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The effect of unemployment insurance benefits on (self-)employment: Two sides of the same coin?

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Non-technical summary

Research Question

Even though current active labor market policies in Europe often subsidize unemployed individuals to start their own businesses, little is known about the relationship between unemployment insurance (UI) benefits and self-employment. Our study investigates how a decrease in UI generosity in Spain affects both sides of the same coin: employment and self-employment. We address the following questions: How does a cut in UI benefits affect the extensive margin of employment and self-employment? Does the unemployment duration of those who become self-employed differ from that of those who pursue re-employment? Does less UI generosity lead to quality differences in post-unemployment labor market states?

Contribution

To shed light on these issues, we analyze the heterogeneous effects of an exogenous cut in UI benefits on self-employment and employment in Spain using administrative employer-employee data from the Spanish social security system. First, we estimate the causal effect of the reduction in UI generosity on the probability of exiting unemployment in general. We then decompose the overall effect into distinct causal effects on the self-employment probability (startup rate) and the employment probability (job-finding rate). Second, we estimate the causal 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.

Results

We find heterogeneous effects of the cut in UI benefits 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. Over different time horizons, the negative effect on self-employment (35-50%) is consistently stronger than the positive effect on employment (5-33%). While we cannot confirm any causal relationship between the cut in benefits and changes in (self-)employment quality, we find that the UI duration elasticity is larger in absolute terms for those who exit into self-employment compared to those who exit into employment. The different effects of UI benefit levels on self-employment and re-employment might be important to consider for the optimal design of UI systems.

Nichttechnische Zusammenfassung

Fragestellung

Obwohl die derzeitige aktive Arbeitsmarktpolitik in Europa oft darauf abzielt Arbeitslose finanziell zu unterstützen, damit diese sich selbständig machen können, ist wenig über die Auswirkungen der Höhe des Arbeitslosengeldes auf die berufliche Selbständigkeit bekannt. Unsere Arbeit untersucht, wie eine Absenkung der Leistungen der Arbeitslosenversicherung in Spanien zwei Seiten derselben Medaille beeinflusst: abhängige Wiederbeschäftigung und berufliche Selbständigkeit. Wir beschäftigen uns dabei mit folgenden Fragen: Wie beeinflusst eine Reduzierung des Arbeitslosengeldes die Entscheidung sich selbständig zu machen oder in ein abhängiges Beschäftigungsverhältnis einzutreten? Unterscheidet sich die Arbeitslosigkeitsdauer derer, die sich für die Selbständigkeit entscheiden gegenüber den abhängig Wiederbeschäftigten? Führt geringere Generosität zu Qualitätsunterschieden des jeweiligen Arbeitsmarktverhältnisses nach Beendigung der Arbeitslosigkeit?

Beitrag

Um diese Fragen beantworten zu können, untersuchen wir die heterogenen Effekte einer exogenen Reduzierung des Arbeitslosengeldes auf Selbständigkeit und Wiederbeschäftigung in Spanien mithilfe von administrativen Sozialversicherungsdaten. Zunächst schätzen wir den kausalen Effekt dieser Reduzierung auf die Wahrscheinlichkeit die Arbeitslosigkeit zu verlassen. Danach spalten wir den generellen Effekt auf und schätzen den kausalen Effekt auf die Wahrscheinlichkeit, sich selbständig zu machen (Gründungsrate) und den Effekt auf die Wahrscheinlichkeit wieder ein abhängiges Beschäftigungsverhältnis einzugehen (Job-Findungsrate). Darüber hinaus schätzen wir den Effekt auf die Arbeitslosendauer der Selbständigen und Wiederbeschäftigten und berechnen die jeweiligen Elastizitäten. Außerdem analysieren wir den kausalen Zusammenhang zwischen der Reduzierung des Arbeitslosengeldes und der Qualität des neuen Arbeitsverhältnisses, um mögliche Wohlfahrtsimplikationen abzuleiten.

