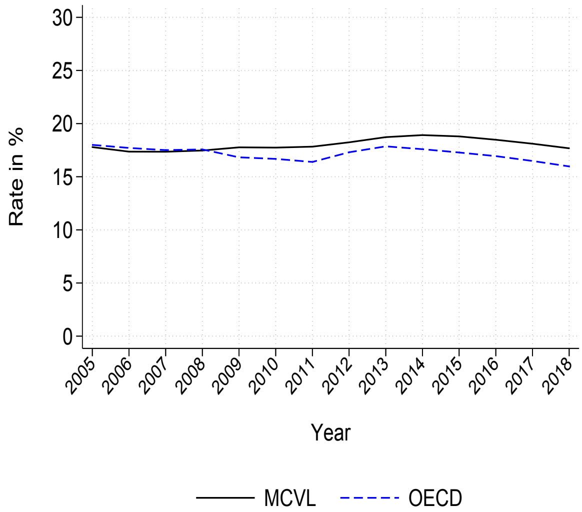
(b) Annually

Notes: The left-hand figure illustrates the evolution of the unemployment rates in Spain from 2005 to 2018 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis. Note that our definition of unemployment includes individuals who receive either UI or UA benefits, as well as individuals who do not receive any benefits at all, and those who are tagged as receiving cease-of-activity benefits.
Source: Authors' calculations are based on MCVL 2005-2018 data and official statistics provided by INE (2018) and OECD (2018).

Figure A.4: Self-Employment Rate



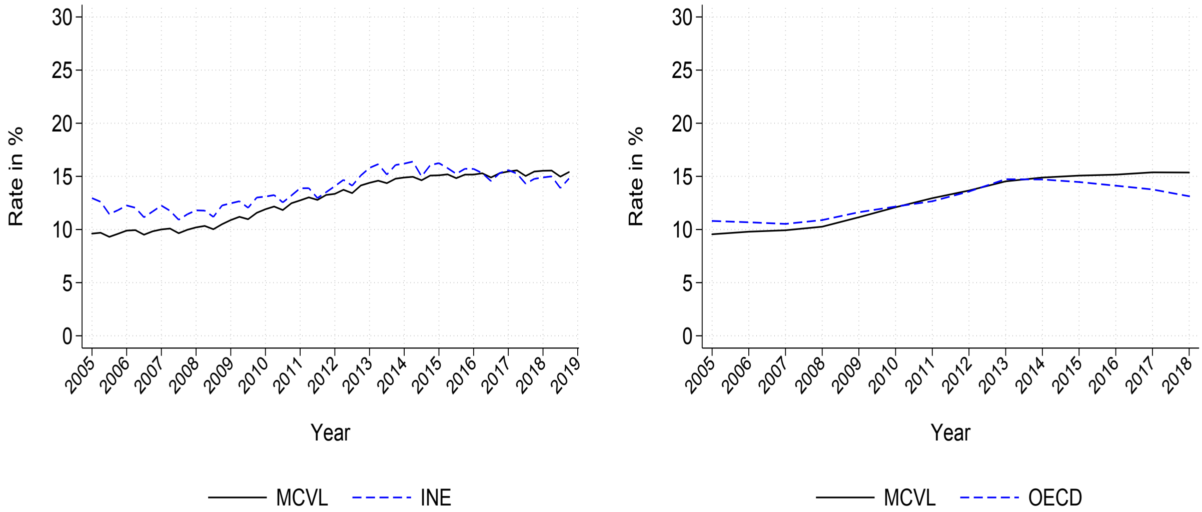
(a) Quarterly



(b) Annually

Notes: The left-hand figure illustrates the evolution of the self-employment rates in Spain from 2005 to 2018 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis.
Source: Authors' calculations are based on MCVL 2005-2018 data and official statistics provided by INE (2018) and OECD (2018).

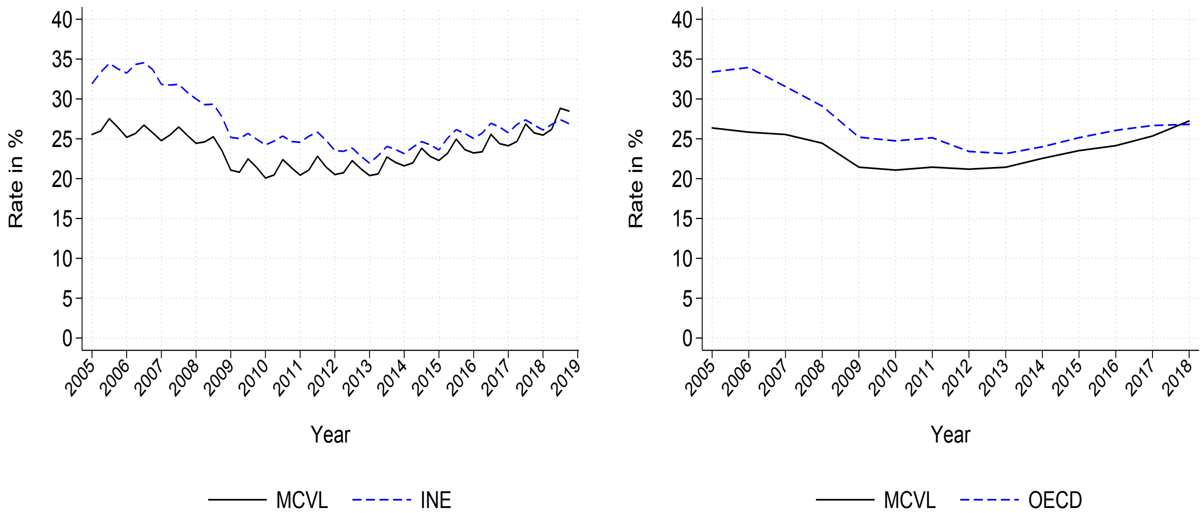
Figure A.5: Part-Time Employment Rate



(a) Quarterly **(b)** Annually

Notes: The left-hand figure illustrates the evolution of the part-time employment rates in Spain from 2005 to 2018 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis.
Source: Authors' calculations are based on MCVL 2005-2018 data and official statistics provided by INE (2018) and OECD (2018).

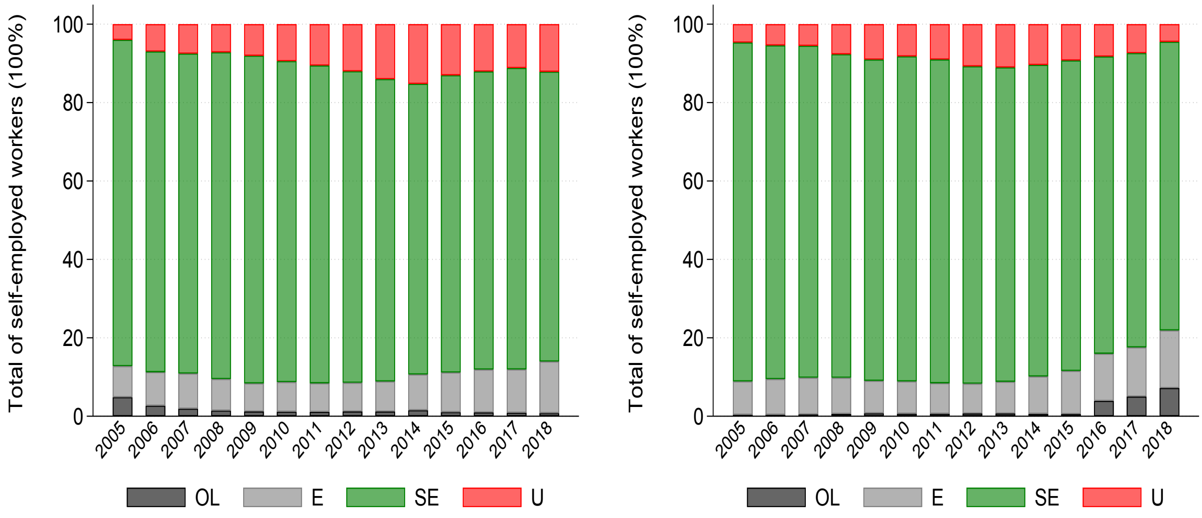
Figure A.6: Temporary Employment Rate



(a) Quarterly **(b)** Annually

Notes: The left-hand figure illustrates the evolution of the temporary employment rates in Spain from 2005 to 2018 on a quarterly basis. The right-hand figure illustrates the evolution of the same rates on a yearly basis.
Source: Authors' calculations are based on MCVL 2005-2018 data and official statistics provided by INE (2018) and OECD (2018).

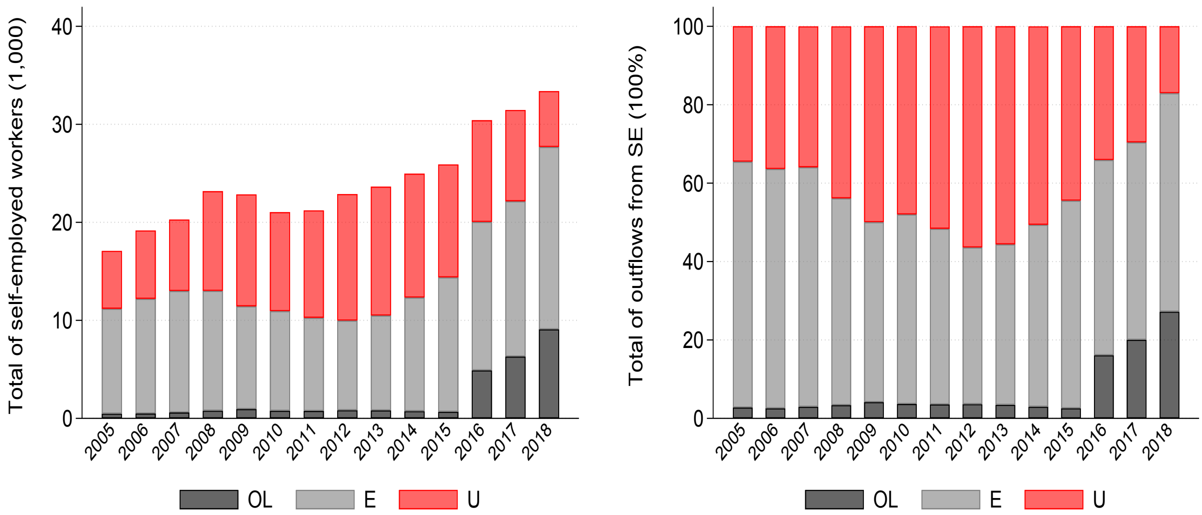
Figure A.7: Composition of Self-Employment Inflows and Outflows incl. Stocks



(a) Yearly Inflows in % **(b)** Yearly Outflows in %

Notes: These figures illustrate the yearly composition of Self-Employment (SE) in Spain providing the share of each component in percentage of the total stock. The sample is restricted to individuals of working age (18 years of age or older). We distinguish transitions to SE (inflows), on the left-hand side, and transitions from self-employment (outflows), on the right-hand side, with respect to the following labor market states: Out of Labor Force (OL), Employment (E), Unemployment (U), and the corresponding stock of those who remain in SE. See Figure 1 for the composition of inflows into SE excluding stocks.
Source: Authors' calculations are based on MCVL 2005-2018 data.

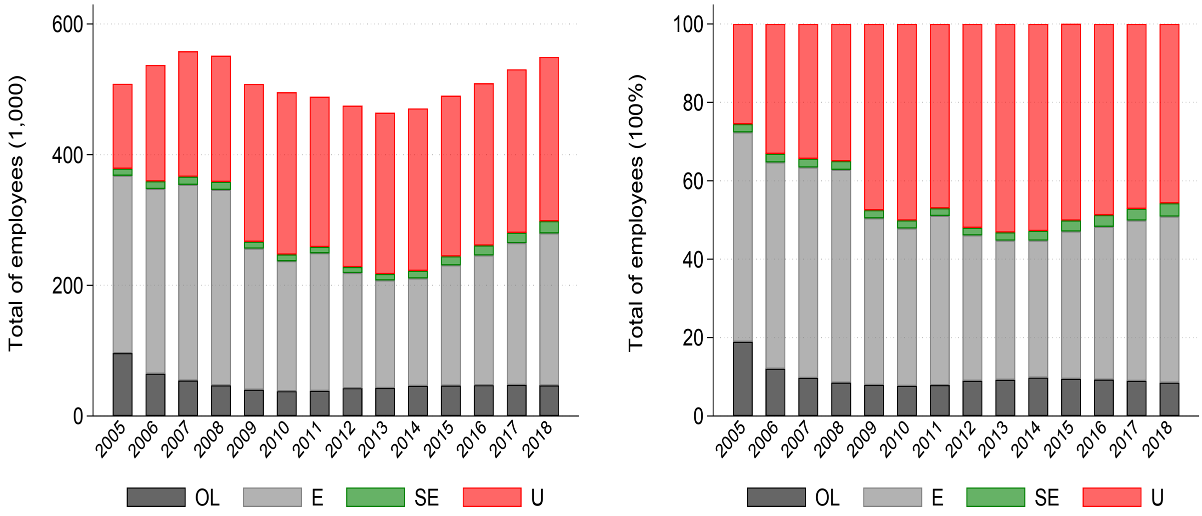
Figure A.8: Composition of Outflows from Self-Employment excl. Stocks



(a) Yearly Outflows **(b)** Yearly Outflows in %

Notes: These figures illustrate the yearly outflows from Self-Employment (SE) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (18 or older). We distinguish outflows of individuals from SE to the following labor market states: Out of Labor Force (OL), Employment (E), and Unemployment (U). This is the other side of the coin: the inflows are shown in the main text in Figure 1.
Source: Authors' calculations are based on MCVL 2005-2018 data.

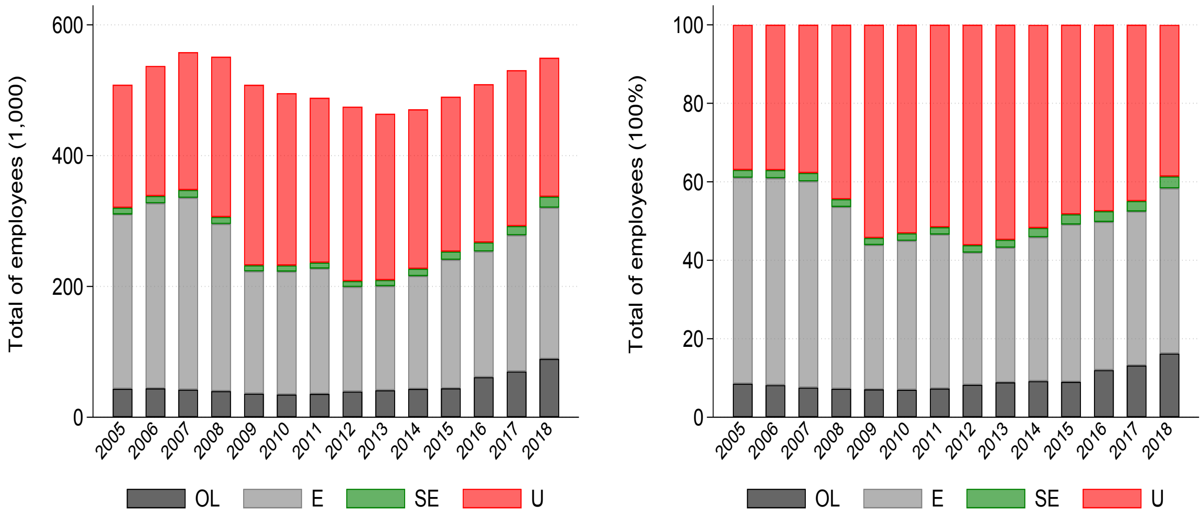
Figure A.9: Composition of Inflows into Employment



(a) Yearly Inflows **(b)** Yearly Inflows in %

<u>*Notes:*</u> These figures illustrate the yearly composition of transitions to Employment (E) (inflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, 18 years of age or older). We consider inflows of individuals into E from the following labor market states: Out of Labor Force (OL), Self-Employment (SE), and Self-Employment (SE), along with the corresponding stock of those who remain in E.
<u>*Source:*</u> Authors' calculations are based on MCVL 2005-2018 data.

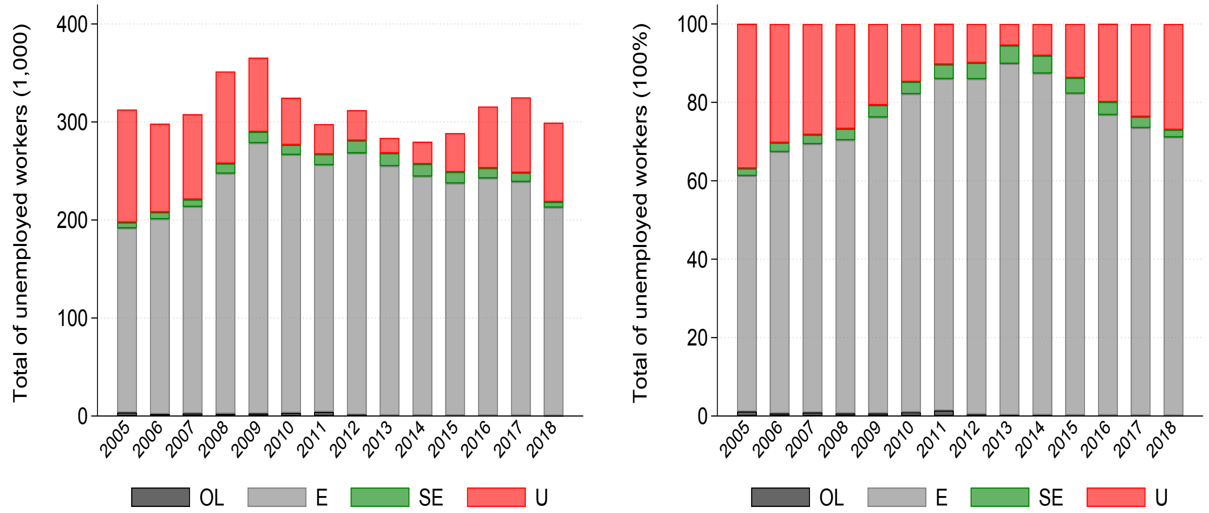
Figure A.10: Composition of Outflows from Employment



(a) Yearly Outflows **(b)** Yearly Outflows in %

<u>*Notes:*</u> These figures illustrate the yearly composition of transitions from Employment (E) (outflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (in this case, 18 years of age or older). We consider outflows of individuals from E into the following labor market states: Out of Labor Force (OL), Self-Employment (SE), and Unemployment (U), along with the corresponding stock of those who remain in E.
<u>*Source:*</u> Authors' calculations are based on MCVL 2005-2018 data.

Figure A.11: Composition of Inflows into Unemployment



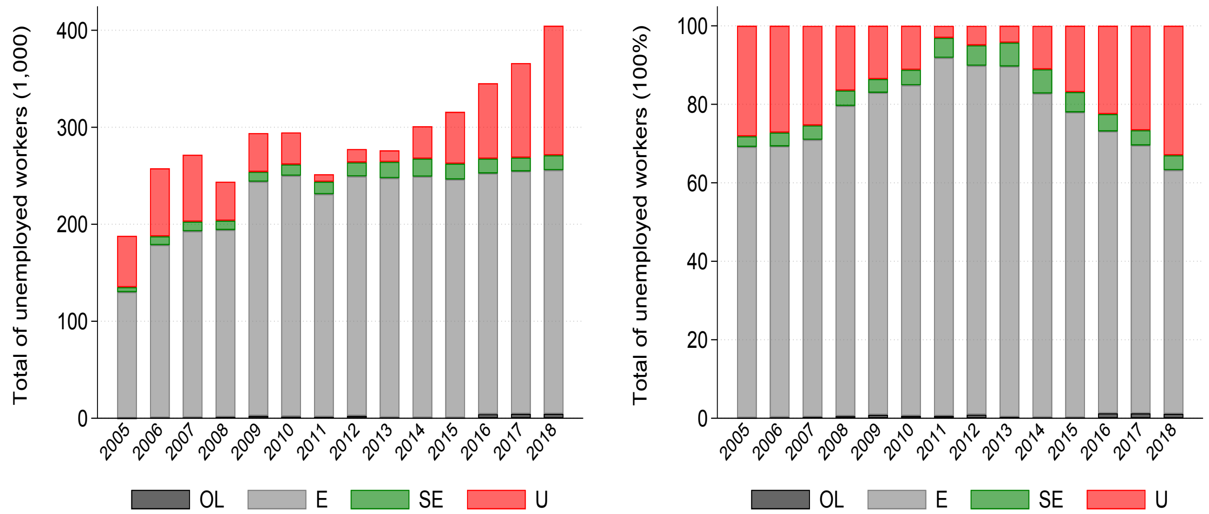
(a) Yearly Inflows

(b) Yearly Inflows in %

Notes: These figures illustrate the yearly composition of transitions to Unemployment (U) (inflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (18 years of age or older). We consider inflows of individuals into U from the following labor market states: Out of Labor Force (OL), Employment (E), and Self-Employment (SE), along with the corresponding stock of those who remain in U.

Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure A.12: Composition of Outflows from Unemployment



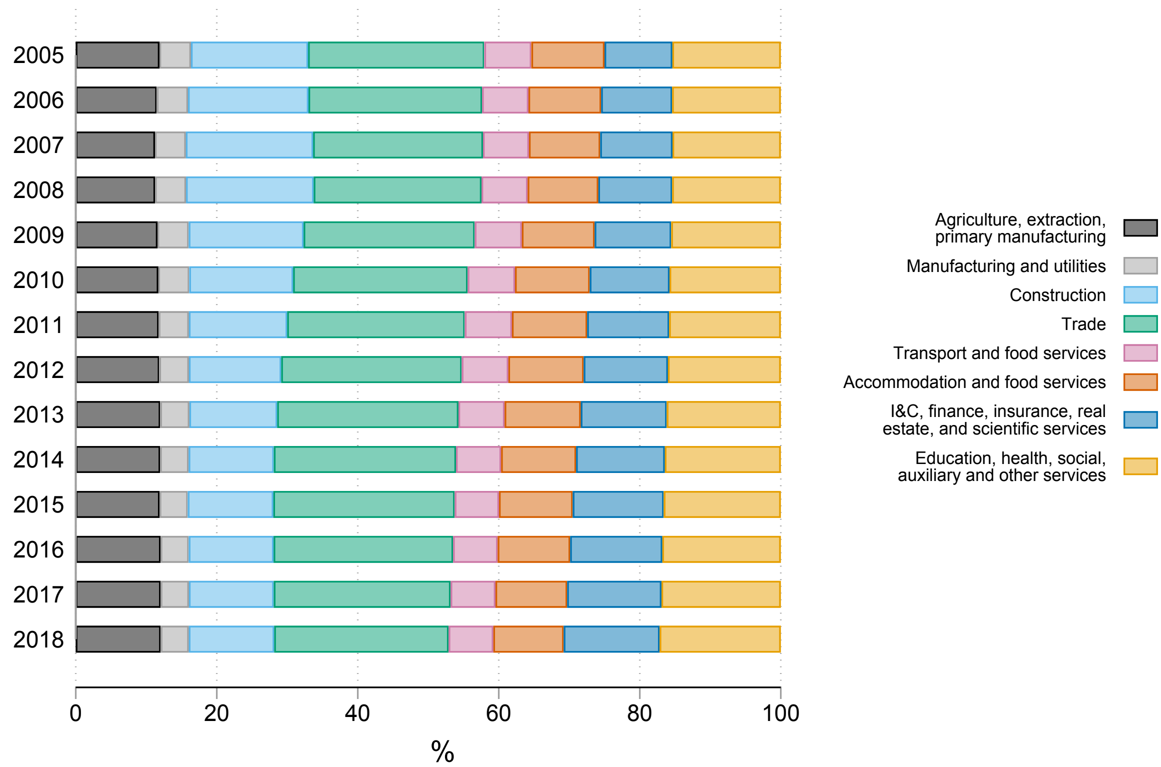
(a) Yearly Outflows

(b) Yearly Outflows in %

Notes: These figures illustrate the yearly composition of transitions from Unemployment (U) (outflows) in Spain, in both absolute (left) and relative (right) terms. The sample is restricted to individuals of working age (18 years of age or older). We consider outflows of individuals from U into the following labor market states: Out of Labor Force (OL), Employment (E), and Self-Employment (SE), along with the corresponding stock of those who remain in U.

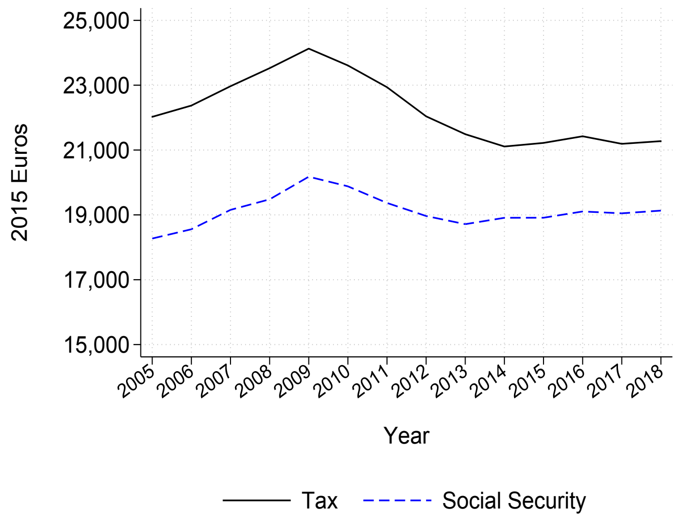
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure A.13: Sector Distribution of the Self-Employed



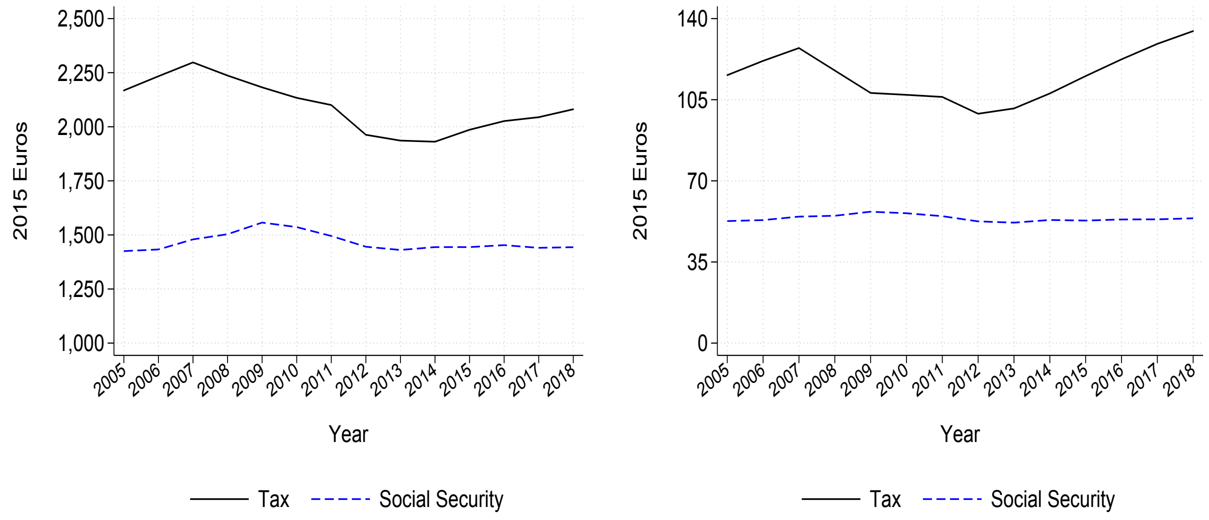
Notes: This figure illustrates the composition of self-employment in Spain, with respect to the sector variable in each year. The sample is restricted to individuals who are 25 to 52 years old.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure A.14: Evolution of Average Annual Earnings



Notes: This figure illustrates the evolution of average annual real earnings in Spain for Employment, according to the Social Security records and the tax files. The sample is restricted to individuals who are 18 years of age or older.
Source: Authors' calculations are based on MCVL 2005-2018 data.

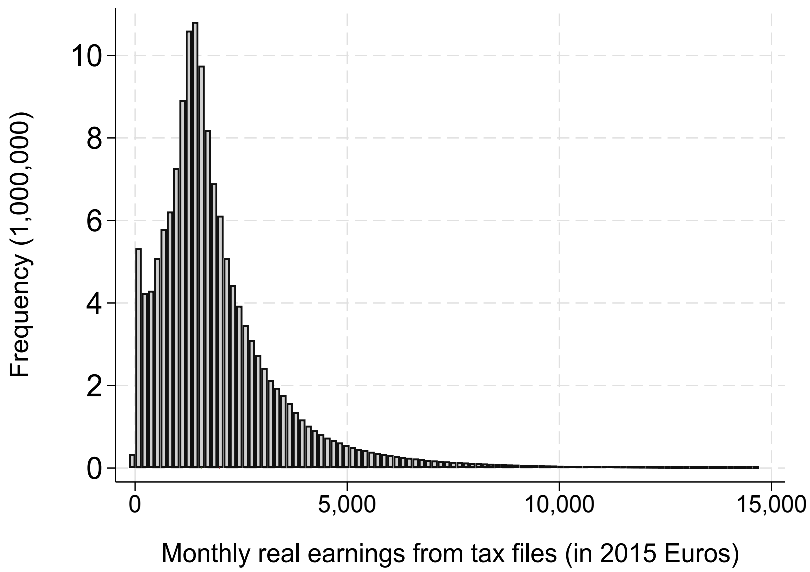
Figure A.15: Evolution of Average Monthly and Daily Earnings



(a) Monthly Earnings **(b)** Daily Earnings

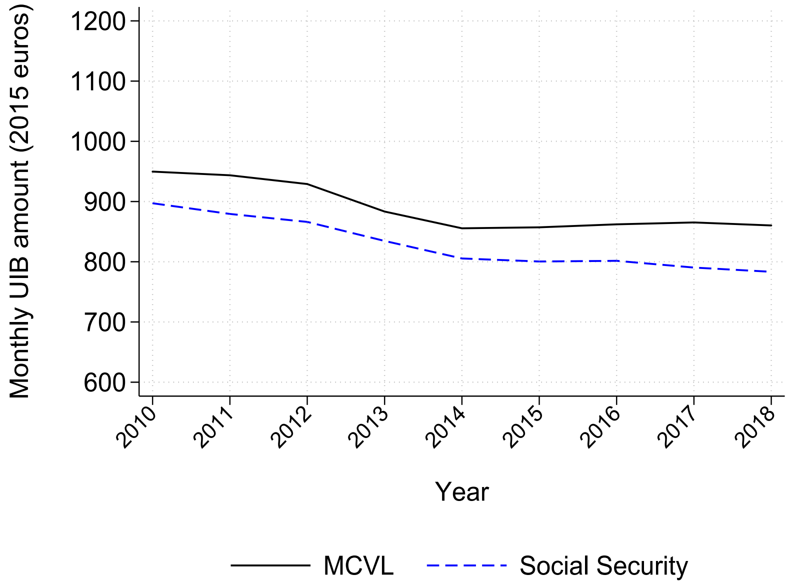
Notes: These figures illustrate the evolution of average monthly (left) and daily (right) real earnings in Spain for Employment, according to the Social Security records and the tax files. The sample is restricted to individuals who are 18 years of age or older.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure A.16: Distribution of Monthly Earnings (Tax Data)



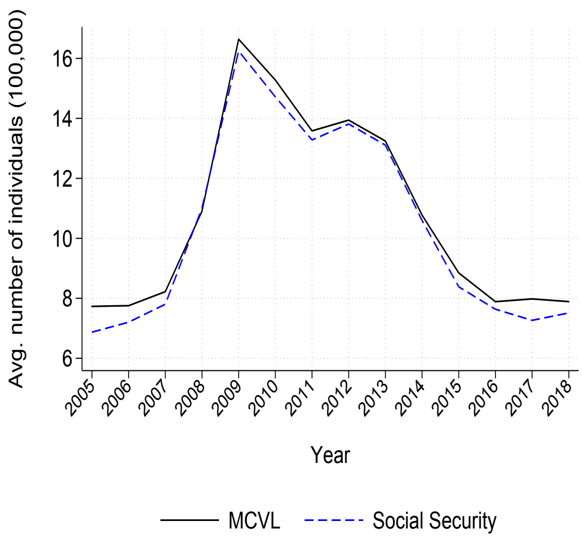
Notes: This figure illustrates the distribution of monthly real earnings in Spain with a mean value of 1,981.81 euros and a median of 1,564.67 euros, according to the tax files. The sample is restricted to individuals who are 18 years of age or older.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure A.17: Evolution of Unemployment Insurance Benefit Levels

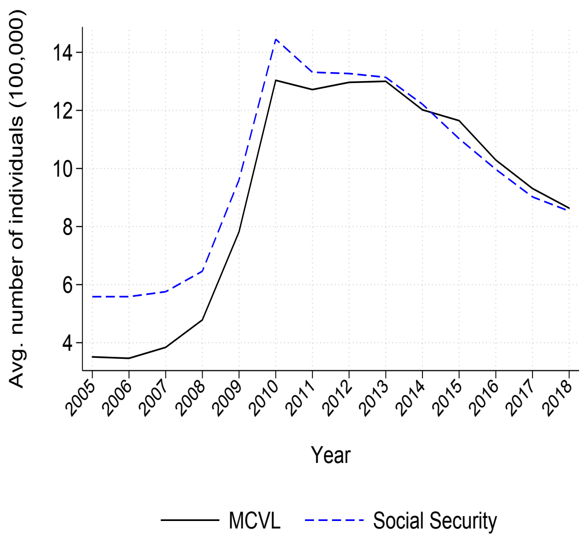


Notes: This figure illustrates the evolution of yearly average UI benefit levels. The solid line corresponds to our data set, whilst the dashed line has been obtained from the official statistics published by the Ministry of Labor. Moreover, our sample is restricted to individuals who are 18 years of age or older.
Source: Authors' calculations are based on MCVL 2005-2018 data; and official statistics by Spain's Ministry of Labor (2020).

Figure A.18: Evolution of Unemployment Insurance and Unemployment Assistance Beneficiaries



(a) UI Beneficiaries



(b) UA Beneficiaries

Notes: These figures illustrate the evolution of the average number of UI and UA beneficiaries on a yearly basis. The solid line corresponds to our dataset, where the numbers have been re-scaled using the official proportions provided in Dirección General de Ordenación de la Seguridad Social (2020). The dashed line has been obtained from the official statistics published by the Ministry of Labor. Moreover, our sample is restricted to individuals who are 18 years of age or older.
Source: Authors' calculations are based on MCVL 2005-2018 data; and official statistics by Spain's Ministry of Labor (2020).

B Appendix: Supplementary Tables

Table B.1: Personal Characteristics - Descriptives Sample

	Self-employment		Employment		Total Sample	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Female	0.352	(0.478)	0.471	(0.499)	0.465	(0.499)
Age (years)	43.760	(12.016)	36.884	(12.261)	38.190	(12.719)
Lower education	0.632	(0.482)	0.599	(0.490)	0.623	(0.485)
Medium education	0.239	(0.427)	0.252	(0.434)	0.244	(0.429)
Higher education	0.129	(0.335)	0.149	(0.356)	0.133	(0.339)
Presence of children	0.393	(0.488)	0.458	(0.498)	0.459	(0.498)
Immigrant	0.153	(0.360)	0.256	(0.436)	0.240	(0.427)
Employment experience (months)	61.258	(80.541)	144.068	(146.124)	122.854	(135.461)
Self-employment experience indicator			0.115	(0.319)	0.223	(0.416)
Real monthly average earnings	1,266.161	(2,856.369)	1,731.593	(3,056.539)	1,660.452	(2,988.341)
ln(real monthly average earnings)	6.909	(0.396)	7.153	(0.739)	7.125	(0.690)
Low-skilled occupation			0.540	(0.498)	0.686	(0.464)
Medium-skilled occupation			0.296	(0.457)	0.205	(0.404)
High-skilled occupation			0.163	(0.370)	0.109	(0.312)
Permanent contract			0.399	(0.490)	0.234	(0.423)
Agriculture, extraction, primary manufacturing	0.132	(0.338)	0.078	(0.267)	0.059	(0.235)
Manufacturing and utilities	0.040	(0.197)	0.067	(0.250)	0.043	(0.203)
Construction	0.125	(0.330)	0.075	(0.264)	0.057	(0.231)
Trade	0.247	(0.431)	0.132	(0.338)	0.102	(0.302)
Transport and storage	0.058	(0.235)	0.035	(0.185)	0.027	(0.161)
Acommodation and food services	0.094	(0.292)	0.098	(0.297)	0.067	(0.250)
I&C, finance, insurance, real estate, and scientific services	0.104	(0.305)	0.090	(0.286)	0.063	(0.243)
Education, health, social, auxiliary and other services	0.156	(0.363)	0.357	(0.479)	0.225	(0.417)
PBD (months)	15.457	(7.041)	13.254	(7.330)	11.315	(9.657)
Local unemployment rate	11.402	(5.257)	12.194	(5.829)	12.157	(5.794)
Observations	133,746		790,152		1,347,976	

Notes: This table presents mean values and standard deviations for personal characteristics. We distinguish between self-employed individuals and employed individuals. The *Total Sample* column additionally includes cease-of-activity/UI/UA benefit recipients, and unregistered/inactive individuals. Time-varying characteristics refer to the last spell of each individual. The information refers to our sample for the years between 2005 and 2018, restricted to individuals who are 18 years of age or older. Note that information on occupational codes is not provided for individuals in the social security scheme of self-employed workers. Therefore, we do not have data on skill levels for the self-employed (Dirección General de Ordenación de la Seguridad Social, 2020).
Source: Authors' calculations are based on the 2005-2018 MCVL data.

Table B.2: Minimum and Maximum UI Benefit Amount (valid 2010-2016)

Dependent Children	Minimum	Maximum
0	80% IPREM + 1/6 · (monthly benefit) [497.01€]	175% IPREM [1,087.21€]
1	107% IPREM + 1/6 · (monthly benefit) [664.75€]	200% IPREM [1,242.52€]
≥ 2	107% IPREM + 1/6 · (monthly benefit) [664.75€]	225% IPREM [1,397.84€]

Notes: This table summarizes the computation of the legal maximum and minimum benefit amounts. These limits depend on the family responsibilities (number of dependent children or descendants) and the value of the IPREM index in a given year. In the period 2010-2016, the IPREM index remained unchanged at 532.51 euros per month.
Source: Authors' own illustration based on the SEPE (2019).

Table B.3: Duration of Entitlement to UI Benefits

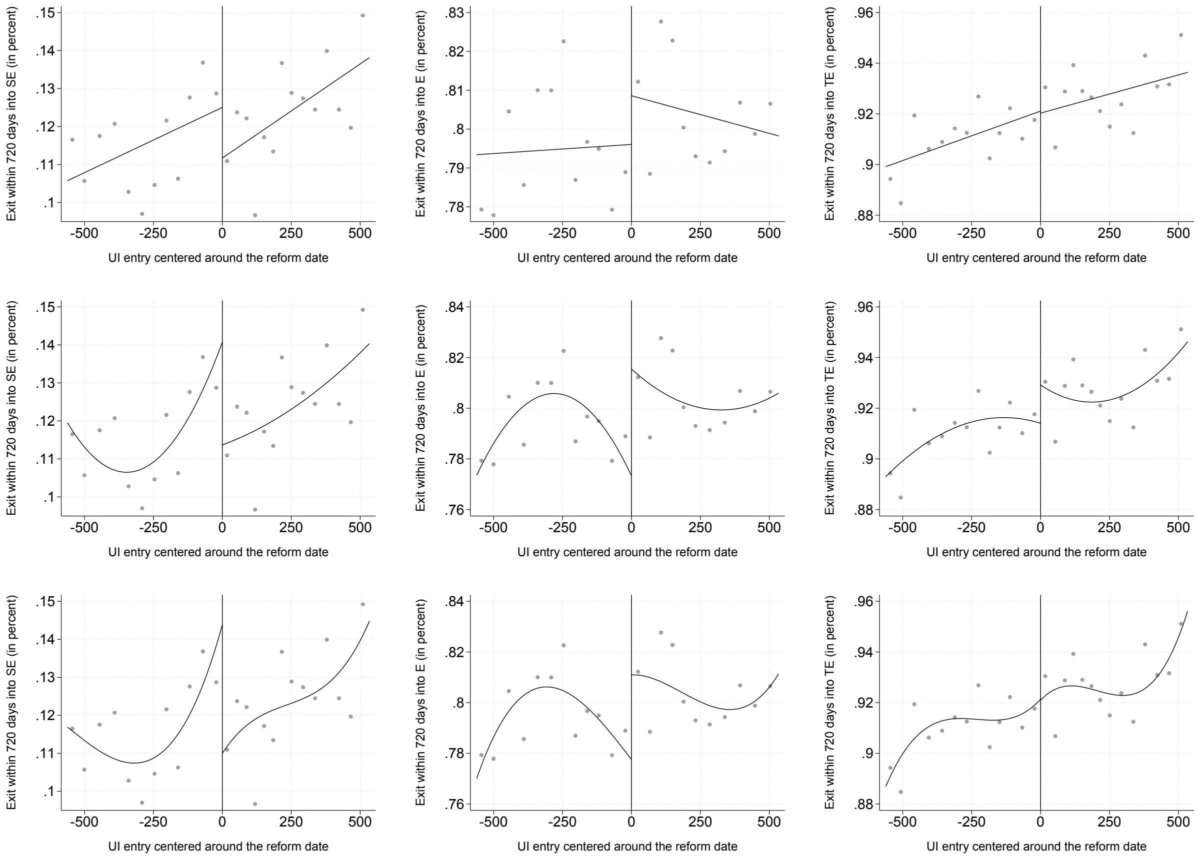
Contribution Period (in days)	**Potential Benefit Duration** (in days)
< 360	0
360 - 539	120
540 - 719	180
720 - 899	240
900 - 1,079	300
1,080 - 1,259	360
1,260 - 1,439	420
1,440 - 1,619	480
1,620 - 1,799	540
1,800 - 1,979	600
1,980 - 2,159	660
$\geq$ 2,160	720

Notes: This table summarizes the Spanish system of PBD. Eligibility requires a minimum contribution period of 360 days. PBD is a function of the individual's contribution period and ranges from 120 to 720 days.
Source: Authors' own illustration based on the SEPE (2019).

C Appendix: RDD Analysis

Figure C.1: Reform Effects on the Extensive Margin from the Raw Data

(a) Self-Employment **(b)** Employment **(c)** Self-Employment or Employment



Notes: These figures illustrate the reform effect on the probability of exiting unemployment into self-employment, employment, or either one of them within the first 720 days of the UI spell from the raw data. We apply the IMSE-optimal number of quantile-spaced bins using a linear (first row), quadratic (second row), and cubic (third row) polynomial. Our sample includes individuals who are 25-52 years old, entitled to more than 180 days of UI benefits, and who entered their UI benefit spell between January 1 2011 and December 31 2013, after having been laid-off from a full-time employment spell in a private firm (see Section 4.1 for a description of detailed sample restrictions). Figure 3 shows the main effects using only a cubic polynomial.
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.1 Summary Statistics

Table C.1: Personal Characteristics - RDD Sample

	Self-employment		Employment		Total Sample	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Female	0.289	(0.453)	0.365	(0.482)	0.365	(0.481)
Age (years)	37.069	(6.893)	36.889	(7.193)	36.907	(7.160)
Lower education	0.523	(0.500)	0.588	(0.492)	0.580	(0.494)
Medium education	0.306	(0.461)	0.276	(0.447)	0.279	(0.449)
Higher education	0.171	(0.376)	0.136	(0.343)	0.141	(0.348)
Presence of children	0.551	(0.497)	0.522	(0.500)	0.527	(0.499)
Immigrant	0.165	(0.371)	0.192	(0.394)	0.200	(0.400)
Employment experience (months)	146.773	(78.642)	143.195	(83.588)	140.966	(82.361)
Self-employment experience indicator	0.224	(0.417)	0.145	(0.352)	0.155	(0.362)
Real monthly average earnings	1697.946	(685.418)	1633.885	(636.977)	1625.817	(640.979)
ln(real monthly average earnings)	7.368	(0.364)	7.341	(0.332)	7.335	(0.334)
Low-skill occupation	0.495	(0.500)	0.577	(0.494)	0.565	(0.496)
Medium-skill occupation	0.326	(0.469)	0.309	(0.462)	0.316	(0.465)
High-skill occupation	0.179	(0.383)	0.114	(0.317)	0.119	(0.324)
Permanent contract	0.792	(0.406)	0.683	(0.465)	0.694	(0.461)
Agriculture, extraction, primary manufacturing	0.054	(0.226)	0.063	(0.243)	0.061	(0.240)
Manufacturing and utilities	0.082	(0.274)	0.119	(0.323)	0.110	(0.313)
Construction	0.174	(0.380)	0.184	(0.388)	0.181	(0.385)
Trade	0.242	(0.428)	0.196	(0.397)	0.205	(0.404)
Transport and storage	0.059	(0.235)	0.057	(0.233)	0.056	(0.230)
Acommodation and food services	0.084	(0.277)	0.118	(0.322)	0.115	(0.318)
I&C, finance, insurance, real estate, and scientific services	0.140	(0.347)	0.098	(0.297)	0.104	(0.305)
Education, health, social, auxiliary and other services	0.165	(0.371)	0.165	(0.371)	0.168	(0.374)
PBD (months)	20.250	(5.375)	18.904	(6.105)	18.857	(6.103)
Local unemployment rate	23.635	(6.402)	23.613	(6.366)	23.641	(6.370)
Observations	4,132		27,630		34,581	

Notes: This table presents mean values and standard deviations for pre-displacement personal characteristics of individuals in our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). We distinguish between individuals who transition from unemployment into self-employment or employment within the first 720 days of their unemployment spell. The *Total Sample* column additionally includes those who stay unemployed or whose actual exit states are censored.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.2: Summary Statistics - Extensive Margin Outcome Variables

Outcome Variable	**Mean**	**Pre Mean**	**Post Mean**	**Difference**
SE within 90 days	0.055	0.055	0.056	0.000
	(0.229)	(0.229)	(0.229)	(0.002)
SE within 180 days	0.078	0.077	0.079	0.002
	(0.269)	(0.267)	(0.270)	(0.003)
SE within 360 days	0.099	0.096	0.103	0.007**
	(0.299)	(0.295)	(0.304)	(0.003)
SE within 720 days	0.119	0.115	0.124	0.009**
	(0.324)	(0.320)	(0.330)	(0.003)
E within 90 days	0.279	0.294	0.263	-0.030***
	(0.449)	(0.455)	(0.440)	(0.005)
E within 180 days	0.443	0.458	0.426	-0.032***
	(0.497)	(0.498)	(0.495)	(0.005)
E within 360 days	0.629	0.628	0.629	0.001
	(0.483)	(0.483)	(0.483)	(0.005)
E within 720 days	0.799	0.795	0.804	0.009**
	(0.401)	(0.404)	(0.397)	(0.004)
SE or E within 90 days	0.335	0.349	0.319	-0.030***
	(0.472)	(0.477)	(0.466)	(0.005)
SE or E within 180 days	0.521	0.536	0.505	-0.030***
	(0.500)	(0.499)	(0.500)	(0.005)
SE or E within 360 days	0.728	0.724	0.732	0.008*
	(0.445)	(0.447)	(0.443)	(0.005)
SE or E within 720 days	0.918	0.910	0.928	0.018***
	(0.274)	(0.286)	(0.259)	(0.003)
N	34,581	18,324	16,257	34,581

Notes: This table shows the general sample mean, pre-reform period mean, post-reform period mean and the difference between post- and pre-reform period mean of our extensive margin outcome variables using our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). The outcome variables are binary and indicate whether the person transitioned into a (self-)employment spell within the first 90, 180, 360 or 720 days of unemployment, respectively.
Source: Authors' calculations are based on MCVL 2005-2018 data.