Ergebnisse

Unsere Ergebnisse weisen auf heterogene Effekte durch die Reduzierung des Arbeitslosengeldes hin. Der Effekt auf die Gründungsrate ist negativ und wird über die Dauer der Arbeitslosigkeit stärker, während der Effekt auf die Wiederbeschäftigung positiv ist und abnimmt. Über verschiedene Zeithorizonte ist der negative Effekt auf die Selbständigkeit (35-50%) durchweg stärker als der positive Effekt auf die Wiederbeschäftigung (5-33%). Einen kausalen Zusammenhang zwischen der Reduzierung des Arbeitslosengeldes und der Qualität des jeweiligen Arbeitsverhältnisses nach der Arbeitslosigkeit können wir zwar nicht bestätigen, jedoch finden wir absolut eine höhere Elastizität der Arbeitslosendauer für Selbständige verglichen mit abhängig Wiederbeschäftigten. Der unterschiedliche Effekt der Höhe des Arbeitslosengelds auf Selbstständigkeit und Wiederbeschäftigung könnte für das optimale Design von Arbeitslosenversicherungssystemen wichtig sein.

Bundesbank Discussion Paper No 18/2021

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

Sebastian Camarero Garcia† & Michelle Hansch‡

Abstract

Although a meaningful percentage of firms are 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 previously unavailable 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%). 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. Due to the reform's unintended consequences for self-employment, its *general employment* effect is much smaller than claimed by analyses that focus only on employment.

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

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

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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 and Meyer, 1990; Card and Levine, 2000; Kolsrud, Landais, Nilsson, and 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 are 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 probability of exiting into self-employment (startup rate) and re-employment (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 the effect of UI benefit levels on self-employment is different to that on re-employment. According to *standard search theory*, a cut in UI benefit levels lowers the reservation wage, i.e. the opportunity costs of job search decline. Consequently, unemployed individuals should increase search intensity. Therefore, the

[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 denote 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 were unemployed before starting their firms in Spain. In Germany, about one quarter of startups emerged out of unemployment between 2005 and 2015 (Camarero Garcia and Murmann, 2020).

[3] In Spain, such policies were applied in response to 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 and Cueto, 2015). Garcia-Cabo and Madera (2019) provide a good overview of self-employment options in rigid labor markets like Spain.

probability of exiting unemployment should increase and, thus, actual unemployment duration would decrease (Mortensen, 1977; Schmieder, von Wachter, and 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. However, taking general equilibrium effects into account, once 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 we would also predict a relative decrease in the startup rate because more job vacancies only increase re-employment options but do not directly affect self-employment opportunities. Taking both partial and general equilibrium considerations 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 and Laffont, 1979; Evans and Jovanovic, 1989). The basic versions of this model focus on the role of personal characteristics[4] in the entrepreneurial choice problem. Alba-Ramirez (1994) expands the model to consider the role of unemployment.[5] According to this model, 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 fewer stigma effects as proposed by Jarosch and Pilossoph (2019)), and thus, relatively better employment prospects compared to a setting with unchanged UI benefits. Consequently, the expected employment income remains relatively higher, suggesting that the job-finding rate should increase. Regarding the expected self-employment income, shorter actual UI benefit duration, however, implies less time in which to learn about 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 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 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 be based.

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; by contrast similar data for other countries are mostly unavailable.

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

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: the net replacement rate for the time after 180 days of benefit receipt subsequently 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 setup 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 startup rate 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 other hand, the exit probability of finding a job is positively affected in the short term while attenuating in the medium and long term. The effect on the union of both exit states is slightly positive in the short term 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 each other out. 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 increase (decrease) in the short-term probability of exiting into re-employment (self-employment). 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 corresponding estimates range between 17% and 19%. Additionally, we find that in relative terms the negative effect on self-employment (35-50%) is consistently 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,

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

[7] Rebollo-Sanz and Rodríguez-Planas (2020) exploit the same Spanish labor market reform in 2012. Using an RDD, 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. Unlike Rebollo-Sanz and Rodríguez-Planas (2020), we take all possible unemployment exit states into account and also consider long-term effects.