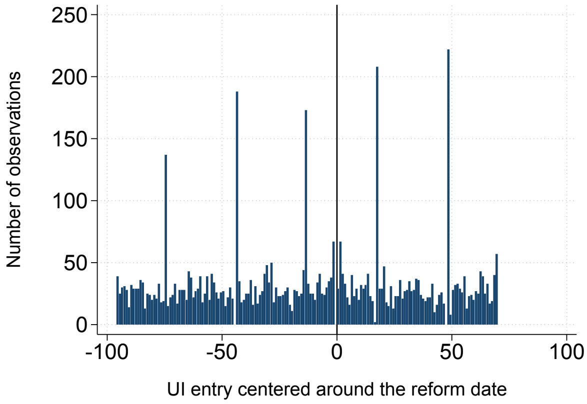
Table C.3: Summary Statistics - Covariates

Covariate	**Mean**	**Pre Mean**	**Post Mean**	**Difference**
Female	0.365	0.353	0.379	0.026***
	(0.481)	(0.478)	(0.485)	(0.005)
Age (years)	36.907	36.739	37.096	0.357***
	(7.160)	(7.158)	(7.157)	(0.077)
Lower education	0.580	0.596	0.562	-0.034***
	(0.494)	(0.491)	(0.496)	(0.005)
Medium education	0.279	0.278	0.280	0.002
	(0.449)	(0.448)	(0.449)	(0.005)
Higher education	0.141	0.126	0.158	0.033***
	(0.348)	(0.331)	(0.365)	(0.004)
Presence of children	0.527	0.532	0.521	-0.011**
	(0.499)	(0.499)	(0.500)	(0.005)
Immigrant	0.200	0.204	0.195	-0.010**
	(0.400)	(0.403)	(0.396)	(0.004)
Employment experience (months)	140.966	138.105	144.191	6.086***
	(82.361)	(82.869)	(81.667)	(0.887)
Self-employment experience indicator	0.155	0.158	0.152	-0.005
	(0.362)	(0.365)	(0.360)	(0.004)
ln(real monthly average earnings)	7.335	7.339	7.330	-0.009**
	(0.334)	(0.328)	(0.341)	(0.004)
Low skilled occupation	0.565	0.582	0.547	-0.034***
	(0.496)	(0.493)	(0.498)	(0.005)
Medium skilled occupation	0.316	0.306	0.327	0.021***
	(0.465)	(0.461)	(0.469)	(0.005)
High skilled occupation	0.119	0.113	0.126	0.013***
	(0.324)	(0.316)	(0.332)	(0.003)
Permanent contract	0.694	0.686	0.703	0.018***
	(0.461)	(0.464)	(0.457)	(0.005)
Agriculture, extraction, primary manufacturing	0.061	0.063	0.059	-0.004
	(0.240)	(0.243)	(0.237)	(0.003)
Manufacturing and utilities	0.110	0.115	0.105	-0.010***
	(0.313)	(0.319)	(0.306)	(0.003)
Construction	0.181	0.205	0.154	-0.051***
	(0.385)	(0.403)	(0.361)	(0.004)
Trade	0.205	0.199	0.213	0.014***
	(0.404)	(0.399)	(0.409)	(0.004)
Transport and storage	0.056	0.054	0.058	0.004
	(0.230)	(0.227)	(0.234)	(0.002)
Accommodation and food services	0.115	0.107	0.123	0.016***
	(0.318)	(0.309)	(0.328)	(0.003)
I&C	0.104	0.098	0.110	0.011***
	(0.305)	(0.298)	(0.313)	(0.003)
Education, health, social, and other services	0.168	0.159	0.178	0.019***
	(0.374)	(0.366)	(0.382)	(0.004)
PBD (months)	18.857	18.843	18.874	0.031
	(6.103)	(6.055)	(6.157)	(0.066)
Local unemployment rate	25.214	2.984***		
	(6.370)	(6.017)	(6.384)	(0.068)
N	34,581	18,324	16,257	34,581

Notes: This table shows the total mean, pre-reform period mean, post-reform period mean and the difference between post- and pre-reform period mean of our covariates using our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.2 Continuity of the Running Variable

Figure C.2: Histogram of the Running Variable



Notes: This figure plots the number of UI entrants at each date (centered around the cutoff) using our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). As there are many more entrants at the beginning of each month, it shows that UI entry is systematic. Nonetheless, we cannot visually detect any evidence of *precise* manipulation. The histogram is constructed using the `rddensity` routine in Stata (Cattaneo et al., 2018).

Source: Authors' calculations are based on MCVL 2005-2018 data.

C.3 Balancing Tests

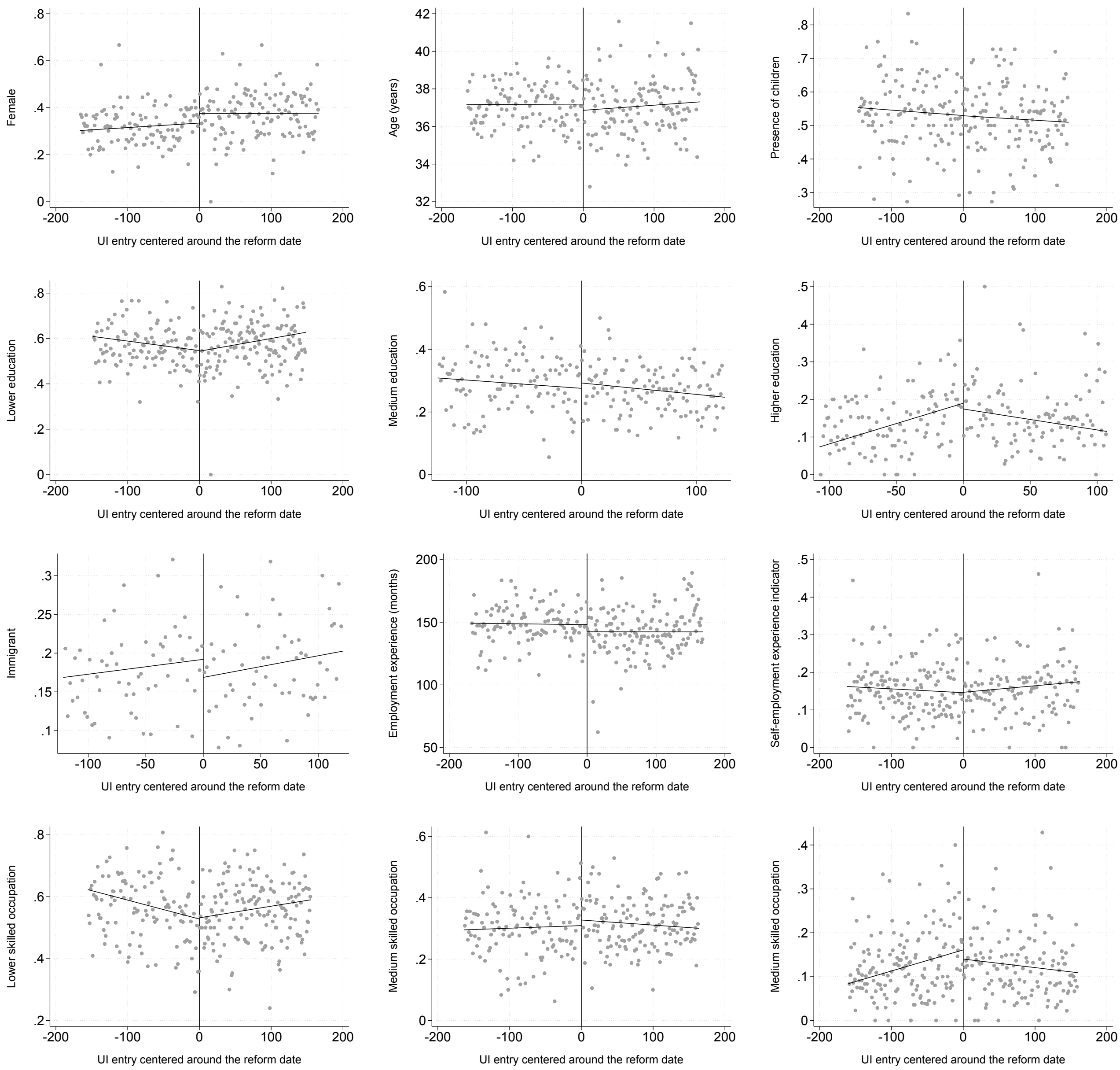
Table C.4: Balancing Table (linear, including all covariates)

Outcome Variable	RD Estimate	Rel. Change	s.e.	p-value	Bandwidth	N Left	N Right
Female	0.028	7.9%	0.021	0.221	156.474	5008	5296
Age (years)	0.142	0.4%	0.209	0.577	142.852	4556	4884
Lower education	-0.006	-1.0%	0.023	0.556	136.971	4376	4564
Medium education	0.021	7.6%	0.024	0.219	121.588	3737	4084
Higher education	-0.015	-11.9%	0.015	0.196	98.929	3026	3278
Presence of children	0.009	1.7%	0.022	0.547	162.667	5235	5529
Immigrant	-0.032	-15.7%	0.015	0.016	143.763	4583	4909
Employment experience (months)	-2.769	-2.0%	1.855	0.176	142.670	4556	4884
Self-employment experience indicator	0.002	1.3%	0.014	0.829	228.472	7705	7749
ln(real monthly average earnings)	-0.002	-0.0%	0.025	0.960	213.113	7151	7358
Low skilled occupation	0.021	3.6%	0.019	0.172	143.468	4583	4909
Medium skilled occupation	0.004	1.3%	0.026	0.942	152.837	4882	5152
High skilled occupation	-0.026	-23.0%	0.020	0.114	132.732	4112	4454
Permanent contract	-0.003	-0.4%	0.024	0.883	122.226	3767	4107
Agriculture, extraction, primary manufacturing	0.001	1.6%	0.011	0.752	168.183	5577	5660
Manufacturing and utilities	-0.011	-9.6%	0.015	0.405	232.575	7788	7975
Construction	0.011	5.4%	0.025	0.555	132.551	4112	4454
Trade	-0.008	-4.0%	0.025	0.776	138.987	4424	4611
Transport and storage	0.012	22.2%	0.012	0.188	158.515	5073	5364
Accommodation and food services	-0.013	-12.1%	0.019	0.527	107.935	3350	3484
I&C, finance, real estate, and scientific services	0.014	14.3%	0.014	0.195	135.240	4206	4542
Education, health, social, and other services	0.013	8.2%	0.018	0.394	135.015	4206	4542
PBD (months)	-0.010	-0.1%	0.250	0.909	200.063	6816	6713
Local unemployment rate	-0.035	-0.2%	0.558	0.865	130.696	4014	4398

Notes: The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We use a linear version of the running variable and include all covariates. Relative changes are calculated based on the pre-reform average values illustrated in Appendix Table C.3. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).

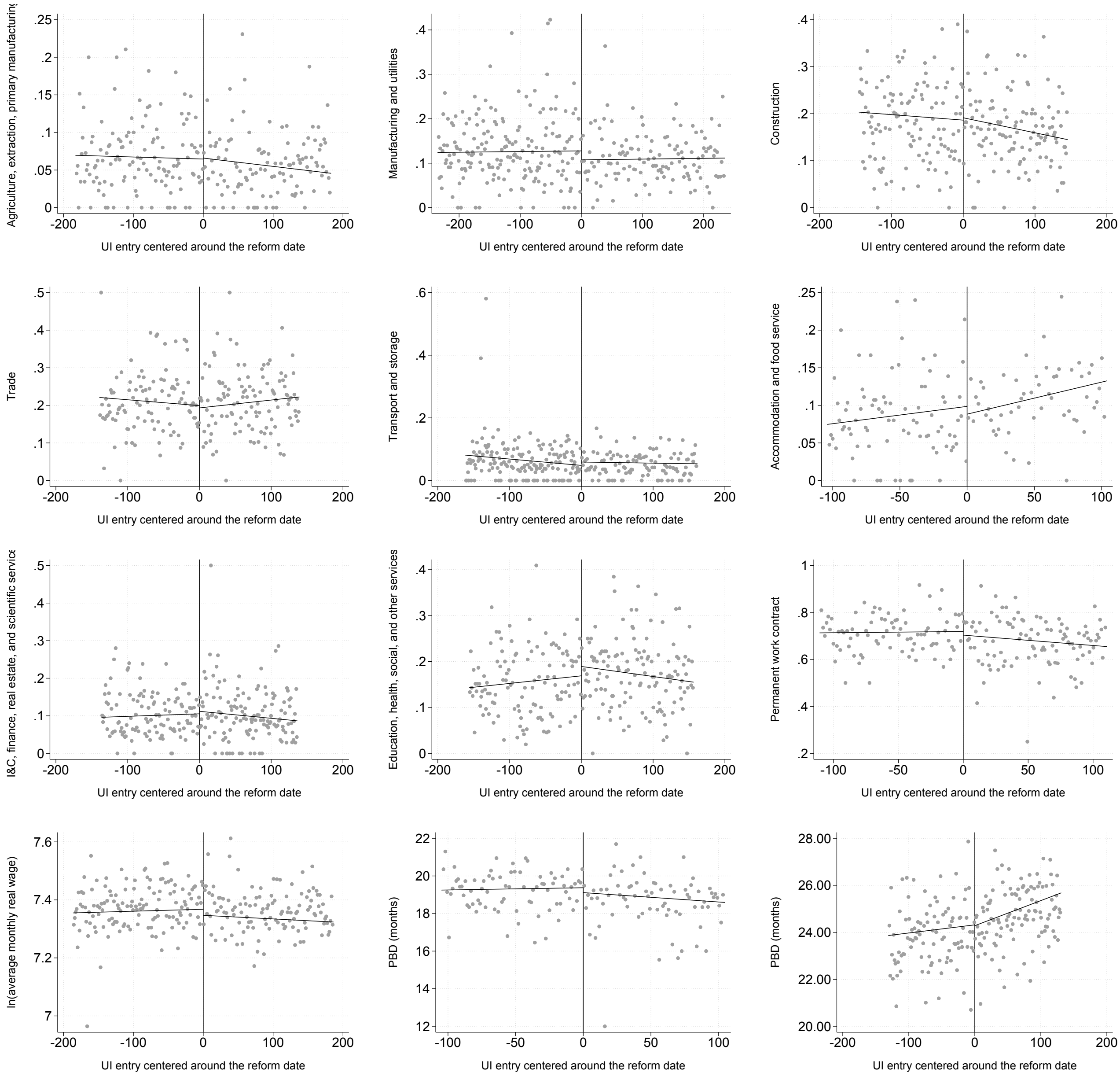
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure C.3: Balanced Covariates (linear)



Notes: These figures illustrate that our covariates are balanced around the vicinity of the cutoff date. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure C.4: Balanced Covariates cont'd (linear)



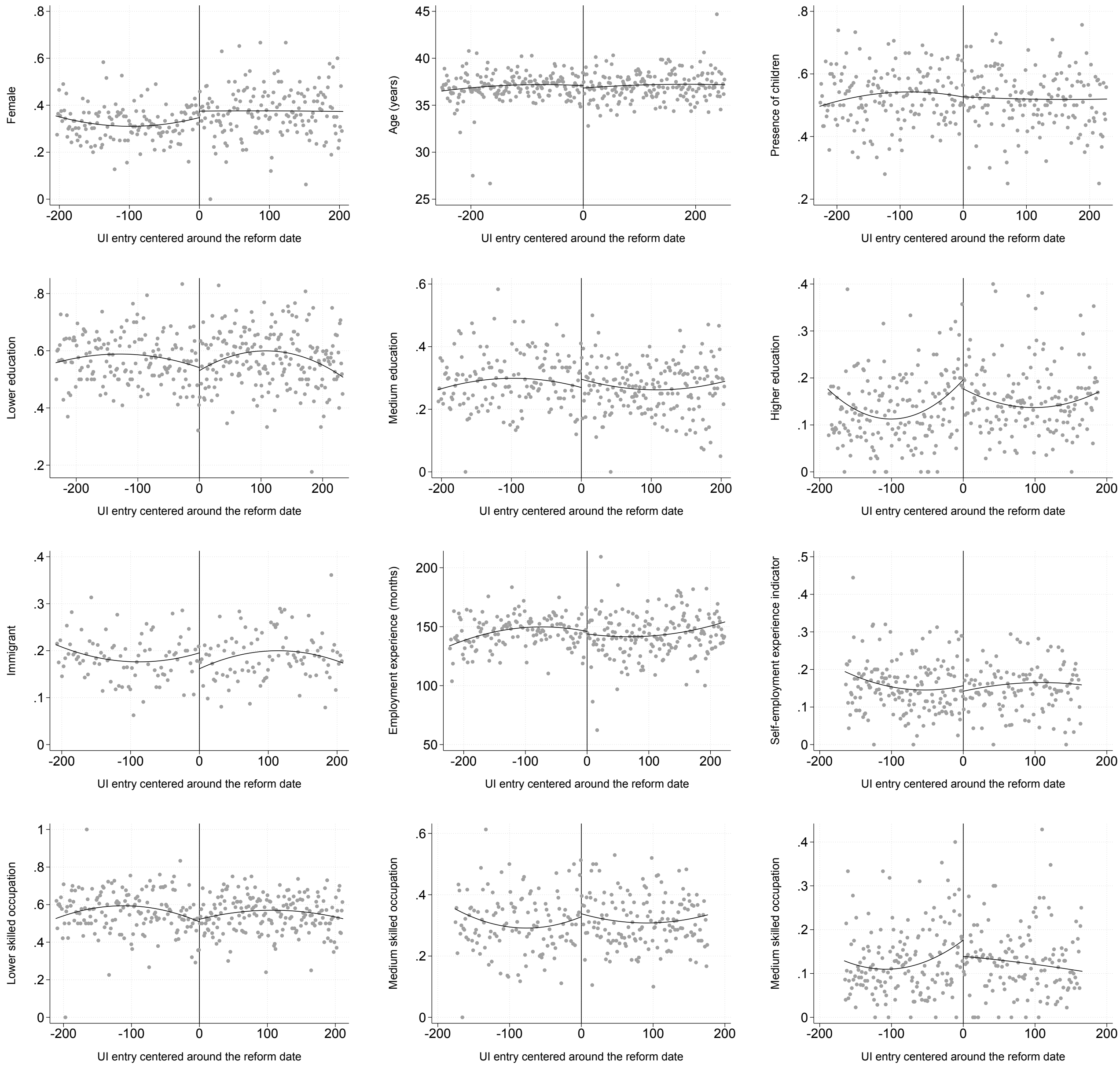
Notes: These figures illustrate that our covariates are balanced around the vicinity of the cutoff date. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.5: Balancing Table (quadratic, including all covariates)

Outcome Variable	RD Estimate	Rel. Change	s.e.	p-value	Bandwidth	N Left	N Right
Female	0.022	6.2%	0.024	0.398	222.532	7357	7597
Age (years)	0.097	0.3%	0.219	0.774	239.356	7973	8172
Lower education	-0.014	-2.3%	0.025	0.437	238.878	7927	8145
Medium education	0.030	10.8%	0.028	0.185	189.785	6229	6474
Higher education	-0.019	-15.1%	0.018	0.204	141.928	4525	4854
Presence of children	0.019	3.6%	0.028	0.408	198.635	6765	6693
Immigrant	-0.038	-18.6%	0.017	0.016	188.079	6217	6455
Employment experience (months)	-2.647	-1.9%	2.004	0.205	211.974	7094	7287
Self-employment experience indicator	-0.028	-17.7%	0.024	0.146	147.013	4708	5017
ln(real monthly average earnings)	0.013	0.2%	0.032	0.577	206.944	6973	7143
Low skilled occupation	0.039	6.7%	0.024	0.060	165.035	5540	5587
Medium skilled occupation	-0.001	-0.3%	0.033	0.849	176.030	5798	6116
High skilled occupation	-0.031	-27.4%	0.023	0.131	200.958	6816	6713
Permanent contract	0.003	0.4%	0.025	0.745	203.368	6860	6995
Agriculture, extraction, primary manufacturing	0.004	6.3%	0.013	0.652	215.952	7205	7395
Manufacturing and utilities	-0.029	-25.2%	0.023	0.147	178.529	5863	6171
Construction	0.012	5.9%	0.029	0.691	195.773	6405	6636
Trade	-0.009	-4.5%	0.026	0.738	238.400	7927	8145
Transport and storage	0.017	31.5%	0.015	0.222	223.279	7387	7623
Accommodation and food services	-0.014	-13.0%	0.021	0.662	165.099	5540	5587
I&C, finance, real estate, and scientific services	0.019	19.4%	0.016	0.232	187.092	6180	6418
Education, health, social, and other services	0.016	10.1%	0.021	0.412	202.717	6845	6958
PBD (months)	-0.012	-0.1%	0.325	0.959	221.552	7327	7583
Local unemployment rate	0.108	0.5%	0.552	0.736	245.730	8169	8349

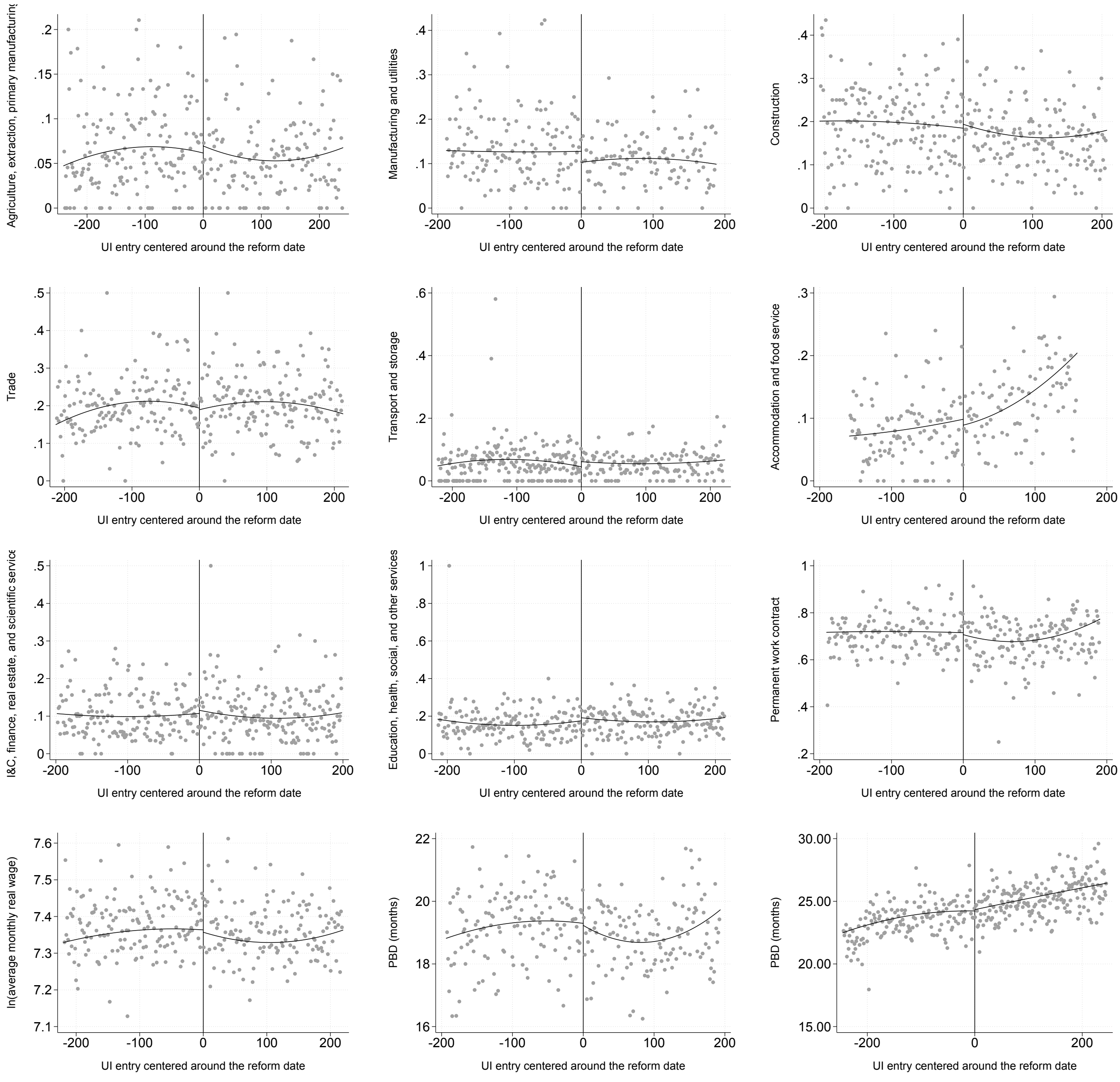
Notes: The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We use a quadratic version of the running variable and include all covariates. Relative changes are calculated based on the pre-reform average values illustrated in Appendix Table C.3. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure C.5: Balanced Covariates (quadratic)



Notes: These figures illustrate that our covariates are balanced around the vicinity of the cutoff date. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure C.6: Balanced Covariates cont'd (quadratic)



Notes: These figures illustrate that our covariates are balanced around the vicinity of the cutoff date. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.4 Results

Table C.6: Effect on the Probability of Exiting into Self-Employment

Outcome Variable	RD Estimate	Rel. Change	s.e.	p-value	Polynomial	Covs.	Bandwidth	N Left	N Right
(A) SE within 90 days	-0.019	-34.5%	0.012	0.113	linear		193.654	6512	6748
	-0.019	-34.5%	0.011	0.082	linear	✓	197.335	6743	6670
	-0.021	-38.2%	0.015	0.134	quadratic		218.016	7445	7682
	-0.019	-34.5%	0.013	0.171	quadratic	✓	257.573	8675	8652
	-0.022	-40.0%	0.016	0.120	cubic		264.809	9105	9125
	-0.020	-36.4%	0.016	0.182	cubic	✓	249.984	8287	8454
(B) SE within 180 days	-0.025	-32.5%	0.014	0.071	linear		192.417	6467	6716
	-0.022	-28.6%	0.012	0.067	linear	✓	207.507	7003	7174
	-0.028	-36.4%	0.018	0.088	quadratic		190.724	6400	6680
	-0.027	-35.1%	0.016	0.085	quadratic	✓	202.732	6845	6958
	-0.036	-46.8%	0.019	0.047	cubic		226.785	7686	7905
	-0.032	-41.6%	0.019	0.062	cubic	✓	210.880	7054	7260
(C) SE within 360 days	-0.029	-30.2%	0.017	0.085	linear		181.890	6118	6450
	-0.025	-26.0%	0.016	0.107	linear	✓	186.304	6142	6392
	-0.043	-44.8%	0.022	0.028	quadratic		166.751	5676	5758
	-0.035	-36.5%	0.021	0.060	quadratic	✓	178.160	5863	6171
	-0.048	-50.0%	0.023	0.018	cubic		264.475	9105	9125
	-0.044	-45.8%	0.023	0.030	cubic	✓	239.670	7973	8172
(D) SE within 720 days	-0.024	-20.9%	0.018	0.144	linear		156.627	5126	5436
	-0.020	-17.4%	0.017	0.198	linear	✓	155.774	4978	5264
	-0.049	-42.6%	0.023	0.018	quadratic		150.458	4926	5244
	-0.036	-31.3%	0.023	0.069	quadratic	✓	160.135	5126	5483
	-0.058	-50.4%	0.026	0.011	cubic		203.958	7033	7182
	-0.047	-40.9%	0.026	0.036	cubic	✓	203.165	6860	6995

Notes: The outcome variables are binary and indicate whether the person transitioned into a self-employment spell within the first 90, 180, 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. Relative changes are calculated based on the pre-reform average exit probabilities illustrated in Appendix Table C.2. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). An overview of the results for comparison purposes is provided in Table 1.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.7: Effect on the Probability of Exiting into Employment

Outcome variable	RD Estimate	Rel. Change	s.e.	p-value	Polynomial	Covs.	Bandwidth	N Left	N Right
(A) E within 90 days	0.040	13.6%	0.029	0.072	linear		154.605	5051	5373
	0.047	16.0%	0.029	0.042	linear	✓	151.370	4843	5137
	0.091	31.0%	0.042	0.013	quadratic		155.519	5096	5403
	0.094	32.0%	0.041	0.009	quadratic	✓	156.595	5008	5296
	0.096	32.7%	0.042	0.012	cubic		276.448	9463	9425
	0.095	32.3%	0.041	0.012	cubic	✓	280.747	9298	9299
(B) E within 180 days	0.062	13.5%	0.035	0.035	linear		92.107	2897	3151
	0.066	14.4%	0.035	0.025	linear	✓	91.028	2803	3038
	0.076	16.6%	0.042	0.033	quadratic		150.063	4926	5244
	0.080	17.5%	0.041	0.027	quadratic	✓	150.738	4813	5109
	0.085	18.6%	0.044	0.025	cubic		235.693	8071	8292
	0.086	18.8%	0.044	0.024	cubic	✓	234.536	7829	8057
(C) E within 360 days	0.043	6.8%	0.032	0.159	linear		132.275	4206	4569
	0.044	7.0%	0.033	0.165	linear	✓	130.985	4010	4392
	0.047	7.5%	0.040	0.186	quadratic		171.657	5810	6150
	0.045	7.2%	0.040	0.223	quadratic	✓	176.446	5798	6116
	0.055	8.8%	0.043	0.139	cubic		237.828	8127	8345
	0.049	7.8%	0.045	0.239	cubic	✓	233.850	7807	7994
(D) E within 720 days	0.025	3.1%	0.020	0.190	linear		144.147	4724	5082
	0.022	2.8%	0.020	0.250	linear	✓	138.955	4424	4611
	0.040	5.0%	0.028	0.107	quadratic		153.514	5033	5343
	0.037	4.7%	0.027	0.130	quadratic	✓	148.689	4748	5047
	0.046	5.8%	0.033	0.149	cubic		203.087	7033	7182
	0.042	5.3%	0.032	0.163	cubic	✓	194.632	6375	6591

Notes: The outcome variables are binary and indicate whether the person transitioned into an employment spell within the first 90, 180, 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. Relative changes are calculated based on the pre-reform average exit probabilities illustrated in Appendix Table C.2. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). An overview of the results for comparison purposes is provided in Table 1.
Source: Authors' calculations are based on MCVL 2005-2018 data.

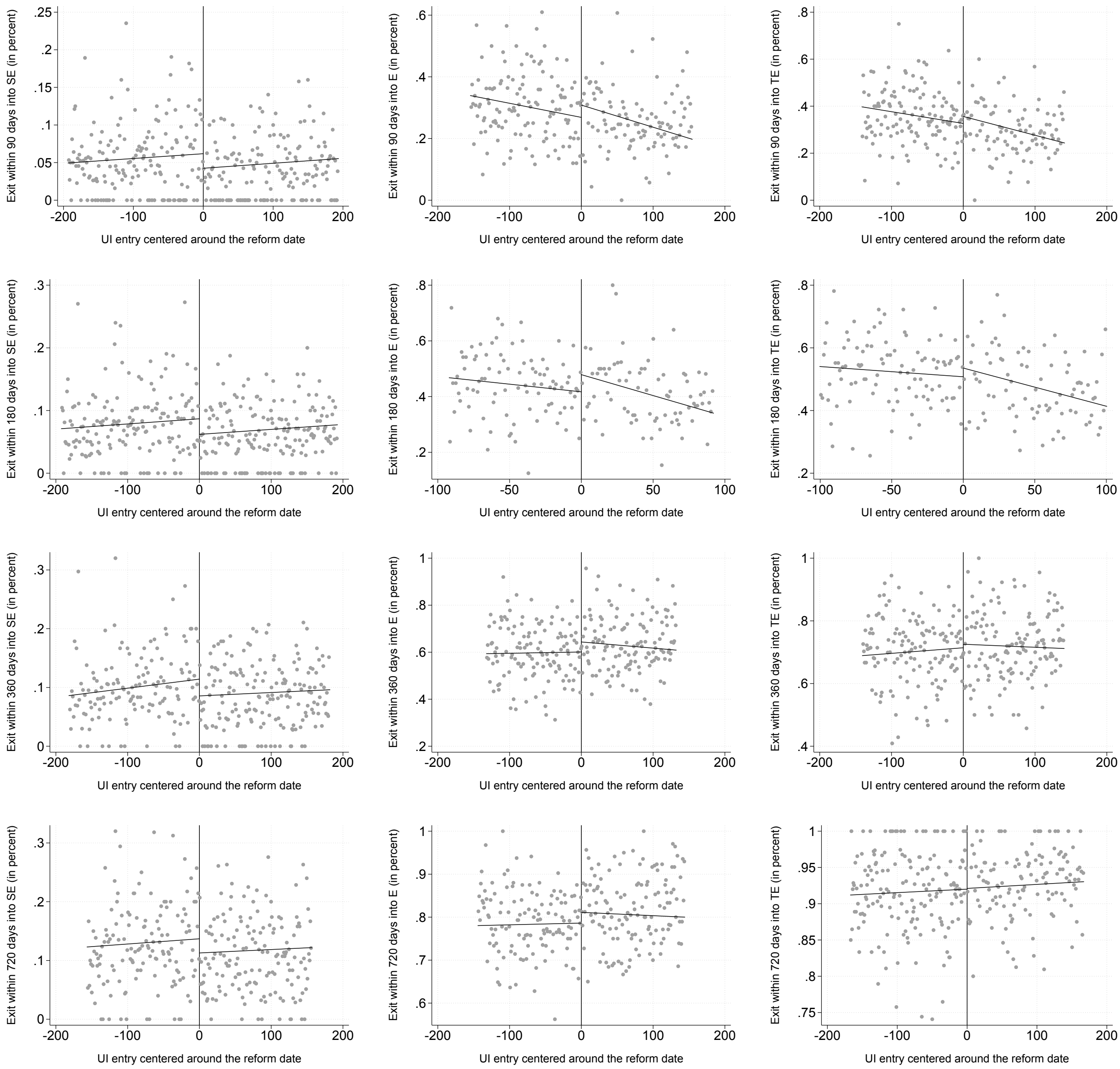
Table C.8: Effect on the Probability of Exiting into Self-Employment or Employment

Outcome Variable	RD Estimate	Rel. Change	s.e.	p-value	Polynomial	Covs.	Bandwidth	N Left	N Right
(A) SE or E within 90 days	0.030	8.6%	0.032	0.185	linear		141.196	4632	4980
	0.034	9.7%	0.032	0.146	linear	✓	142.328	4556	4884
	0.070	20.1%	0.043	0.056	quadratic		160.199	5252	5626
	0.076	21.8%	0.042	0.039	quadratic	✓	156.880	5008	5296
	0.070	20.1%	0.042	0.069	cubic		289.288	10045	9718
	0.074	21.2%	0.042	0.057	cubic	✓	284.684	9427	9388
(B) SE or E within 180 days	0.028	5.2%	0.038	0.275	linear		100.351	3132	3406
	0.036	6.7%	0.038	0.202	linear	✓	101.980	3097	3342
	0.039	7.3%	0.044	0.242	quadratic		159.914	5229	5559
	0.047	8.8%	0.044	0.187	quadratic	✓	159.225	5104	5416
	0.028	5.2%	0.043	0.383	cubic		295.716	10190	9940
	0.033	6.2%	0.043	0.350	cubic	✓	300.297	9983	9894
(C) SE or E within 360 days	0.011	1.5%	0.034	0.799	linear		140.648	4603	4949
	0.016	2.2%	0.035	0.699	linear	✓	138.587	4424	4611
	0.007	1.0%	0.037	0.838	quadratic		213.007	7337	7552
	0.010	1.4%	0.037	0.849	quadratic	✓	231.391	7759	7946
	0.006	0.8%	0.042	0.855	cubic		252.160	8591	8751
	0.009	1.2%	0.043	0.830	cubic	✓	257.047	8675	8652
(D) SE or E within 720 days	0.001	0.1%	0.012	0.854	linear		167.029	5696	5792
	0.002	0.2%	0.012	0.920	linear	✓	141.956	4525	4854
	-0.001	-0.1%	0.015	0.930	quadratic		220.254	7495	7750
	-0.001	-0.1%	0.014	0.956	quadratic	✓	188.296	6217	6455
	-0.019	-2.1%	0.020	0.236	cubic		176.952	5939	6283
	-0.013	-1.4%	0.018	0.352	cubic	✓	184.259	6066	6335

Notes: The outcome variables are binary and indicate whether the person transitioned into self-employment or employment within the first 90, 180, 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. Relative changes are calculated based on the pre-reform average exit probabilities illustrated in Appendix Table C.2. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). An overview of the results for comparison purposes is provided in Table 1.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure C.7: RD Plots by UI Exit State (linear)

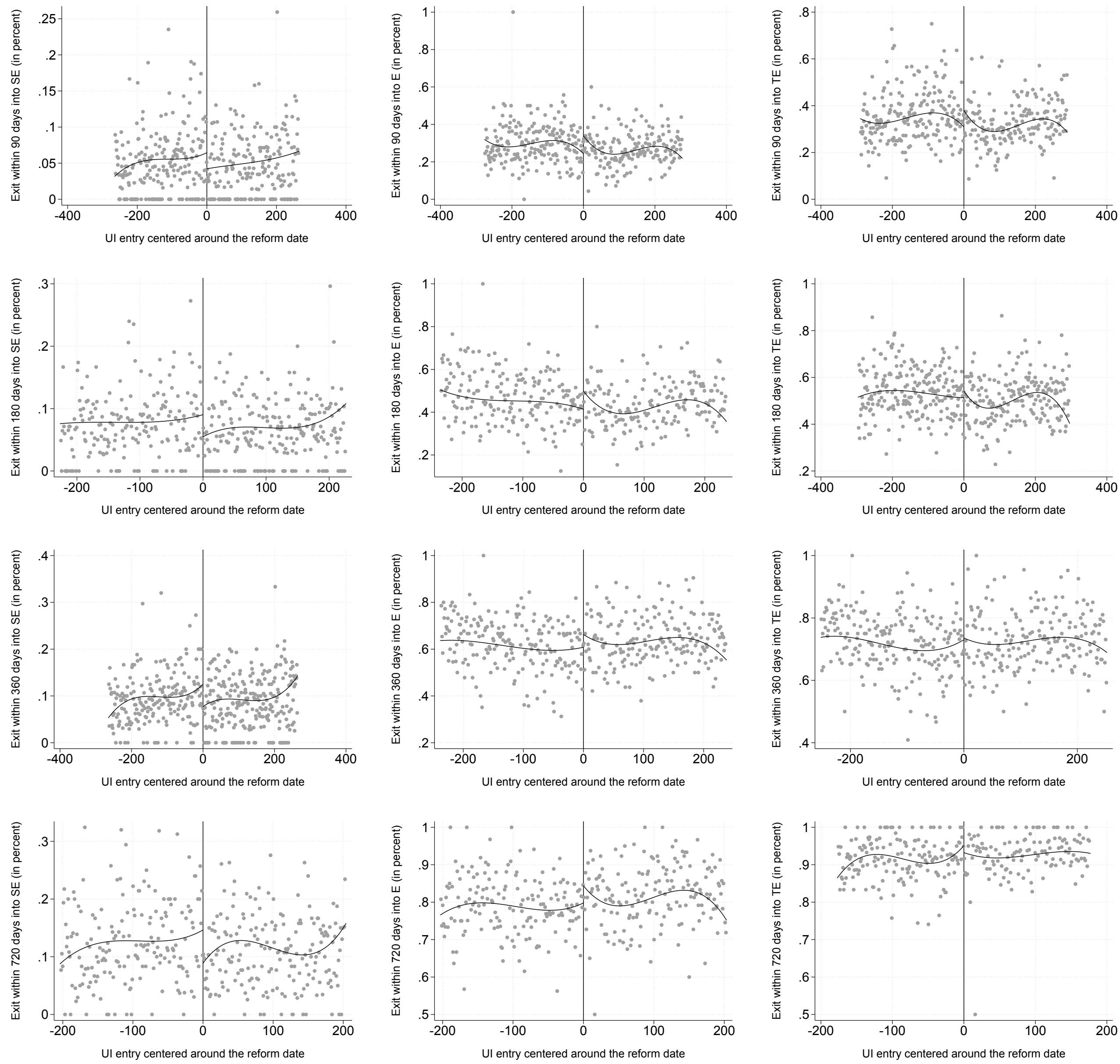
(a) Self-Employment **(b)** Employment **(c)** Self-Employment or Employment



Notes: These figures illustrate the estimated linear reform effect on different UI exit states without covariates using MSE-optimal bandwidths as suggested by Calonico et al. (2014). We use the `rdrobust` routine in Stata to select the MSE-optimal bandwidth and the `rdplot` routine to generate the graphs. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). See Figure 6 for the main quadratic specification.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure C.8: RD Plots by UI Exit State (cubic)

(a) Self-Employment **(b)** Employment **(c)** Self-Employment or Employment



Notes: These figures illustrate the estimated cubic reform effect on different UI exit states without covariates using MSE-optimal bandwidths as suggested by Calonico et al. (2014). We use the `rdrobust` routine in Stata to select the MSE-optimal bandwidth and the `rdplot` routine to generate the graphs. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions). See Figure 6 for the main quadratic specification.
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.5 Placebo Tests

Table C.9: Placebo Test - Individuals whose RR did not Drop after the Reform

	(1)	(2)	(3)	(4)	(5)	(6)
(A): SE within 360 days						
RD Estimate	-0.007	-0.040	-0.032	0.002	-0.034	-0.035
s.e.	0.051	0.067	0.068	0.049	0.067	0.070
p-value	0.784	0.405	0.504	0.945	0.467	0.477
Bandwidth	193	208.9	339	199.3	206.7	323.9
N left	568	654	1,207	614	638	1,131
N right	540	600	990	544	590	926
(B): SE within 720 days						
RD Estimate	-0.005	-0.019	-0.020	0.001	-0.012	-0.017
s.e.	0.064	0.080	0.082	0.062	0.076	0.081
p-value	0.901	0.689	0.691	0.953	0.768	0.754
Bandwidth	185.4	217.4	334.4	185.2	225	337.2
N left	534	684	1,188	526	699	1,174
N right	512	621	978	507	634	976
(C): E within 360 days						
RD Estimate	0.006	-0.001	-0.015	0.018	0.012	-0.007
s.e.	0.071	0.076	0.089	0.073	0.078	0.090
p-value	0.904	0.831	0.776	0.926	0.964	0.830
Bandwidth	150.6	254.8	320.1	149.8	255.7	325.6
N left	412	842	1,142	401	833	1,133
N right	424	733	919	420	728	934
(D): E within 720 days						
RD Estimate	-0.041	-0.051	-0.048	-0.036	-0.042	-0.068
s.e.	0.063	0.074	0.105	0.067	0.087	0.113
p-value	0.443	0.460	0.634	0.533	0.636	0.486
Bandwidth	217.2	299.8	250.2	211.6	248.8	241.8
N left	684	1,050	823	649	795	768
N right	621	879	722	601	709	698
(E): SE or E within 360 days						
RD Estimate	-0.007	-0.032	-0.056	0.012	-0.008	-0.042
s.e.	0.081	0.096	0.108	0.082	0.097	0.111
p-value	0.726	0.549	0.462	0.940	0.730	0.545
Bandwidth	169.4	236.4	324.5	169.7	243.4	324.5
N left	482	764	1,153	475	783	1,132
N right	455	693	939	451	702	931
(F): SE or E within 720 days						
RD Estimate	-0.049	-0.081	-0.107	-0.034	-0.094	-0.122
s.e.	0.045	0.055	0.065	0.044	0.058	0.069
p-value	0.251	0.093	0.062	0.359	0.058	0.050
Bandwidth	178.8	187.9	211.5	195.3	162.5	196.6
N left	510	545	662	563	444	605
N right	495	519	608	538	444	540
Polynomial	linear	quadratic	cubic	linear	quadratic	cubic
Covariates				✓	✓	✓