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 irrespective of whether they become re-employed or self-employed, but in opposing directions. We estimate a positive UI benefit duration elasticity of approximately 0.8-0.9 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 -1.2 and -1.5). This finding could be explained through liquidity constraints imposed by the cut in UI benefits, which impact potential founders more than individuals in search of 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 0.3-0.7). 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. While re-employment wages appear to stagnate, our proxy for self-employment income increases in response to the reform. The cut in UI benefits did not significantly affect the quality of post-unemployment startups and only slightly worsens the quality of jobs. Altogether, UI benefits affect the extensive margin of transitions into (self-)employment but not 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 and Leighton, 1989; Levine and 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, and Bender, 2012; Schmieder et al., 2016; Schmieder and 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 and Rodríguez-Planas, 2020; Meyer and Mok, 2014; Lalive, Van Ours, and Zweimüller, 2006). However, to the best of our knowledge, self-employment is usually ignored when these effects are estimated due to data limitations. 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 policies considering UI generosity changes which has mostly focused on the US (e.g., Farber, Rothstein, and Valletta, 2015; Card, Johnston, Leung, Mas, and 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 to this day, given the economic crisis on the heels of the COVID-19 pandemic, many European countries have been suffering from high unemployment rates which policymakers often aim to mitigate by easing the transition into self-employment.[8] This illustrates the highly relevant nature of our research questions. 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 makes for an interesting case study because it allows us to investigate a policy (in times of crisis) with good internal validity and high data quality. We can thus 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 explains the institutional background of social security in Spain and the labor market reform on which our identification strategy relies. Section 3 describes our data and provides a descriptive analysis of the Spanish labor market flows over time (2005-2018). Section 4 explains our estimation methodology and its underlying assumptions. Section 5 presents our results. Finally, Section 6 discusses our results and concludes.

2 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 A.

[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.

2.1 Unemployment Benefits in Spain

Unemployment Insurance (UI) Benefits. To be eligible for 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. The duration of entitlement to UI benefits depends on the contribution period. Table 1 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).

Table 1: 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: 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).

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, a second replacement rate of 60% is valid from day 181 onward. 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)[9] – 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.[10] 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 benefit levels were kept nominally constant in Spain.[11]

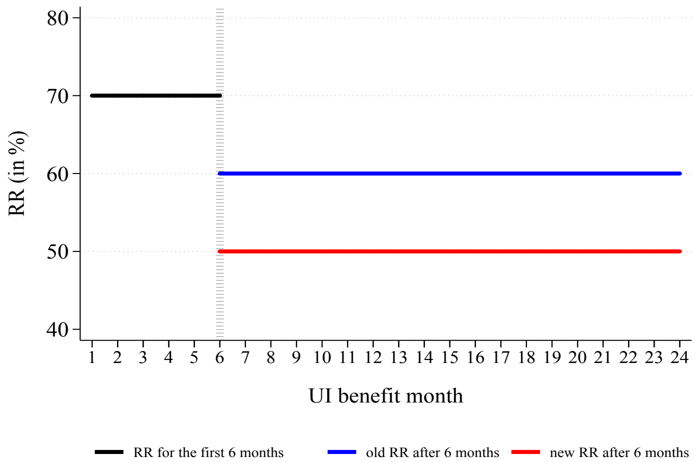
Unemployment Assistance (UA) Benefits. Under certain conditions registered job seekers are eligible for Unemployment Assistance (UA) benefits: the individual must be ineligible for UI benefits (or exhausted them) and the monthly gross income must be less than 75% of Spain's minimum wage.[12]

[9]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.
[10]Details on the calculation of UI benefits can be inferred from Table A.1 in Appendix A.2.
[11]The evolution of UI benefit levels and the number of beneficiaries is shown in Figure A.1 in Appendix A.2. For more details on the Spanish UI system, we refer to Appendix A.2.
[12]Additional information on the Spanish UA system is provided in Appendix A.3.

2.2 Labor Market Reform in 2012

We focus on a Spanish labor market reform which was publicly announced on July 13, 2012.[13] 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 1. 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 1: 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.

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.