Notes: We run this placebo test using workers unaffected by the RR drop because they either hit the ceiling or the floor of UI benefits. We cannot conduct this test for exit state outcomes measured within the first 90 or 180 days of the unemployment spell because we have too few observations for this specific group of people. The outcome variables are binary and indicate whether the person transitioned into an (self-)employment spell within the first 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.10: Placebo Test for Self-Employment - Fictive Reform Date (July 15 2013)

	(1)	(2)	(3)	(4)	(5)	(6)
(A): SE within 90 days						
RD Estimate	0.044	0.041	0.041	0.042	0.045	0.045
s.e.	0.025	0.031	0.035	0.024	0.028	0.031
p-value	0.059	0.168	0.226	0.065	0.084	0.123
Bandwidth	61.7	48.09	64.59	61.62	46.67	65.71
N left	1,608	1,317	1,659	1,577	1,247	1,734
N right	1,604	1,331	1,663	1,574	1,177	1,649
(B): SE within 180 days						
RD Estimate	0.028	0.012	0.010	0.016	0.007	0.007
s.e.	0.031	0.054	0.063	0.031	0.047	0.053
p-value	0.381	0.852	0.890	0.673	0.896	0.895
Bandwidth	76.96	62.18	77.62	64.02	57.63	78.49
N left	2,089	1,627	2,106	1,628	1,487	2,089
N right	1,895	1,617	1,910	1,632	1,495	2,033
(C): SE within 360 days						
RD Estimate	0.004	-0.013	-0.017	-0.006	-0.013	-0.018
s.e.	0.031	0.048	0.054	0.030	0.041	0.046
p-value	0.993	0.724	0.731	0.725	0.716	0.684
Bandwidth	60.73	56.28	70.4	54.71	53.25	69.65
N left	1,592	1,503	1,872	1,429	1,408	1,818
N right	1,582	1,494	1,779	1,447	1,415	1,735
(D): SE within 720 days						
RD Estimate	0.016	-0.013	-0.024	0.006	-0.016	-0.025
s.e.	0.029	0.044	0.049	0.028	0.039	0.043
p-value	0.585	0.634	0.590	0.860	0.575	0.525
Bandwidth	59.96	51.52	65.76	56.83	49.3	64.09
N left	1,554	1,378	1,766	1,476	1,310	1,628
N right	1,563	1,381	1,681	1,469	1,325	1,632
Polynomial	linear	quadratic	cubic	linear	quadratic	cubic
Covariates				✓	✓	✓

Notes: This placebo test uses a fictive cutoff date (July 15 2013) to test whether the estimated reform effects are driven by seasonality. We drop observations before the actual cutoff date (July 15 2012) to avoid bias from the true reform effect. The outcome variables are binary and indicate whether the person transitioned into a self-employment spell within the first 90, 180, 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.11: Placebo Test for Employment - Fictive Reform Date (July 15 2013)

	(1)	(2)	(3)	(4)	(5)	(6)
(A): E within 90 days						
RD Estimate	0.019	0.006	-0.016	0.027	0.014	-0.001
s.e.	0.040	0.050	0.070	0.042	0.055	0.072
p-value	0.803	0.996	0.786	0.619	0.865	0.948
Bandwidth	61.59	87.59	83.55	60.75	78.36	84.05
N left	1,608	2,342	2,232	1,561	2,089	2,223
N right	1,604	2,321	2,215	1,553	2,033	2,195
(B): E within 180 days						
RD Estimate	-0.045	-0.037	-0.053	-0.039	-0.033	-0.034
s.e.	0.059	0.058	0.086	0.059	0.065	0.086
p-value	0.327	0.371	0.607	0.401	0.531	0.792
Bandwidth	46.21	93.88	79.59	44.45	78.89	77.3
N left	1,265	2,485	2,148	1,228	2,089	2,070
N right	1,196	2,502	2,105	1,154	2,033	1,874
(C): E within 360 days						
RD Estimate	-0.022	-0.047	-0.058	-0.011	-0.023	-0.031
s.e.	0.036	0.054	0.069	0.038	0.045	0.058
p-value	0.461	0.298	0.444	0.675	0.516	0.656
Bandwidth	73.68	71.3	72.27	56.43	72.94	69.79
N left	1,941	1,889	1,915	1,476	1,880	1,818
N right	1,833	1,795	1,811	1,469	1,777	1,735
(D): E within 720 days						
RD Estimate	0.002	0.002	0.003	0.011	0.009	0.007
s.e.	0.036	0.056	0.063	0.031	0.052	0.058
p-value	0.964	0.969	0.976	0.688	0.880	0.975
Bandwidth	69.51	54.58	69	79.37	54.28	66.41
N left	1,852	1,454	1,852	2,111	1,429	1,764
N right	1,767	1,469	1,767	2,064	1,447	1,668
Polynomial	linear	quadratic	cubic	linear	quadratic	cubic
Covariates				✓	✓	✓

Notes: This placebo test uses a fictive cutoff date (July 15 2013) to test whether the estimated reform effects are driven by seasonality. We drop observations before the actual cutoff date (July 15 2012) to avoid bias from the true reform effect. The outcome variables are binary and indicate whether the person transitioned into an employment spell within the first 90, 180, 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.12: Placebo Test for Self-Employment or Employment - Fictive Reform Date (July 15 2013)

	(1)	(2)	(3)	(4)	(5)	(6)
(A): SE or E within 90 days						
RD Estimate	0.066	0.023	0.022	0.069	0.039	0.038
s.e.	0.046	0.066	0.079	0.048	0.068	0.078
p-value	0.194	0.823	0.738	0.171	0.620	0.600
Bandwidth	66.49	57.94	71.79	61.81	55.32	74.8
N left	1,796	1,514	1,889	1,577	1,456	1,928
N right	1,700	1,521	1,795	1,574	1,459	1,822
(B): SE or E within 180 days						
RD Estimate	-0.031	-0.042	-0.043	-0.030	-0.036	-0.034
s.e.	0.068	0.083	0.087	0.067	0.086	0.086
p-value	0.483	0.513	0.624	0.506	0.687	0.731
Bandwidth	43.44	67.79	101.9	42.82	60.07	99.54
N left	1,114	1,810	2,691	1,079	1,561	2,597
N right	1,157	1,721	2,683	1,127	1,553	2,586
(C): SE or E within 360 days						
RD Estimate	-0.041	-0.069	-0.076	-0.042	-0.048	-0.054
s.e.	0.029	0.035	0.036	0.031	0.035	0.036
p-value	0.078	0.024	0.029	0.092	0.114	0.125
Bandwidth	47.45	56.82	86	34.97	57.14	83.74
N left	1,298	1,503	2,314	902	1,487	2,193
N right	1,197	1,494	2,295	955	1,495	2,170
(D): SE or E within 720 days						
RD Estimate	0.021	0.004	-0.016	0.011	0.006	-0.015
s.e.	0.019	0.023	0.025	0.021	0.023	0.026
p-value	0.322	0.968	0.400	0.780	0.963	0.428
Bandwidth	71.96	73.75	78.99	46.84	75.36	73.75
N left	1,889	1,941	2,126	1,247	2,038	1,906
N right	1,795	1,833	2,074	1,177	1,847	1,799
Polynomial	linear	quadratic	cubic	linear	quadratic	cubic
Covariates				✓	✓	✓

Notes: This placebo test uses a fictive cutoff date (July 15 2013) to test whether the estimated reform effects are driven by seasonality. We drop observations before the actual cutoff date (July 15 2012) to avoid bias from the true reform effect. The outcome variables are binary and indicate whether the person transitioned into self-employment or employment within the first 90, 180, 360 or 720 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.6 Overestimation Bias if Self-Employment is Excluded

In Table C.13 we demonstrate the overestimation bias, which arises if self-employed workers are excluded from the sample. For reasons of comparability, the results are based on a parametric (global) estimation approach using different bandwidths between 150 and 170 days. Our findings are very similar, however, if we use a non-parametric approach (Appendix Table C.14).

As can be seen from our illustration of extensive margin outcome variables in Figure 4, the exclusion of individuals who transition from unemployment into self-employment causes columns (2) and (6) to be equivalent, the same as columns (4) and (8).

When estimating the causal reform effect on the employment probability (E=1) in columns (1)-(4), counterfactual outcomes are to become self-employed (SE=0) or to stay unemployed (UE=0). Through the exclusion of self-employed workers from our sample, the counterfactual outcome is restricted to individuals who stay unemployed. In other words, there are fewer individuals with an outcome variable which is equal to zero. Based on this sample selection criterion, we find that the reform effect on the probability of exiting from unemployment into employment within 90 days (column 2) is slightly overestimated. The estimated effects on the probability of exiting into employment within 180 days (columns 3 and 4) are similar, regardless of the inclusion or exclusion of self-employment.

When estimating the causal reform effect on the probability of exiting into self-employment or employment (SE=1, E=1) in columns (5)-(8), the counterfactual outcome is unemployment (UE=0). If self-employment is excluded, our sample contains fewer individuals with an outcome variable

Table C.13: Parametric Approach

	Employment E=1, (SE=0), UE=0				Self-Employment or Employment E=1, (SE=1), UE=0				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	Bandwidth
RD Estimate	0.094**	0.101*	0.065	0.065	0.078*	0.101*	0.043	0.065	
	(0.035)	(0.040)	(0.034)	(0.038)	(0.038)	(0.040)	(0.038)	(0.038)	150
Rel. Change	32.0%	34.4%	14.2%	14.2%	22.3%	28.9%	8.0%	12.1%	
N	9,922	8,607	9,922	8,607	9,922	8,607	9,922	8,607	
RD Estimate	0.079*	0.086*	0.057	0.058	0.064	0.086*	0.038	0.058	
	(0.034)	(0.038)	(0.033)	(0.037)	(0.037)	(0.038)	(0.037)	(0.037)	160
Rel. Change	26.9%	29.3%	12.4%	12.7%	18.3%	24.6%	7.1%	10.8%	
N	10,609	9,218	10,609	9,218	10,609	9,218	10,609	9,218	
RD Estimate	0.079*	0.084*	0.045	0.042	0.059	0.084*	0.024	0.042	
	(0.033)	(0.037)	(0.034)	(0.037)	(0.035)	(0.037)	(0.037)	(0.037)	170
Rel. Change	26.9%	28.6%	9.8%	9.2%	16.9%	24.1%	4.5%	7.8%	
N	11,600	10,073	11,600	10,073	11,600	10,073	11,600	10,073	
Self-Employment	included	excluded	included	excluded	included	excluded	included	excluded	
Exit within...	90 days	90 days	180 days	180 days	90 days	90 days	180 days	180 days	

Notes: This table demonstrates the overestimation bias which arises if self-employed workers are excluded from the sample. The outcome variable is binary and indicates whether the person transitioned into a (self-)employment spell within the first 90 or 180 days of unemployment. We use a quadratic version of the running variable and include all covariates. Relative changes are calculated based on the pre-reform average probabilities illustrated in Appendix Table C.2. Standard errors are clustered at the UI entry date level (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$). We use our RDD estimation sample (detailed sample restrictions in Section 4.1).
Source: Authors' calculations are based on MCVL 2005-2018 data.

which is equal to one. Since the startup rate is negatively affected by the reform (compare Section 5.1), estimating the joint effect on self-employment and employment yields much smaller point estimates (columns 5 and 7) compared to the case when self-employment is excluded (columns 6 and 8). Consequently, under heterogeneous treatment effects the isolated look at the job-finding rate does not accurately represent the reform's general employment effects (on self-employment and employment).

Table C.14: Non-Parametric Approach

Outcome variable	RD Estimate	Rel. Change	s.e.	p-value	Polynomial	Covs.	Bandwidth	N Left	N Right
(A): E within 90 days									
Self-Employment included	0.094	32.0%	0.041	0.009	quadratic	✓	156.595	5,008	5,296
Self-Employment excluded	0.096	32.7%	0.044	0.015	quadratic	✓	161.270	4,444	4,807
(B): E within 180 days									
Self-Employment included	0.080	17.5%	0.041	0.027	quadratic	✓	150.738	4,813	5,109
Self-Employment excluded	0.069	15.1%	0.043	0.060	quadratic	✓	161.872	4,444	4,807
(C): SE or E within 90 days									
Self-Employment included	0.076	21.8%	0.042	0.039	quadratic	✓	156.880	5,008	5,296
Self-Employment excluded	0.096	27.5%	0.044	0.015	quadratic	✓	161.270	4,444	4,807
(D): SE or E within 180 days									
Self-Employment included	0.047	8.8%	0.044	0.187	quadratic	✓	159.225	5,104	5,416
Self-Employment excluded	0.069	12.9%	0.043	0.060	quadratic	✓	161.872	4,444	4,807

Notes: This table demonstrates the overestimation bias which arises if self-employed workers are excluded from the sample. The outcome variables are binary and indicate whether the person transitioned into a (self-)employment spell within the first 90 or 180 days of unemployment, respectively. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. Relative changes are calculated based on the pre-reform average exit probabilities illustrated in Appendix Table C.2. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.7 Subgroup Analysis of Extensive Margin Effects

Table C.15: Subgroup Analysis - Self-Employment within 360 days (including covariates)

Sub Sample	RD Estimate	s.e.	p-value	Polynomial	Bandwidth	N Left	N Right
	-0.036	0.019	0.057	linear	237.226	3858	3888
Age $\leq$ median(age)	-0.051	0.031	0.064	quadratic	174.601	2768	2888
	-0.062	0.033	0.047	cubic	241.635	3926	3928
	-0.011	0.021	0.613	linear	186.612	3173	3332
Age $>$ median(age)	-0.019	0.027	0.403	quadratic	200.810	3506	3510
	-0.022	0.029	0.373	cubic	281.018	4759	4882
	-0.026	0.022	0.170	linear	192.638	2000	2460
Women	-0.057	0.032	0.042	quadratic	161.119	1634	2039
	-0.063	0.034	0.036	cubic	243.132	2633	3057
	-0.025	0.023	0.240	linear	160.348	3498	3450
Men	-0.029	0.026	0.224	quadratic	209.357	4792	4559
	-0.031	0.027	0.227	cubic	295.563	6615	6116
	-0.028	0.020	0.133	linear	183.442	4360	4420
Permanent contract	-0.034	0.026	0.135	quadratic	192.851	4536	4578
	-0.040	0.027	0.097	cubic	285.764	6664	6696
	-0.015	0.022	0.525	linear	199.002	1937	2006
Temporary contract	-0.027	0.030	0.266	quadratic	199.959	1937	2006
	-0.051	0.036	0.092	cubic	213.809	2057	2174
	-0.041	0.019	0.034	linear	181.942	3250	3345
Children	-0.050	0.026	0.030	quadratic	190.843	3403	3469
	-0.049	0.027	0.040	cubic	294.857	5379	5140
	-0.008	0.027	0.620	linear	146.330	2110	2352
No Children	-0.016	0.032	0.497	quadratic	190.212	2845	3035
	-0.029	0.035	0.323	cubic	237.485	3596	3801
	-0.008	0.032	0.913	linear	151.336	792	893
Immigrant	-0.041	0.037	0.172	quadratic	154.735	805	910
	-0.079	0.041	0.028	cubic	183.960	1003	1085
	-0.026	0.016	0.087	linear	194.186	5298	5459
No immigrant	-0.036	0.021	0.060	quadratic	182.951	4999	5210
	-0.042	0.022	0.037	cubic	273.473	7511	7573
	-0.018	0.021	0.398	linear	224.562	4263	4389
Lower education	-0.023	0.029	0.384	quadratic	224.984	4263	4389
	-0.022	0.032	0.447	cubic	275.410	5372	5283
	-0.030	0.024	0.141	linear	197.777	1940	1818
Medium education	-0.058	0.034	0.049	quadratic	186.739	1783	1735
	-0.068	0.037	0.036	cubic	277.145	2615	2508
	-0.021	0.029	0.528	linear	153.584	674	759
Higher education	-0.078	0.032	0.007	quadratic	134.560	569	685
	-0.101	0.035	0.001	cubic	175.520	773	910
	-0.019	0.020	0.283	linear	169.291	2644	2854
ln(wage) $\leq$ median(ln(wage))	-0.021	0.025	0.398	quadratic	209.006	3342	3693
	-0.025	0.027	0.359	cubic	307.673	4938	5073
	-0.029	0.018	0.108	linear	212.703	3746	3602
ln(wage) $>$ median(ln(wage))	-0.060	0.025	0.007	quadratic	152.603	2620	2517
	-0.076	0.027	0.003	cubic	188.191	3290	3184

Notes: The outcome variable is binary and indicates whether the person transitioned into a self-employment spell within the first 360 days of unemployment. We include all covariates. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N left) and to the right (N right) of the cutoff. Standard errors are clustered at the UI entry date level. We used the following median values: median(age)= 36 and the median ln(real monthly average wage)= 7.3. Pre-reform average probability of becoming self-employed within the first 360 days of the unemployment spell: 9.6%. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.16: Subgroup Analysis - Employment within 180 days (including covariates)

Sub Sample	RD Estimate	s.e.	p-value	Polynomial	Bandwidth	N Left	N Right
	0.073	0.044	0.050	linear	111.561	1645	1831
Age $\leq$ median(age)	0.101	0.054	0.028	quadratic	153.419	2371	2505
	0.125	0.063	0.030	cubic	198.852	3282	3189
	0.047	0.038	0.114	linear	95.036	1517	1639
Age $>$ median(age)	0.059	0.045	0.129	quadratic	158.756	2618	2795
	0.062	0.049	0.171	cubic	230.138	3978	4125
	-0.003	0.049	0.878	linear	111.129	1097	1426
Women	0.019	0.059	0.581	quadratic	146.418	1498	1874
	0.025	0.066	0.607	cubic	202.532	2195	2596
	0.100	0.039	0.003	linear	93.878	1948	1936
Men	0.114	0.043	0.003	quadratic	163.706	3603	3494
	0.116	0.045	0.005	cubic	272.936	6112	5750
	0.042	0.039	0.177	linear	102.649	2234	2297
Permanent contract	0.048	0.044	0.185	quadratic	172.051	4116	4214
	0.053	0.047	0.179	cubic	272.751	6416	6462
	0.089	0.047	0.029	linear	126.469	1065	1324
Temporary contract	0.136	0.058	0.007	quadratic	155.867	1380	1595
	0.155	0.070	0.018	cubic	193.903	1797	1964
	0.069	0.045	0.074	linear	114.329	1930	2071
Children	0.073	0.052	0.100	quadratic	187.597	3364	3416
	0.084	0.058	0.117	cubic	257.651	4731	4598
	0.065	0.041	0.046	linear	91.817	1252	1417
No children	0.109	0.048	0.010	quadratic	131.111	1843	2081
	0.109	0.050	0.013	cubic	215.132	3272	3464
	-0.026	0.065	0.756	linear	144.734	753	867
Immigrant	0.024	0.081	0.572	quadratic	182.046	997	1084
	0.106	0.113	0.268	cubic	190.694	1050	1112
	0.069	0.037	0.029	linear	100.230	2553	2788
No immigrant	0.077	0.043	0.043	quadratic	163.960	4407	4583
	0.076	0.043	0.053	cubic	291.908	8063	7977
	0.052	0.041	0.116	linear	108.658	1943	1988
Lower education	0.059	0.049	0.148	quadratic	172.370	3273	3476
	0.067	0.052	0.152	cubic	261.406	5118	5092
	0.079	0.052	0.060	linear	103.913	906	941
Medium education	0.102	0.057	0.038	quadratic	158.903	1486	1459
	0.121	0.063	0.036	cubic	199.206	1956	1828
	0.052	0.068	0.316	linear	132.678	559	676
Higher education	0.096	0.080	0.145	quadratic	162.266	714	807
	0.119	0.091	0.137	cubic	203.496	933	1056
	0.023	0.036	0.375	linear	107.591	1555	1758
ln(wage) $\leq$ median(ln(wage))	0.035	0.044	0.349	quadratic	143.728	2134	2512
	0.033	0.051	0.548	cubic	178.490	2748	3127
	0.083	0.049	0.044	linear	97.206	1623	1592
ln(wage) $>$ median(ln(wage))	0.093	0.054	0.045	quadratic	161.120	2757	2711
	0.109	0.063	0.059	cubic	214.668	3769	3632

Notes: The outcome variable is binary and indicates whether the person transitioned into an employment spell within the first 180 days of unemployment. We include all covariates. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. We show the effective number of observations used to the left (N left) and to the right (N right) of the cutoff. Standard errors are clustered at the UI entry date level. We used the following median values: median(age)= 36 and the median ln(real monthly average wage)= 7.3. Pre-reform average probability of finding a job within the first 180 days of the unemployment spell: 45.8%. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.8 Unemployment Duration Analysis

Table C.17: UI and UE Duration Means

	Pre-Reform			Post-Reform			Total Period		
	Mean	**Std. Dev.**	*N*	**Mean**	**Std. Dev.**	*N*	**Mean**	**Std. Dev.**	*N*
(A) UI Duration									
Self-Employment	5.850	6.189	2,329	6.004	5.985	2,189	5.924	6.091	4,518
Employment	6.884	6.137	15,958	7.086	6.022	14,035	6.979	6.084	29,993
Self-Employment or Employment	6.752	6.153	18,287	6.940	6.029	16,224	6.841	6.095	34,511
(B) UE Duration									
Self-Employment	8.914	12.414	2,329	8.433	10.776	2,189	8.681	11.651	4,518
Employment	9.829	12.025	15,958	9.363	10.300	14,035	9.611	11.253	29,993
Self-Employment or Employment	9.712	12.079	18,287	9.237	10.370	16,224	9.489	11.310	34,511

Notes: This table presents the estimated UI (panel A) and UE (panel B) duration means, standard deviations, and the number of observations for the pre- and post-reform period, respectively. The final column shows the respective values for the whole period. Relative changes in Table C.19 are based on the pre-reform means from this table. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.18: UI and UE Duration Elasticities (360 Days)

Outcome Variable	Duration Elast. (η)	RD Est.	% change in duration	s.e.	p-value	Polyn.	Covs.	N left	N right
(A) UI Duration									
(A1) Self-Employment	-0.510	0.257	8.5	0.474	0.649	linear		583	555
	-0.545	0.275	9.1	0.477	0.620	linear	✓	506	475
	-0.066	0.034	1.1	0.615	0.907	quadratic		633	589
	-0.200	0.101	3.3	0.616	0.984	quadratic	✓	584	557
	0.170	-0.086	-2.8	0.688	0.774	cubic		807	786
	-0.029	0.014	0.5	0.644	0.922	cubic	✓	860	854
(A2) Employment	0.389	-0.254	-6.5	0.274	0.181	linear		2163	2503
	0.621	-0.406	-10.4	0.268	0.054	linear	✓	1817	2074
	0.752	-0.491	-12.5	0.334	0.071	quadratic		3451	3670
	0.772	-0.504	-12.9	0.308	0.052	quadratic	✓	3384	3593
	0.990	-0.647	-16.5	0.394	0.065	cubic		4695	4988
	0.971	-0.634	-16.2	0.369	0.060	cubic	✓	4357	4647
(A3) Self-Employment or Employment	0.351	-0.222	-5.9	0.268	0.215	linear		2389	2551
	0.496	-0.314	-8.3	0.261	0.106	linear	✓	2128	2361
	0.584	-0.370	-9.7	0.311	0.127	quadratic		4070	4230
	0.641	-0.406	-10.7	0.296	0.094	quadratic	✓	3915	4065
	0.814	-0.516	-13.6	0.360	0.095	cubic		5709	5923
	0.861	-0.545	-14.4	0.343	0.074	cubic	✓	5104	5352
(B) UE Duration									
(B1) Self-Employment	-0.700	0.388	11.7	0.513	0.438	linear		589	560
	-0.756	0.419	12.6	0.507	0.378	linear	✓	555	506
	-0.278	0.154	4.6	0.673	0.975	quadratic		626	585
	-0.475	0.264	7.9	0.665	0.817	quadratic	✓	595	567
	0.292	-0.162	-4.9	0.784	0.680	cubic		751	718
	-0.085	0.047	1.4	0.747	0.894	cubic	✓	769	725
(B2) Employment	0.623	-0.424	-10.4	0.314	0.075	linear		1733	1982
	0.713	-0.486	-11.9	0.289	0.036	linear	✓	1622	1844
	0.783	-0.534	-13.1	0.349	0.061	quadratic		3313	3641
	0.821	-0.559	-13.7	0.317	0.038	quadratic	✓	3181	3511
	1.027	-0.700	-17.1	0.408	0.053	cubic		4878	5039
	1.010	-0.688	-16.8	0.376	0.045	cubic	✓	4394	4675
(B3) Self-Employment or Employment	0.478	-0.318	-8.0	0.301	0.139	linear		2021	2233
	0.548	-0.364	-9.1	0.275	0.082	linear	✓	1963	2158
	0.615	-0.409	-10.2	0.329	0.111	quadratic		4018	4186
	0.668	-0.444	-11.1	0.306	0.077	quadratic	✓	3642	3988
	0.862	-0.573	-14.4	0.384	0.081	cubic		5697	5902
	0.889	-0.591	-14.8	0.357	0.061	cubic	✓	5104	5352

Notes: This table presents our estimated UI (panel A) and UE (panel B) duration regression results. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. Standard errors are clustered at the UI entry date level. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) but restricted it to individuals who exit into self-employment, employment or either one of them within the first 360 days of unemployment. The duration elasticity, η, is computed from the percentage change in UI or UE duration (relative to the pre-reform average duration, see Table C.17), divided by the percentage change in the RR due to the reform (approx. 16.67%), as illustrated in equation 2. A summary of the results for exit within 360 and 720 days is provided in Table 2.
Source: Authors' calculations are based on MCVL 2005-2018 data.

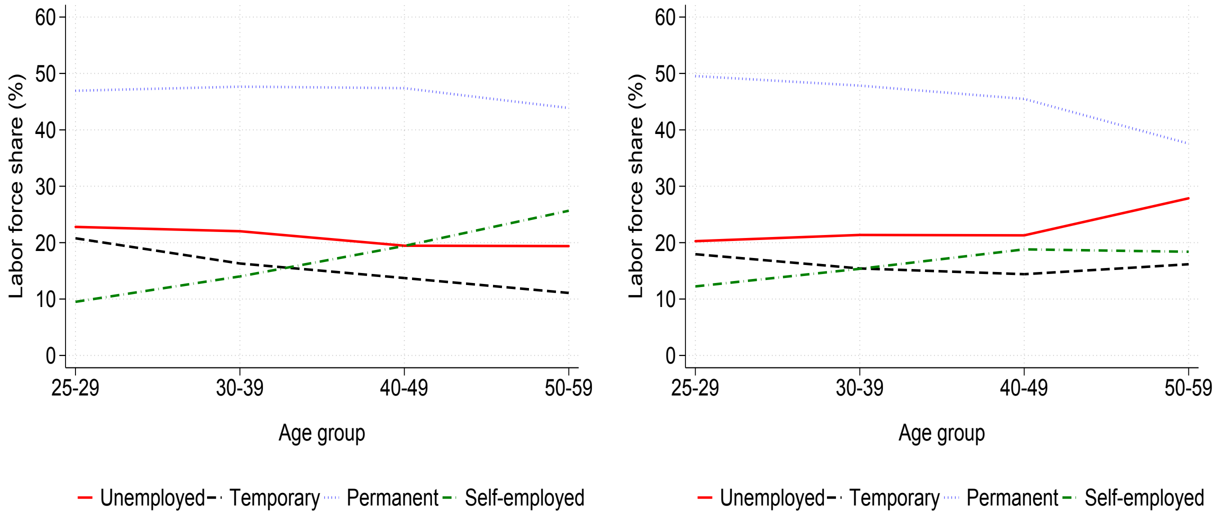
Table C.19: UI and UE Duration Elasticities (720 Days)

Outcome Variable	Duration Elast. (η)	RD Est.	% change in duration	s.e.	p-value	Polyn.	Covs.	N left	N right
(A) UI Duration									
(A1) Self-Employment	-1.199	1.006	20.0	1.045	0.296	linear		743	718
	-1.453	1.219	24.2	1.067	0.204	linear	✓	640	611
	-1.249	1.047	20.8	1.174	0.375	quadratic		1080	1049
	-1.480	1.241	24.7	1.168	0.288	quadratic	✓	1016	1008
	-1.430	1.199	23.8	1.389	0.356	cubic		1088	1073
	-1.725	1.447	28.7	1.401	0.303	cubic	✓	973	956
(A2) Employment	0.557	-0.581	-9.3	0.439	0.100	linear		3590	3988
	0.729	-0.760	-12.2	0.501	0.061	linear	✓	2920	3292
	0.830	-0.865	-13.8	0.543	0.063	quadratic		4881	5269
	0.865	-0.901	-14.4	0.551	0.062	quadratic	✓	5593	5897
	0.991	-1.032	-16.5	0.627	0.076	cubic		6234	6421
	1.032	-1.075	-17.2	0.657	0.088	cubic	✓	6337	6626
(A3) Self-Employment or Employment	0.308	-0.313	-5.1	0.457	0.331	linear		4370	4726
	0.428	-0.435	-7.1	0.516	0.247	linear	✓	3733	4090
	0.528	-0.536	-8.8	0.551	0.239	quadratic		5954	6252
	0.532	-0.541	-8.9	0.559	0.248	quadratic	✓	6759	7064
	0.663	-0.674	-11.1	0.619	0.225	cubic		7404	7685
	0.703	-0.714	-11.7	0.655	0.245	cubic	✓	7519	7743
(B) UE Duration									
(B1) Self-Employment	-1.118	1.053	18.6	1.202	0.341	linear		679	650
	-1.274	1.200	21.2	1.216	0.269	linear	✓	612	581
	-1.182	1.113	19.7	1.408	0.451	quadratic		926	879
	-1.352	1.274	22.5	1.370	0.350	quadratic	✓	915	877
	-1.034	0.974	17.2	1.570	0.583	cubic		1032	1023
	-1.321	1.244	22.0	1.553	0.466	cubic	✓	977	958
(B2) Employment	0.416	-0.473	-6.9	0.506	0.205	linear		3034	3429
	0.536	-0.610	-8.9	0.540	0.142	linear	✓	2850	3251
	0.566	-0.644	-9.4	0.565	0.169	quadratic		5474	5603
	0.584	-0.665	-9.7	0.565	0.163	quadratic	✓	6073	6255
	0.716	-0.814	-11.9	0.670	0.189	cubic		6345	6626
	0.780	-0.888	-13.0	0.699	0.191	cubic	✓	6337	6626
(B3) Self-Employment or Employment	0.195	-0.217	-3.2	0.532	0.487	linear		3711	4133
	0.292	-0.325	-4.9	0.561	0.384	linear	✓	3569	3974
	0.336	-0.373	-5.6	0.596	0.409	quadratic		6398	6590
	0.358	-0.398	-6.0	0.596	0.397	quadratic	✓	6818	7114
	0.469	-0.522	-7.8	0.679	0.374	cubic		7523	7787
	0.529	-0.589	-8.8	0.705	0.360	cubic	✓	7461	7711

Notes: This table presents our estimated UI (panel A) and UE (panel B) duration regression results. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. Standard errors are clustered at the UI entry date level. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) but restricted it to individuals who exit into self-employment, employment or either one of them within the first 720 days of unemployment. The duration elasticity, η, is computed from the percentage change in UI or UE duration (relative to the pre-reform average duration, see Table C.17), divided by the percentage change in the RR due to the reform (approx. 16.67%), as illustrated in equation 2. A summary of the results for exit within 360 and 720 days is provided in Table 2.
Source: Authors' calculations are based on MCVL 2005-2018 data.

C.9 Reform Effect on the Self-Employment Quality

Figure C.9: Distribution of Workers Across Employment States and Age Groups



(a) Before the Labor Market Reform (2005-2012) **(b)** After the Labor Market Reform (2013-2018)

Notes: These figures illustrate the distribution of workers across the different employment states, including unemployment, temporary employment, permanent employment and self-employment, with respect to their age group, as a percentage of the Spanish labor force. The share of self-employed among older individuals (50 and older) appears to decline in favor of unemployment and part-time employment, whereas for the youth (below 30) self-employment becomes more relevant.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.20: Mean Comparison Test of Self-Employment Quality

Variable	Pre Mean	Post Mean	Mean Diff.	N Pre	N Post
ln(real monthly average contribution basis)	7.359	6.816	-0.543 (0.005)	4,514	4,513
Agriculture, extraction, primary manufacturing	0.054	0.048	-0.005 (0.004)	4,518	4,518
Manufacturing and utilities	0.079	0.033	-0.045 (0.004)	4,518	4,518
Construction	0.174	0.133	-0.041 (0.005)	4,518	4,518
Trade	0.244	0.268	0.024 (0.007)	4,518	4,518
Transport and storage	0.058	0.053	-0.005 (0.003)	4,518	4,518
Accomodation and food services	0.085	0.127	0.042 (0.005)	4,518	4,518
I&C, finance, real estate, and scientific services	0.140	0.167	0.027 (0.006)	4,518	4,518
Education, health, social, and other services	0.167	0.170	0.003 (0.006)	4,518	4,518

Notes: This table presents the results of the mean-comparison tests of two measures of self-employment quality, including earnings and sector of activity. Standard errors are indicated in parentheses.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.21: Effect on (Self-)Employment Quality (Quadratic, 360 Days)

Outcome Variable	RD Est.	s.e.	p-value	Bandwidth	N Left	N Right	Covs.
Duration (monthly)							
Employment	0.153	1.271	0.878	189.308	3885	4230	
	0.174	1.346	0.941	183.778	3667	4003	✓
Self-Employment	-2.555	3.837	0.501	255.519	837	831	
	-2.488	3.788	0.531	255.300	821	819	✓
ln(real monthly average contribution basis)							
Employment	0.058	0.055	0.173	149.582	2938	3312	
	0.035	0.045	0.322	184.375	3654	4005	✓
Self-Employment	0.017	0.031	0.536	289.762	955	933	
	0.035	0.035	0.278	236.841	780	768	✓
Above median wage pre UI receipt							
Employment	-0.011	0.051	0.930	212.312	4460	4768	
	-0.015	0.039	0.666	238.501	4903	5138	✓
Self-Employment	-0.088	0.072	0.294	239.573	804	783	
	-0.081	0.073	0.190	162.239	527	500	✓
Agriculture, extraction, primary manufacturing							
Employment	0.034	0.019	0.091	185.251	3798	4159	
	0.027	0.015	0.062	258.377	5407	5453	✓
Self-Employment	-0.004	0.033	0.986	205.679	725	670	
	-0.008	0.031	0.824	198.800	694	605	✓
Manufacturing and utilities							
Employment	-0.036	0.023	0.140	247.853	5271	5443	
	-0.024	0.017	0.150	255.389	5246	5398	✓
Self-Employment	-0.039	0.033	0.280	205.349	725	670	
	-0.035	0.032	0.368	207.013	712	668	✓
Construction							
Employment	0.055	0.039	0.086	163.220	3296	3634	
	0.034	0.022	0.063	141.143	2731	3072	✓
Self-Employment	0.076	0.081	0.304	238.389	798	783	
	0.048	0.065	0.472	226.788	752	716	✓
Trade							
Employment	-0.028	0.034	0.269	154.413	3071	3419	
	-0.023	0.027	0.243	154.691	2990	3321	✓
Self-Employment	0.136	0.102	0.095	146.303	495	462	
	0.116	0.095	0.136	141.213	472	442	✓
Transport and storage							
Employment	0.009	0.014	0.436	234.767	4972	5226	
	-0.001	0.014	0.995	215.834	4394	4675	✓
Self-Employment	-0.026	0.051	0.473	184.042	626	585	
	-0.041	0.045	0.254	153.685	503	472	✓
Accommodation and food services							
Employment	-0.062	0.030	0.031	188.693	3875	4218	
	-0.011	0.018	0.589	212.487	4344	4630	✓
Self-Employment	0.003	0.055	0.975	187.165	638	595	
	-0.036	0.051	0.362	188.812	628	589	✓
I&C, finance, real estate, and scientific services							
Employment	0.012	0.022	0.617	223.071	4651	4950	
	0.007	0.018	0.636	177.926	3535	3892	✓
Self-Employment	0.017	0.067	0.784	249.894	829	816	
	0.009	0.054	0.935	174.633	584	557	✓
Education, health, social, and other services							
Employment	0.018	0.033	0.505	275.273	5903	5919	
	-0.015	0.028	0.499	211.154	4323	4608	✓
Self-Employment	-0.088	0.060	0.088	164.003	541	513	
	-0.031	0.051	0.462	199.361	699	607	✓
Permanent contract							
Employment	-0.039	0.047	0.549	166.596	3451	3670	
	-0.032	0.039	0.524	170.982	3417	3756	✓

Notes: In this table we estimate the causal reform effect on (self-)employment quality. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014), a quadratic specification and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We restrict our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) to individuals who exit into self-employment within the first 360 days of unemployment. Results for individuals who exit within the first 720 days of unemployment are provided in Table 3. Detailed results for the linear and cubic specifications are available upon request.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.22: Reform Effects on Monthly Earnings (Self-Employment within 360 Days)

Time Horizon	RD Estimate	s.e.	p-value	Polyn.	Covs.	Bandwidth	N left	N right
12 months after	0.069	0.055	0.114	linear		152.455	510	473
	0.080	0.048	0.046	linear	✓	177.611	589	561
	0.093	0.067	0.112	quadratic		238.643	796	779
	0.103	0.064	0.076	quadratic	✓	234.423	776	763
	0.066	0.102	0.537	cubic		222.493	756	715
	0.085	0.096	0.387	cubic	✓	226.554	750	713
18 months after	0.051	0.054	0.222	linear		147.178	496	457
	0.059	0.058	0.207	linear	✓	132.331	441	388
	0.068	0.074	0.304	quadratic		199.310	712	611
	0.076	0.074	0.268	quadratic	✓	189.292	626	587
	0.063	0.097	0.583	cubic		230.444	786	756
	0.081	0.089	0.379	cubic	✓	247.165	807	789
24 months after	0.080	0.048	0.049	linear		181.793	603	575
	0.093	0.045	0.018	linear	✓	175.057	576	552
	0.110	0.071	0.097	quadratic		199.315	702	608
	0.127	0.065	0.035	quadratic	✓	181.345	591	567
	0.115	0.091	0.212	cubic		223.534	749	711
	0.149	0.082	0.055	cubic	✓	214.403	715	682
36 months after	0.023	0.066	0.724	linear		159.546	515	491
	0.035	0.063	0.615	linear	✓	141.823	461	434
	0.023	0.072	0.737	quadratic		258.245	844	824
	0.026	0.075	0.852	quadratic	✓	201.639	685	627
	-0.077	0.114	0.390	cubic		192.078	635	592
	-0.017	0.100	0.773	cubic	✓	195.807	630	590
48 months after	0.040	0.067	0.425	linear		182.025	588	562
	0.066	0.066	0.229	linear	✓	136.870	443	388
	0.060	0.086	0.426	quadratic		228.101	755	709
	0.085	0.085	0.271	quadratic	✓	170.966	549	526
	0.046	0.119	0.743	cubic		217.165	718	685
	0.093	0.104	0.370	cubic	✓	205.968	683	637

Notes: In this table, we estimate the causal reform effect on earnings, approximated by the contribution basis in the case of self-employment for those who transition into (self-)employment within 360 days of entering unemployment. The dependent variable corresponds to log monthly earnings after different time periods measured in months after having entered UI. Note that the earnings or contribution basis in Table 3 and Table C.21 correspond to the exit status, i.e. the first status which an individual has after unemployment, whereas the earnings considered here might belong to a status which is different from the exit status, as we are able to trace the individuals' complete labor market trajectories until 2018. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014), a linear/quadratic/cubic specification and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We restrict our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) to individuals who exit into self-employment within the first 360 days of unemployment. An overview of the results for comparison purposes is provided in Table 4.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table C.23: Reform Effects on Monthly Earnings (Employment within 360 Days)

Time Horizon	RD Estimate	s.e.	p-value	Polyn.	Covs.	Bandwidth	N left	N right
12 months after	-0.001	0.040	0.766	linear		114.526	2152	2480
	-0.005	0.027	0.924	linear	✓	142.833	2733	3064
	0.031	0.048	0.335	quadratic		145.690	2861	3219
	0.007	0.034	0.632	quadratic	✓	166.876	3338	3528
	0.046	0.051	0.253	cubic		216.753	4499	4792
	0.017	0.037	0.486	cubic	✓	242.795	4968	5162
18 months after	-0.001	0.039	0.758	linear		117.698	2174	2523
	0.006	0.022	0.714	linear	✓	150.830	2867	3199
	0.027	0.045	0.369	quadratic		150.268	2942	3294
	0.014	0.027	0.438	quadratic	✓	179.923	3520	3887
	0.036	0.045	0.324	cubic		249.679	5221	5409
	0.021	0.028	0.319	cubic	✓	266.258	5440	5535
24 months after	-0.029	0.045	0.653	linear		144.996	2797	3184
	-0.009	0.032	0.706	linear	✓	153.455	2912	3249
	0.005	0.055	0.714	quadratic		175.068	3499	3908
	-0.015	0.035	0.595	quadratic	✓	263.940	5358	5477
	0.025	0.060	0.524	cubic		236.181	4890	5173
	0.002	0.051	0.913	cubic	✓	225.331	4456	4768
36 months after	-0.017	0.042	0.843	linear		140.262	2693	3049
	-0.001	0.032	0.997	linear	✓	141.264	2641	2988
	0.021	0.052	0.497	quadratic		169.375	3372	3606
	-0.009	0.033	0.710	quadratic	✓	254.617	5037	5243
	0.036	0.057	0.390	cubic		236.777	4833	5110
	0.011	0.044	0.633	cubic	✓	252.757	5003	5219
48 months after	0.003	0.037	0.685	linear		102.545	1812	2078
	-0.014	0.028	0.739	linear	✓	132.503	2352	2702
	0.021	0.043	0.419	quadratic		150.893	2850	3193
	-0.022	0.028	0.440	quadratic	✓	257.730	5159	5205
	0.017	0.043	0.529	cubic		265.068	5439	5515
	0.007	0.041	0.752	cubic	✓	222.913	4296	4579

Notes: In this table, we estimate the causal reform effect on earnings for those who transition into (self-)employment within 360 days of entering unemployment. The dependent variable corresponds to log monthly earnings after different time periods measured in months after having entered UI. Note that the earnings or contribution basis in Table 3 and Table C.21 correspond to the exit status, i.e. the first status which an individual has after unemployment, whereas the earnings considered here might belong to a status which is different from the exit status, as we are able to trace the individuals' complete labor market trajectories until 2018. The local polynomial estimation results are calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014), a linear/quadratic/cubic specification and a triangular kernel. We show the effective number of observations used to the left (N Left) and to the right (N Right) of the cutoff. Standard errors are clustered at the UI entry date level. We restrict our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions) to individuals who exit into employment within the first 360 days of unemployment. An overview of the results for comparison purposes is provided in Table 4.
Source: Authors' calculations are based on MCVL 2005-2018 data.

D Appendix: RDD Robustness Check - Competing Risks Model

In this section, we briefly discuss the results of an alternative approach to the estimation of the impact of unemployment benefit levels on the job-finding and startup rates. We consider that the response of unemployed individuals to the cut in UI benefits can be expressed as *failure* events. In this context, *failure* corresponds to the events of exiting from unemployment into self-employment or employment. The counterfactual outcome would be to stay unemployed. We also look at the *failure* of exiting into the union of self-employment and employment (general employment) vs. remaining unemployed.

Fine and Gray (1999) propose a framework to analyze such models. They take different *failure* events into account by modeling their respective cumulative incidence function (CIF) under a proportional hazard rate assumption. The Fine-Gray subdistribution hazard model can be defined as:

$$\lambda_k(t; \boldsymbol{X}) = \lambda_{k0}(t) \exp(\boldsymbol{X}^T \beta_k) \tag{D.1}$$

where $\lambda_k(t; \boldsymbol{X})$ denotes the subdistribution hazard function, $\lambda_{k0}(t)$ the baseline subdistribution hazard function for the kth event type, and $\boldsymbol{X}$ a set of covariates (Austin, Latouche, & Fine, 2020). The subdistribution hazard model allows us to estimate the effect of being treated on the CIF for each *failure* event, while controlling for other time-invariant covariates measured at the time of displacement. In our context $\boldsymbol{X}$ includes the same set of predetermined covariates as in our RDD specification. Beyersmann and Schumacher (2008) introduce time-dependent categorical and discrete covariates to the Fine-Gray model. We follow their approach to include variables which indicate whether individuals leave unemployment in a given month after the start of the UI spell in order to control for duration dependence.

Tables D.1 to D.3 summarize the results of the maximum-likelihood RDD hazard ratios and estimates of the competing-risks regression models according to the Fine and Gray (1999) model. Based on our estimated coefficients, we have computed the relative effects on the job-finding and startup rates (fourth column). In line with the RDD results from our baseline specification in Section 5.1, we observe consistently negative effects on the startup rate which are relatively stronger than the positive effects on the job-finding rate, regardless of the considered time horizon. Our estimates for self-employment, considered in more detail, seem to be robust to the inclusion of predetermined covariates and duration dependence controls, especially in the cubic polynomials. The effects' sizes seem to be stable over different time horizons, i.e. heterogeneity over time vanishes in the competing-risks framework. With respect to the probability of exiting into re-employment, we observe similar patterns to our baseline RDD results as well. Effects seem to be larger and more significant in the short term, while they decrease in the medium and long term, suggesting that the heterogeneity over time is still relevant in the competing-risks framework when it comes to employment. Lastly, the effects on the probability of exiting into the union of self-employment and employment are rather insignificant, slightly positive in the short term, and closer to zero (and sometimes even negative) in the medium and long term. Again, the negative effects on self-employment and the positive effects on employment cancel out each other if the union of both (general employment) is considered. Our estimated CIFs are also graphically expressed in Figure D.1 using the quadratic setting. Altogether, we find that the results pattern from our baseline RDD specification is still observed in more complex competing-risks regression models.

Table D.1: Competing-Risks Regression Results for Self-Employment

Event of Interest	Hazard Ratio	Estimate	Rate	s.e.	Polynomial	Covs.	Dur. Dep.
(A) SE within 90 days	0.905	-0.100	-9.5%	0.070	linear		
	0.912	-0.092	-8.8%	0.071	linear	✓	
	0.956	-0.045	-4.4%	0.069	linear	✓	✓
	0.789**	-0.236	-21.1%	0.108	quadratic		
	0.851	-0.162	-14.9%	0.108	quadratic	✓	
	0.866	-0.144	-13.4%	0.105	quadratic	✓	✓
	0.763*	-0.270	-23.7%	0.147	cubic		
	0.787	-0.240	-21.3%	0.148	cubic	✓	
	0.774*	-0.256	-22.6%	0.144	cubic	✓	✓
(B) SE within 180 days	0.908	-0.097	-9.2%	0.064	linear		
	0.912	-0.092	-8.8%	0.064	linear	✓	
	0.933	-0.069	-6.7%	0.064	linear	✓	✓
	0.803**	-0.219	-19.7%	0.097	quadratic		
	0.863	-0.148	-13.7%	0.097	quadratic	✓	
	0.858	-0.153	-14.2%	0.097	quadratic	✓	✓
	0.773*	-0.257	-22.7%	0.133	cubic		
	0.793*	-0.232	-20.7%	0.133	cubic	✓	
	0.775*	-0.255	-22.5%	0.133	cubic	✓	✓
(C) SE within 360 days	0.892*	-0.114	-10.8%	0.061	linear		
	0.894*	-0.112	-10.6%	0.061	linear	✓	
	0.916	-0.088	-8.4%	0.061	linear	✓	✓
	0.799**	-0.224	-20.1%	0.092	quadratic		
	0.857*	-0.154	-14.3%	0.092	quadratic	✓	
	0.865	-0.145	-13.5%	0.093	quadratic	✓	✓
	0.776**	-0.254	-22.4%	0.126	cubic		
	0.794*	-0.230	-20.6%	0.126	cubic	✓	
	0.794*	-0.231	-20.6%	0.127	cubic	✓	✓
(D) SE within 720 days	0.907*	-0.097	-9.3%	0.059	linear		
	0.909	-0.095	-9.1%	0.059	linear	✓	
	0.927	-0.075	-7.3%	0.060	linear	✓	✓
	0.818**	-0.201	-18.2%	0.089	quadratic		
	0.873	-0.136	-12.7%	0.089	quadratic	✓	
	0.875	-0.134	-12.5%	0.090	quadratic	✓	✓
	0.792*	-0.233	-20.8%	0.122	cubic		
	0.807*	-0.214	-19.3%	0.122	cubic	✓	
	0.797*	-0.227	-20.3	0.123	cubic	✓	✓

Notes: This table presents the maximum-likelihood RDD estimates of the competing-risks regression models according to the method of Fine and Gray (1999). The failure event of primary interest is exiting into self-employment within 90, 180, 360 or 720 days, and the competing failure event is exiting into re-employment in the same window. We provide results for different specifications of the RDD polynomial, including and excluding control variables. The last column indicates whether we control for duration dependence. The *Rate* column is computed from the value of the estimate: Rate $= (\exp(\hat{\beta}_{\text{RDD}}) - 1) \times 100$. We use the `stcrreg` routine in Stata to estimate the competing-risks regression models. $N = 33{,}833$ without controls, $N = 32{,}900$ with controls. Standard errors are clustered at the individual level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Table D.2: Competing-Risks Regression Results for Employment

Event of Interest	Hazard Ratio	Estimate	Rate	s.e.	Polynomial	Covs.	Dur. Dep.
(A) E within 90 days	0.975	-0.025	-2.5%	0.028	linear		
	0.981	-0.019	-1.9%	0.029	linear	✓	
	1.003	0.003	0.3%	0.025	linear	✓	✓
	1.045	0.044	4.5%	0.044	quadratic		
	1.035	0.034	3.5%	0.045	quadratic	✓	
	1.032	0.031	3.2%	0.039	quadratic	✓	✓
	1.066	0.064	6.6%	0.060	cubic		
	1.076	0.073	7.6%	0.061	cubic	✓	
	1.063	0.061	6.3%	0.054	cubic	✓	✓
(B) E within 180 days	1.017	0.017	1.7%	0.023	linear		
	1.028	0.027	2.8%	0.024	linear	✓	
	1.016	0.016	1.6%	0.025	linear	✓	✓
	1.105***	0.099	10.5%	0.036	quadratic		
	1.088**	0.084	8.8%	0.036	quadratic	✓	
	1.046	0.045	4.6%	0.039	quadratic	✓	✓
	1.101*	0.097	10.1%	0.049	cubic		
	1.098*	0.093	9.8%	0.050	cubic	✓	
	1.085	0.082	8.5%	0.053	cubic	✓	✓
(C) E within 360 days	1.026	0.025	2.6%	0.022	linear		
	1.033	0.032	3.3%	0.022	linear	✓	
	1.024	0.024	2.4%	0.025	linear	✓	✓
	1.086**	0.082	8.6%	0.034	quadratic		
	1.067*	0.065	6.7%	0.034	quadratic	✓	
	1.050	0.049	5.0%	0.038	quadratic	✓	✓
	1.069	0.067	6.9%	0.046	cubic		
	1.063	0.061	6.3%	0.047	cubic	✓	
	1.085	0.081	8.5%	0.052	cubic	✓	✓
(D) E within 720 days	1.024	0.024	2.4%	0.021	linear		
	1.030	0.030	3.0%	0.021	linear	✓	
	1.024	0.024	2.4%	0.024	linear	✓	✓
	1.086**	0.083	8.6%	0.032	quadratic		
	1.069**	0.067	6.9%	0.033	quadratic	✓	
	1.048	0.047	4.8%	0.038	quadratic	✓	✓
	1.075	0.073	7.5%	0.044	cubic		
	1.070	0.068	7.0%	0.045	cubic	✓	
	1.083	0.079	8.3%	0.051	cubic	✓	✓

Notes: This table presents the maximum-likelihood RDD estimates of the competing-risks regression models according to the method of Fine and Gray (1999). The failure event of primary interest is exiting into re-employment within 90, 180, 360 or 720 days, and the competing failure event is exiting into self-employment in the same window. We provide results for different specifications of the RDD polynomial, including and excluding control variables. The last column indicates whether we control for duration dependence. The *Rate* column is computed from the value of the estimate: $\text{Rate} = (\exp(\hat{\beta}_{\text{RDD}}) - 1) \times 100$. We use the `stcrreg` routine in Stata to estimate the competing-risks regression models. $N = 33{,}833$ without controls, $N = 32{,}900$ with controls. Standard errors are clustered at the individual level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

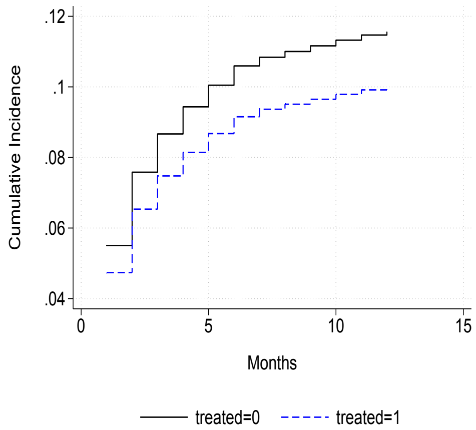
Table D.3: Competing-Risks Regression Results for the Union of Self-Employment and Employment

Event of Interest	**Hazard Ratio**	**Estimate**	**Rate**	**s.e.**	**Polynomial**	**Covs.**	**Dur. Dep.**
(A) SE or E within 90 days	0.951**	-0.050	-4.9%	0.025	linear		
	0.959*	-0.042	-4.1%	0.026	linear	✓	
	0.962	-0.039	-3.8%	0.018	linear	✓	✓
	0.982	-0.018	-1.8%	0.039	quadratic		
	0.992	-0.008	-0.8%	0.040	quadratic	✓	
	0.960	-0.041	-4.0%	0.025	quadratic	✓	✓
	0.988	-0.012	-1.2%	0.053	cubic		
	1.006	0.006	0.6%	0.054	cubic	✓	
	0.929	-0.073	-7.1%	0.034	cubic	✓	✓
(B) SE or E within 180 days	0.985	-0.015	-1.5%	0.021	linear		
	0.998	-0.002	-0.2%	0.021	linear	✓	
	0.939	-0.063	-6.1%	0.047	linear	✓	✓
	1.034	0.034	3.4%	0.032	quadratic		
	1.040	0.039	4.0%	0.033	quadratic	✓	
	0.977	-0.023	-2.3%	0.054	quadratic	✓	✓
	1.022	0.021	2.2%	0.044	cubic		
	1.031	0.031	3.1%	0.045	cubic	✓	
	1.011	0.011	1.1%	0.074	cubic	✓	✓
(C) SE or E within 360 days	0.990	-0.010	-1.0%	0.020	linear		
	1.002	0.002	0.2%	0.020	linear	✓	
	0.931	-0.071	-6.9%	0.057	linear	✓	✓
	1.019	0.019	1.9%	0.030	quadratic		
	1.026	0.025	2.6%	0.031	quadratic	✓	
	0.984	-0.017	-1.6%	0.064	quadratic	✓	✓
	0.991	-0.009	-0.9%	0.041	cubic		
	0.999	-0.001	-0.1%	0.042	cubic	✓	
	1.064	0.062	6.4%	0.090	cubic	✓	✓
(D) SE or E within 720 days	0.989	-0.011	-1.1%	0.019	linear		
	1.003	0.003	0.3%	0.019	linear	✓	
	0.928	-0.075	-7.2%	0.061	linear	✓	✓
	1.023	0.023	2.3%	0.029	quadratic		
	1.035	0.035	3.5%	0.029	quadratic	✓	
	0.982	-0.019	-1.8%	0.069	quadratic	✓	✓
	0.987	-0.014	-1.3%	0.040	cubic		
	0.999	-0.001	-0.1%	0.040	cubic	✓	
	1.084	0.081	8.4%	0.098	cubic	✓	✓

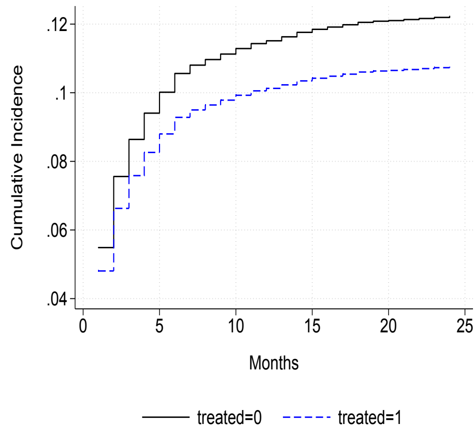
Notes: This table presents the maximum-likelihood RDD estimates of the competing-risks regression models according to the method of Fine and Gray (1999). The failure event of primary interest is transition into the union of self-employment and employment within 90, 180, 360 or 720 days, and the competing failure event is staying unemployed. We provide results for different specifications of the RDD polynomial, including and excluding control variables. The last column indicates whether we control for duration dependence. The *Rate* column is computed from the value of the estimate: $\text{Rate} = (\exp(\hat{\beta}_{\text{RDD}}) - 1) \times 100$. We use the `stcrreg` routine in Stata to estimate the competing-risks regression models. $N = 33,833$ without controls, $N = 32,900$ with controls. Standard errors are clustered at the individual level.
Source: Authors' calculations are based on MCVL 2005-2018 data.

Figure D.1: Cumulative Incidence Functions - Quadratic

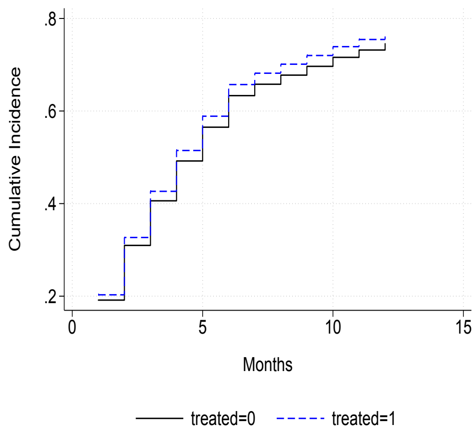
(a) SE within 360 days



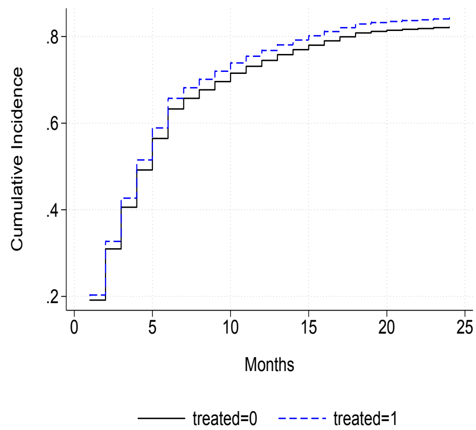
(b) SE within 720 days



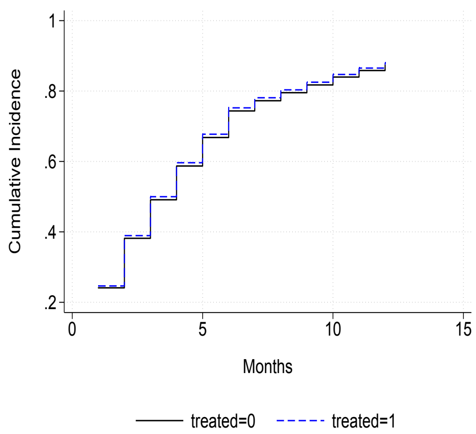
(c) E within 360 days



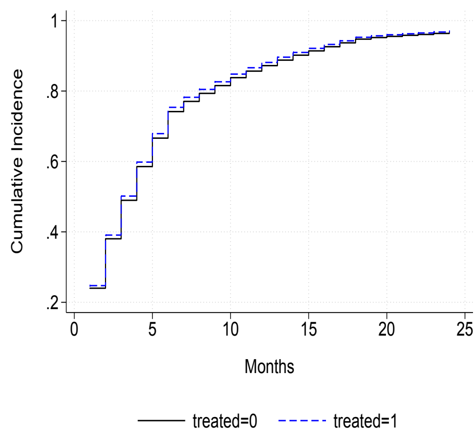
(d) E within 720 days



(e) SE or E within 360 days



(f) SE or E within 720 days



Notes: These figures illustrate the estimated cumulative incidence functions for self-employment, employment, and the union of both exit states. In other words, the probability that individuals become self-employed, employed, or either of them in each month of the respective 360- or 720-day window. The corresponding competing-risks models have been estimated using the complete set of covariates, excluding duration dependence, and a quadratic specification of the RDD polynomial. The `stcurve` routine in Stata has been used to generate the graphs.
Source: Authors' calculations are based on MCVL 2005-2018 data.

E Appendix: Data and Variables

E.1 MCVL Dataset

Spain's Continuous Working Life Sample - *Muestra Continua de Vidas Laborales* (MCVL) - allows us to extract employer-employee linked panel data. Starting from the year 2004, MCVL has been released every year by Spain's Dirección General de Ordenación de la Seguridad Social (DGOSS), with 2018 as the latest edition. It contains social security data of a four percentage non-stratified random sample of the population registered with the Spanish social security. Any individual who is working, receiving unemployment benefits, or receiving a pension in Spain could be in this sample.[45]

The MCVL consists of two versions. The version *Sin Datos Fiscales (SDF)* includes social security data without income tax records. Each edition provides data of contribution bases from which the real labor earnings can be inferred for most individuals. However, these real earnings are top- and bottom coded. In the version *Con Datos Fiscales (CDF)*, income tax records data is added, which provides information on each job and the uncensored real earnings separately. The data files contained in each edition can be merged via the person ID which is maintained across MCVL editions. Each MCVL edition comprises the complete labor market histories of each individual in the sample from 1953 until the respective year of the MCVL wave. Earnings data is available only since 1980. Combining the editions is useful to optimize the representativeness over time, since it allows to detect all individuals who are added because they have been registered with the Social Security, even though they may have been missing in one MCVL wave due to administrative mistakes. Thus, linking the MCVL editions allows us to fill gaps in the affiliations with the Social Security and update variables which are only updated when a new MCVL wave is produced (e.g. residence).

The MCVL provides not only monthly data on labor income and (un-)employment spells but also information on individual characteristics (gender, age, education, nationality, occupation, etc.), working time, and employers' characteristics (firm size, firm sector, etc.). Experience levels can be easily computed. We created an overview document that lists all variables contained in each of the MCVL waves (2005-2018): "***Documentation of MCVL Variables and Labels***".

To be able to work with the MCVL data, one has to apply for data access.[46] For more information on the Spanish social security data and its availability, we refer to the Dirección General de Ordenación de la Seguridad Social.

E.2 Data Construction

Due to space limitation, this part of the appendix provides a brief overview of our extensive data work. As we believe that our data and variable documentation can prove to be useful for other researchers who intend to work with the MCVL data, we refer to more detailed documentations that allow replication of our work.

[45] Note that in this working paper, we do not consider pension data and only partially use taxable income data.
[46] `http://www.seg-social.es/Estadisticas/EST211/1459`

E.2.1 From Raw to Master Data

Our *master dataset* aims to include as many variables and information as possible (e.g. it keeps parallel and overlapping spells from side jobs), such that it can be used as starting point for other research projects. We created an overview of all the variables which we obtain in our *master dataset*: "***MCVL-Variables.xlsx***". Our code partially builds upon the replication files and data documentations provided by Lafuente (2020), Agrawal and Foremny (2019), and De La Roca and Puga (2017). In the data documentations, we cite them for reference when we follow the corresponding author's approach, or we indicate in which way our concept differs. We refer to the first part of our data documentation "***Documentation I: From Original Data to Master Data***" for a detailed description of how to clean the original raw dataset from the Spanish social security authorities and construct our *master dataset*.

E.2.2 From Master Data to Final Results

Our *analysis dataset* is restricted to the needs of this research project. We only keep an individual's main spells and eliminate parallel and overlapping spells from side jobs using the procedures by Erhardt and Künster (2014). Again, we created an overview of all the variables which we obtain in the process of transforming the *master dataset* into the *analysis dataset*: "***MCVL_Variables_-Analysis***". The second part of our data documentation "***Documentation II: From Master to Analysis Data***" describes how we create our *analysis dataset* based on the *master dataset*.

E.3 Variables Overview

The following paragraphs give an overview of the variables that we use in our analysis. For details on all the variables in the MCVL dataset and their transformation, we refer to our data documentations, in particular to "***MCVL-Variables.xlsx***".

E.3.1 Outcome Variables

- **Extensive margin measures**: This is a set of binary outcome variables which take the value one if individual i becomes self-employed, employed, or either one of them within a certain amount of days. The variable takes the value zero if the individual remains unemployed or exits into an alternative state within this period. We choose intervals of 90, 180, 360, and 720 days.
- **Unemployment duration:** As we observe individuals' spells until the end of 2018, those who switch into an UI spell by the end of 2013 can be observed until a maximum of five years. We guarantee that pre- and post-reform period spells potentially have the same duration maximum by artificially right-censoring unemployment duration. We differentiate between two duration measures:
  - ***UI spell duration***: Actual UI spell duration in months. It excludes UA spells and spells without benefit receipt.

- ***UE spell duration***: Actual unemployment duration in months, including UI spells, subsequent UA spells, and spells without benefit receipt (unregistered periods of unemployment).

- **(Self-)employment quality measures**:
  - ***Duration***: Post-unemployment exit spell (either self-employment or employment) duration in months.
  - ***ln(real monthly average contribution basis)***: Natural logarithm of the individual's real monthly average contribution basis from Social Security records in 2015 euros. This variable corresponds to real earnings but only with regards to employment spells. We use it as the best available proxy for self-employment income.
  - ***Above median wage pre-UI receipt dummy***: Indicates whether the individual received a real monthly average wage above the median before he or she became unemployed. We use it as a proxy for high quality workers.
  - ***Permanent contract dummy***: Individual with a permanent contract (1), individual with a temporary contract (0). Permanent contracts may be interpreted as a sign for higher quality. Naturally, this information is not available for self-employment spells.
  - ***Sector of activity indicators***: Sector 1: Agriculture, extraction, primary manufacturing; Sector 2: manufacturing and utilities; Sector 3: construction; Sector 4: trade; Sector 5: transport and storage; Sector 6: accommodation and food services; Sector 7: information and communication (I&C), finance, insurance, real estate, and scientific services; Sector 8: education, health, social, auxiliary, and other services.

E.3.2 Predetermined Covariates

All control variables are measured at the individual's UI spell entry.

- Socioeconomic characteristics
  - ***Female dummy***: Female (1), male (0).
  - ***Age***: Individual's age in years.
  - ***Age^2***: Age squared.
  - ***Education level***: Lower education, medium education, and higher education.[47]
  - ***Presence of children dummy***: Presence of children in the household (1), no presence of children in the household (0).

[47] Lower education includes individuals without studies, with primary education, secondary school diploma (ESO), and basic professional training. Medium education includes Bachillerato, intermediate professional training, and other intermediate diplomas. Higher education includes university graduates, non-university higher studies diplomas, doctorates, masters, and other post-graduate studies (Dirección General de Ordenación de la Seguridad Social, 2020).

- ***Immigrant dummy***: Immigrant (1), no immigrant (0). We define an immigrant as a person with a different birth country than that of Spain. Alternatively we use a person's nationality.

- Pre-displacement job characteristics:
  - ***Employment experience***: Aggregated duration of an individual's employment spells in months.
  - ***Self-employment experience dummy***: Individual with self-employment experience (1), individual without self-employment experience (0).
  - ***ln(real monthly average earnings)***: Natural logarithm of the individual's real monthly average earnings from the Social Security records in 2015 euros. This variable is equivalent to the ln(real monthly average contribution basis) from above, but in this context we only consider previously employed workers. Workers who have been self-employed before they switch into an unemployment spell are excluded from our sample. Consequently, the contribution basis will always correspond to earnings with respect to our predetermined covariates.
  - ***Skill level***: High skilled, medium skilled, and low skilled occupation.[48]
  - ***Permanent contract dummy***: As specified above.
  - ***Sector of activity indicators***: As specified above.

- Unemployment characteristics:
  - ***Local unemployment rate***: Quarterly unemployment rate on a province level.[49]
  - ***Potential benefit duration (PBD)***: Individuals' potential UI benefit duration in months.

[48]This variable is based on the occupational codes described in Dirección General de Ordenación de la Seguridad Social (2020). We follow the same classification as in Rebollo-Sanz and Rodríguez-Planas (2020). High skilled occupations include engineers, college graduates, senior managers, technical engineers, graduate assistants, as well as administrative and technical managers. Medium skilled occupation includes non-graduate assistants, administrative officers, administrative assistants, as well as subordinates and auxiliary workers. Low skilled occupation includes first and second-class officers, third-class officers and technicians, laborers, as well as minors. Note that information on occupational codes is not provided for individuals in the special social security scheme of self-employed workers (Dirección General de Ordenación de la Seguridad Social, 2020).

[49]This variable is based on information extracted from official statistics published by INE (2018).

F Appendix: Institutional Details

F.1 Social Security System in Spain

The Spanish social security system is organized in four different contribution schemes. Ordinary employed individuals are registered within the *general scheme*, but there are also special schemes for sea workers, coal mining workers, and self-employed individuals (*autonomous scheme*). The social security system has increased in complexity over the years, and currently each of these schemes consists of several sub-schemes (artists, domestic workers, seasonal workers, etc.).

The social security legislation established specific regulations of these schemes for some groups, such as civil servants, armed forces, or education and health workers. Some reforms in the last decade have aimed at simplifying this intricate system (Spanish Social Security, 2018). For instance, in 2008, self-employed individuals of the former *special scheme for agriculture* were integrated into the *autonomous scheme*. Furthermore, the former *special scheme for agriculture* and the *special scheme for domestic employees* were integrated into the *general scheme* as of January 2012. For detailed information on unemployment and self-employment programs, we refer to our Online-Appendix "Unemployment and Self-Employment: Institutional Background".

F.2 Unemployment Insurance (UI)

The contribution period, which is used to calculate the PBD, excludes contributions which have already been used for previous UI spells. However, one can still claim the remaining entitlements. If an individual's employment spell lasted for at least 360 days and, thus, he or she qualifies for UI benefits, the individual is allowed to choose between the non-exhausted benefits from the last UI spell, and the new entitlement collected from the most recent employment spell (*option right*). Obviously, not only the PBD may differ but the amount of old and new benefits may differ as well because they are calculated from different pre-unemployment salaries. The non-selected entitlement will be lost. However, if the employment spell that followed the previous UI spell lasted for less than 360 days, the newly gathered entitlement is not lost. Instead, the worker can claim it as soon as the accumulated short-term employment spells reach the 360-days threshold (Alba-Ramirez, Arranz, & Muñoz-Bullón, 2007).

It is important to note that individuals who claim benefits after July 14 2012 (when the new RR was valid) could still receive UI benefits with the RR from the old system if they used the *option right*. We drop every potential *option right* case to avoid biased estimates from these cases. We also exclude individuals who exhaust the remaining entitlement from an old UI spell because they were not able to gather new entitlement in the meantime, i.e. those who did not work for at least 360 days before being laid-off. These individuals would be different from individuals who become less frequently unemployed and therefore have not exhausted any of their entitlements yet. The latter is the group we are interested in, which is why we exclude the former.

In case of part-time employment, the eligibility of a worker can only be determined with respect to the contribution periods of those jobs from which he or she has already been dismissed. As the UI benefit amount, which results from applying the RR to the regulatory base, must be weighted by the corresponding part-time coefficient, a half-day job collects only 50% of the benefits a full-day job would have generated. Additionally, part-time workers are not eligible for UI benefits if they work no more than 48 hours per month (Kyyrä, Arranz, & García-Serrano, 2019). From July 2018 onward, the relevant contribution period for the part-time employed corresponds to the time when the worker had an active affiliation, regardless of how many days in a given week one has worked and regardless of the amount of hours worked. The regulatory base corresponds to the average of the individual's contribution bases in both the lost and ongoing part-time contracts (SEPE, 2019).

Both UI and UA recipients are subject to penalties in terms of (partial) benefit loss if they commit an offense against provisions that regulate the unemployment protection. The level of a penalty depends on an offense's severity. There are minor, serious, and very serious offenses. The penalty becomes more severe the more often an offense is committed. For very serious offenses, benefits are canceled, and unduly collected benefits must be returned (SEPE, 2019).

F.3 Unemployment Assistance (UA)

UA eligibility requires one of the following circumstances: (1) UI benefits are exhausted and the individual has family dependents; (2) the individual received UI benefits for at least 360 days and is at least 45 years old; (3) the individual is ineligible for UI benefits because he or she contributed less than 360 days; (4) the individual is a returned emigrant; (5) the individual was released from prison; (6) the individual's disability spell ended because he or she was declared to be able to work; (7) the individual is at least 55 years old. The UA benefit amount is independent from the pre-displacement salary.[50] Instead, a flat benefit amount equal to 80% of the Public Income Index - *Indicador Público de Renta de Efectos Múltiples* (IPREM) - is paid to UA recipients. The duration of entitlement to UA benefits can reach a maximum of 30 months, depending on the individual's age and family responsibilities (SEPE, 2019).

F.4 Self-Employment and Social Security in Spain

The concept of self-employment (own-account work) is a broad category which includes different types of workers: self-employed workers, self-employed professionals and freelancers, self-employed entrepreneurs, economically-dependent self-employed workers (TRADE), agrarian self-employed workers, and some special cases. Self-employed individuals pay their social security contributions to the *Special Regime of Self-Employed Workers* (*RETA*). RETA includes self-employed workers older than 18 years of age who are not bound by a work contract, but also cases such as unpaid family members, book writers, TRADE workers, managers and CEOs (Spanish Social Security, 2018).

The contributions paid by the self-employed depend on the chosen level of social protection. The self-employed worker determines the contribution rate as well as the desired contribution base within existing legal bounds which are determined each year. For instance, if the worker decides to be insured against the risk of "cease of activity" (analogous to UI benefits in the *General Scheme*),

[50] Our Excel file "*UI_Benefits_Contributions_Calculator.xlsx*" provides a useful tool to check the specific UI and UA benefit limits applicable in each year.

2.20% of his or her income is added to the minimum contribution base. To also be insured against "professional contingencies" (protection in case of inability to work due to work-related reasons, e.g. accidents), between 1.3% and 6.8% is added. The minimum and maximum base among which the self-employed worker can choose depends on personal and occupational characteristics: age, marital status, contribution history, gender, disability, etc. (Spanish Social Security, 2018).

As of 2019, the Spanish government uniformed the RETA scheme, obliging all self-employed to pay all type of contingencies. De facto, the level of protection for the self-employed was equalized to the one of employees. It is noteworthy that, before this reform, only 19.7% of the self-employed had opted in to be covered for work accidents and occupational diseases (Eurofound, 2017).

In the MCVL data, we can observe all self-employed individuals, as they have to contribute at least a minimum amount to the social security system. However, we can only approximately infer the income of self-employed workers by assuming that those making more profits have chosen to contribute more to the social security system. In the future, the reform of 2019 may allow researchers to better approximate self-employment income.

F.5 Budgetary Adjustments and Public Firm Workers

Spain endured the economic and social consequences of the financial crisis of 2008 in a double dip recession. During the early period of the crisis, the national government tried to stimulate the economy through several programs, with the main goal of stabilizing employment. In 2009, investments into infrastructure, unemployment training and services, along with hiring incentives alleviated the first effects of the crisis. This first phase was followed by severe austerity policies aimed at reducing public deficit to 3% by 2013 (Lusiani, 2014). From 2010 to 2012, the Spanish government focused on keeping public spending minimal. These cutbacks had an impact on multiple levels of the public administration, resulting in a loss of about 103,000 public workers from 2012 to 2013, which represented 4.1% of public sector employees (Registro Central de Personal, 2017).

In the health administration, these budgetary adjustments were translated into wage and hiring freezes, which reportedly decreased the number of health professionals in public hospitals. The first ones to be laid off were, of course, temporary workers and substitutes. In 2012 the public job offers were frozen such that the replacement rate of workers was limited to only 10%, and the restrictions were even harder for temporary contracts. Between 2010 and 2013, the number of health workers in the public sector decreased by 21,011 individuals, i.e. 4.5% relative to 2010 (Bandrés & González, 2013). In the education sector, the same model of replacement and salary freezes was applied. Similarly, the number of employed educators decreased for all education levels by almost 20,000 workers (2.6%) from 2012 to 2013 (Pérez García & Uriel Jiménez, 2016).

When we include public firm workers in our RDD sample, our McCrary and non-parametric density test results indicate discontinuities in UI entries around the cutoff date. These discontinuities are caused by the dismissal of suspiciously many public firm workers in the months right after the reform was implemented. The discontinuities disappear when we exclude public firm workers, such that our identification assumptions are fulfilled.

F.6 Reforms

We present an overview of the main Spanish labor market reforms in recent years, along with the strategies we implement to address each one of them throughout our empirical analysis.

F.6.1 Unemployment Insurance System Reforms

In general, our UI entry date accounts for these reforms.[51] Some reforms affected the whole labor force in the same way, and thus do not violate our identification assumption. In addition, we restrict our analysis sample to full-time workers younger than 52 years of age, which avoids bias from the remaining reforms.

- **Introduction of the IPREM**, July 2004. The IPREM substitutes the minimum wage (*SMI*) as a reference for unemployment benefits and other social aids.
- **Active Insertion Income**, November 2006. State subsidy for workers with special economic needs and difficulties to find a job (e.g. individuals older than 45). Any person younger than 65 who fulfills the legal requirements may be eligible for this subsidy (SEPE, 2019).
- **Labour Market Reform I**, September 2010. New classification of fair dismissal conditions, and in some cases reduction of severance payments from 45 to 20 days per year of employment.
- **PREPARA**, February 2011. New extraordinary subsidy as incentive to provide long-term part-time contracts to unemployed individuals younger than 30, as long as they commit to training programs.
- **Labour Market Reform II**, July 2012.
  - RR reduction from 60% to 50% after 180 days of UI benefit receipt.
  - UA benefits extension until retirement for workers older than 55.
- **Budgetary Stability**, December 2013. End of the public contributions to the severance payments of dismissed workers in the case of objective reasons in solvent firms.

F.6.2 Self-Employment Reforms

Again, our UI entry date restrictions account for most of the following reforms. Potential inconsistencies from reforms which mainly target younger individuals are considered in Appendix F.6.3 and can be ruled out.

- **Self-employed Workers Statute**, October 2007.
  - Extension of social protection for temporary sick-leave to the self-employed.
  - Definition of the role of economically dependent self-employed workers (TRADE).

[51] In our analysis sample, we only include individuals transitioning to UI from January 1 2011 to December 31 2013.

- **Cease-of-activity Benefits (CAB)**, August 2010. Introduction of CAB as a voluntary contingency linked to work accidents and professional illness contingencies. CAB amounts are based on the principle of contribution-benefits.
- **Incentives to Entrepreneurship and Job Creation**, March 2013.
  - Capitalization of UI benefits for young employed workers: payment of 100% of the UI benefits to men younger than 30 and women younger than 35 who would like to become self-employed.
  - Reactivation of outstanding UI benefit payments after being self-employed with better conditions for workers under 30.
- **Strategy of Entrepreneurship and Youth Employment**, August 2013.
  - Flat and reduced rate of social security contributions for young self-employed workers (men under 30 and women under 35).
  - Improvement of financing for young self-employed workers.
- **Promotion of Self-Employment**, October 2015. Generalization of many advantages of young self-employed workers to all individuals.
- **Further Reforms**, December 2018.
  - All voluntary contingencies become compulsory (CAB and professional contingencies).
  - CAB duration is extended up to 24 months.

F.6.3 Ruling out Inconsistencies from the Self-Employment Reforms in 2013

In principle, it is possible that the reforms adopted in 2013, with the goal of promoting self-employment among young workers, could affect our results. These reforms incentivize self-employment by improving the financing of young self-employed workers, namely women younger than 30 and men younger than 35. Details about the reforms may be inferred from Appendix F.6.2. Since these reforms come with clear age criteria, we can infer individual eligibility from our data. For the following analysis, we create a self-employment reform eligibility indicator (SE reform), taking a value of one if the eligibility criteria are fulfilled (either female and younger than 35 or male and younger than 30), and zero otherwise.

Even though the self-employment reforms may alter decisions of unemployed individuals who have been previously looking for a job in regular employment, other authors did not address potential bias from these reforms (Rebollo-Sanz & Rodríguez-Planas, 2020; Fernandez-Navia, 2020). Where could these potential inconsistencies come from? Consider a very simplistic expression of the true relationship between the outcome variable of interest Y_i (self-employment or employment exit indicators) and treatment indicators of the UI benefit cut (UI reform) and the self-employment reforms (SE reform) on the right hand side, as illustrated in equation F.1.

$$Y_i = \alpha + \beta \cdot \mathbf{1}(t_i \geq 0) + \gamma \cdot \mathbf{1}(age_i < limit) + \epsilon_i = \alpha + \beta \cdot \text{UI reform} + \gamma \cdot \text{SE reform} + \epsilon_i \tag{F.1}$$

If this was the true relationship, omitting the SE reform dummy from the equation would lead to omitted variable bias, which could lead to inconsistent point estimates, depending on the direction and magnitude of the correlation between SE and UI reform indicators. This potential inconsistency is illustrated in equation F.2. The estimated UI reform coefficient $\hat{\beta}$ converges in probability towards the true effect β if the covariance between UI reform and SE reform indicator is equal to zero.

$$plim\ \hat{\beta} = \beta + \gamma \cdot \frac{Cov(\text{UI reform}, \text{SE reform})}{Var(\text{UI reform})} \tag{F.2}$$

Fortunately, we can compute this covariance directly from our data. We find a covariance which is very close to zero but with a slightly negative tendency (-0.002544) in our RDD sample. Our RDD sample includes individuals who switch into an UI benefit spell in the time between 2011 and 2013 if the bandwidth is not restricted any further (for more details please refer to Section 4.1). It converges even closer to zero if we restrict the bandwidth, as illustrated in Table F.1. As soon as the bandwidth hits 300 days, the covariance shows a positive tendency. Note that bandwidths of 300 days or lower are more plausible reference values, since they are closer to the MSE-optimal bandwidths selected in our local polynomial regressions in Section 5.1. Since the covariance between UI reform and SE reform seems to be very close to zero, we have reason to believe in a consistently estimated UI reform effect.

Table F.1: Covariance between UI Reform and SE Reform Indicators

Bandwidth	530	500	400	300	200	180	150
Covariance	-0.001647	-0.000945	-.000979	.000998	.003919	.003942	.00502

Notes: This table indicates the covariance between SE reform and UI reform indicators, computed from our RDD sample with different bandwidths in days around the UI reform date (July 15 2012).
Source: Authors' calculations are based on MCVL 2005-2018 data.

Nonetheless, we would like to consider, in more detail, any potential inconsistencies from the slightly positive covariance when we use MSE-optimal bandwidths. First, we consider the possibility of inconsistent point estimates in light of equation F.2 if we use the self-employment exit indicator as an outcome variable. Given that the true UI reform effect is indeed negative ($\beta < 0$) and the SE reforms have a positive effect on the self-employment probability ($\gamma > 0$), a slightly positive covariance between UI reform and SE reform would lead to an estimated effect on the self-employment probability ($\hat{\beta}$) which is slightly less negative than it would be under absence of the self-employment reform. Consequently, our estimated negative effect is slightly positively biased and may correspond to a lower bound estimate in absolute terms, which is very close to the true effect. Even if the SE reform effect would be huge, the inconsistency of the estimated UI reform effect would be very small.[52] Thus, our estimated UI reform effects on self-employment can be considered very conservative.

Second, we consider potential inconsistencies in the employment context. According to equation F.2, if we believe that the UI reform affects the job finding probability positively ($\beta > 0$), the SE reforms incentivize self-employment as opposed to employment ($\gamma < 0$), and we restrict the bandwidth to 300 or less, then our estimated positive effect on the employment probability would be slightly negatively biased. Consequently, we would estimate a more conservative lower bound estimate of the true effect as well.

In addition, we empirically test whether the SE reforms affect our outcome variables of interest in combination with the UI benefit reform by adding an interaction between UI reform and SE reform to our estimation equation 1. Since the `rdrobust` routine in Stata, which we use to estimate our local point estimates in Section 5, does not provide the estimated covariates' coefficients, we estimate a parametric regression instead. We test different bandwidths between 140 and 180 days, and use a linear, quadratic, and cubic spline. Our results regarding the medium-term self-employment indicator as outcome variable can be inferred from Table F.2. All specifications contain the covariates explained in Section 4.1, but we add the interaction term and the SE reform indicator in columns 2, 4, and 6. Overall, point estimates stay very robust to the inclusion of the additional variables. The coefficient of the interaction term is always very close to zero and insignificant. We obtain similar evidence if we use the short-term employment indicator as our outcome variable, as illustrated in Table F.3. We obtain a significant interaction effect only once, for a bandwidth of 160 days. Overall, evidence speaks in favor of a consistently estimated UI reform effect regardless of the outcome variable.

[52] Example: We can compute β, using our estimated medium-term UI reform effect of -3.5 p.p. in the quadratic setting from Section 5.1, and the UI variance of 0.249114 which we computed from our data. We use a plausible bandwidth setting of 200 days, for which the covariance between UI reform and SE reform corresponds to 0.003919. If we assume that the SE reforms increase the self-employment probability by 50 p.p. (i.e. $\gamma = 0.5$) which would be a tremendously huge effect, this would increase β by approximately 0.079 p.p. ($= 0.5 \cdot (0.003919/0.249114) = 0.007866$). Consequently, our estimated $\hat{\beta}$ of -3.5 p.p. corresponds to a lower bound estimate of the true effect ($\beta = -4.29$ p.p.) in this extreme setting with a huge SE reform effect.

Table F.2: Reform Interaction Effect on Self-Employment within 360 days

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**Bandwidth**
UI reform	-0.025	-0.024	-0.028	-0.027	-0.070**	-0.069**	
(=RD Effect)	(0.014)	(0.014)	(0.021)	(0.022)	(0.025)	(0.025)	
SE reform		0.013		0.013		0.013	140
		(0.017)		(0.017)		(0.017)	
UI reform · SE reform		-0.003		-0.003		-0.004	
		(0.014)		(0.014)		(0.014)	
N	9322	9322	9322	9322	9322	9322	
UI reform	-0.024	-0.022	-0.028	-0.026	-0.062*	-0.060*	
(=RD Effect)	(0.014)	(0.014)	(0.020)	(0.021)	(0.024)	(0.025)	
SE reform		0.013		0.013		0.013	150
		(0.016)		(0.016)		(0.016)	
UI reform · SE reform		-0.007		-0.007		-0.007	
		(0.013)		(0.013)		(0.013)	
N	9922	9922	9922	9922	9922	9922	
UI reform	-0.025	-0.023	-0.026	-0.024	-0.057*	-0.055*	
(=RD Effect)	(0.013)	(0.013)	(0.020)	(0.020)	(0.024)	(0.024)	
SE reform		0.014		0.014		0.014	160
		(0.015)		(0.015)		(0.015)	
UI reform · SE reform		-0.007		-0.008		-0.008	
		(0.012)		(0.012)		(0.012)	
N	10609	10609	10609	10609	10609	10609	
UI reform	-0.023	-0.021	-0.029	-0.027	-0.045	-0.043	
(=RD Effect)	(0.012)	(0.012)	(0.019)	(0.019)	(0.024)	(0.024)	
SE reform		0.010		0.010		0.010	170
		(0.014)		(0.014)		(0.014)	
UI reform · SE reform		-0.005		-0.005		-0.005	
		(0.012)		(0.012)		(0.012)	
N	11600	11600	11600	11600	11600	11600	
UI reform	-0.021	-0.019	-0.030	-0.029	-0.041	-0.039	
(=RD Effect)	(0.012)	(0.012)	(0.018)	(0.019)	(0.023)	(0.024)	
SE reform		0.011		0.011		0.011	180
		(0.014)		(0.014)		(0.014)	
UI reform · SE reform		-0.005		-0.005		-0.005	
		(0.012)		(0.012)		(0.012)	
N	12175	12175	12175	12175	12175	12175	
Polynomial	linear	linear	quadratic	quadratic	cubic	cubic	
Covariates	✓	✓	✓	✓	✓	✓	

Notes: The outcome variable is binary and indicates whether the person transitioned into a self-employment spell within the first 360 days of unemployment. We use our RDD estimation sample (detailed sample restrictions in Section 4.1). Standard errors are clustered at the UI entry date level. For reasons of comparability, the results are based on a global estimation approach using different bandwidths between 140 and 180 days. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: Authors' calculations are based on MCVL 2005-2018 data.

Table F.3: Reform Interaction Effect on Employment within 180 days

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**Bandwidth**
UI reform	0.013	0.002	0.067	0.056	0.105*	0.094	
(=RD Effect)	(0.025)	(0.026)	(0.035)	(0.035)	(0.049)	(0.049)	
SE reform		-0.046		-0.045		-0.045	140
		(0.025)		(0.025)		(0.025)	
UI reform · SE reform		0.040		0.041		0.041	
		(0.022)		(0.022)		(0.022)	
N	9322	9322	9322	9322	9322	9322	
UI reform	0.009	-0.001	0.065	0.055	0.099*	0.089	
(=RD Effect)	(0.025)	(0.025)	(0.034)	(0.034)	(0.048)	(0.048)	
SE reform		-0.037		-0.037		-0.037	150
		(0.024)		(0.024)		(0.024)	
UI reform · SE reform		0.036		0.037		0.038	
		(0.021)		(0.021)		(0.021)	
N	9922	9922	9922	9922	9922	9922	
UI reform	0.007	-0.006	0.057	0.044	0.100*	0.087	
(=RD Effect)	(0.024)	(0.024)	(0.033)	(0.033)	(0.046)	(0.046)	
SE reform		-0.046*		-0.046*		-0.046*	160
		(0.023)		(0.023)		(0.023)	
UI reform · SE reform		0.046*		0.048*		0.047*	
		(0.020)		(0.020)		(0.020)	
N	10609	10609	10609	10609	10609	10609	
UI reform	0.009	-0.002	0.045	0.034	0.101*	0.090*	
(=RD Effect)	(0.023)	(0.024)	(0.034)	(0.034)	(0.045)	(0.045)	
SE reform		-0.026		-0.026		-0.026	170
		(0.024)		(0.024)		(0.024)	
UI reform · SE reform		0.038		0.039		0.039	
		(0.020)		(0.020)		(0.020)	
N	11600	11600	11600	11600	11600	11600	
UI reform	-0.003	-0.013	0.055	0.046	0.082	0.071	
(=RD Effect)	(0.023)	(0.024)	(0.033)	(0.033)	(0.044)	(0.043)	
SE reform		-0.025		-0.025		-0.025	180
		(0.023)		(0.023)		(0.023)	
UI reform · SE reform		0.036		0.036		0.037	
		(0.020)		(0.020)		(0.020)	
N	12175	12175	12175	12175	12175	12175	
Polynomial	linear	linear	quadratic	quadratic	cubic	cubic	
Covariates	✓	✓	✓	✓	✓	✓	

Notes: The outcome variable is binary and indicates whether the person transitioned into an employment spell within the first 180 days of unemployment. Standard errors are clustered at the UI entry date level. We use our RDD estimation sample (detailed sample restrictions in Section 4.1). For reasons of comparability, the results are based on a global estimation approach using different bandwidths between 140 and 180 days. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: Authors' calculations are based on MCVL 2005-2018 data.

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