Since Spain's unemployment rate peaked at 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 and 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. [14] In Appendix E.4, we show that these self-employment reforms do not influence our results.

[13] By virtue of the Royal Decree-Law 20/2012, this reform aimed to ensure budgetary stability and competitiveness.
[14] A detailed overview of all reforms is given in Appendix A.6.

3 Data and Descriptive Analysis

In this section, we describe our dataset and provide a descriptive analysis of the Spanish labor market, with a particular focus on the transitions between unemployment, self-employment, and employment during the time period 2005-2018. Thus, this section illustrates the relevance of our research questions and provides insights into how the data can be used.

3.1 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 authorities 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. Spells 1) - 4) imply that the individuals are actively registered with the social security authorities, whereas individuals in spell 5) are unregistered. 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. 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.[15] 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. While processing the MCVL data, the nominal contribution basis was deflated using the Consumer Price Index (CPI) with 2015 as a base year. Appendix B provides more information on the MCVL data and gives an overview of our variables and data construction. Procedures for replicating our datasets and results can be gathered from our data documentations. Details on our estimation sample are explained in Section 4.1.

3.2 Descriptives – Labor Market Flows

This section documents how the main labor market states evolve in the period 2005-2018 in Spain. For the construction of the annual 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

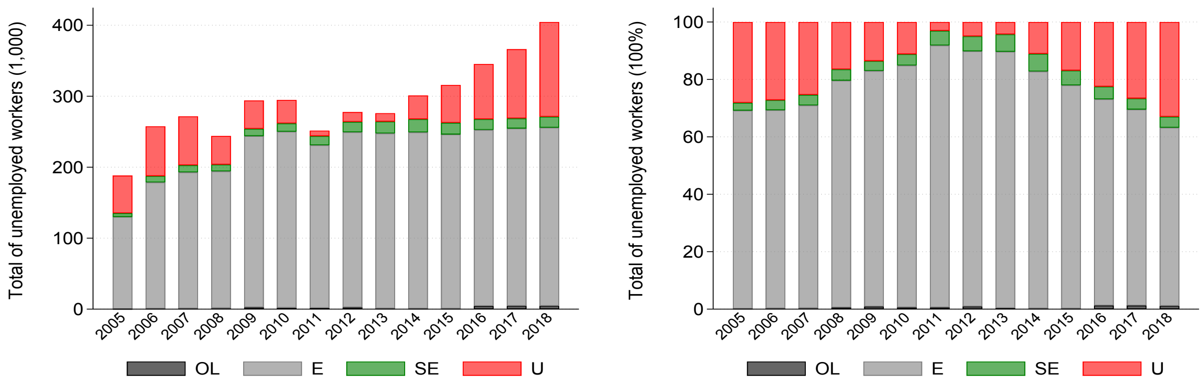
[15] Following the definition of De La Roca and Puga (2017), we compute experience as accumulated time spent in employment, starting from the first job in an individual's life.

social security files from 2005 to 2018. For clarity purposes, the terms Self-Employment (SE), Employment (E), Unemployment (U), and Out of Labor Force (OL) are abbreviated in our graphs.

Figure 2 depicts the yearly composition of transitions from unemployment in Spain. It illustrates that the share of individuals who transition from unemployment to (self-)employment remains relatively stable during the years surrounding the 2012 labor market reform. Even though the share of individuals who transition to self-employment is relatively larger around the reform than at the beginning of the sample period, the outflows from unemployment are clearly dominated by employment. After 2013 unemployed individuals increasingly remain unemployed, while outflows into employment decrease.

At first glance, the self-employment exit channel seems negligible. However, if we examine it from the perspective of self-employment inflows, the picture changes tremendously. Figure 3 illustrates the yearly inflows to self-employment in Spain, in both (a) absolute and (b) relative terms, excluding the stock of self-employed individuals. It 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. Given that Spain's self-employment rate is among the highest in the EU – it varied between 16.4% and 17.9% during the 2010s (OECD, 2018) – the inflow from unemployment into self-employment is important. We find that it makes up 30-50% of all new self-employed individuals every year. 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 2), there are usually job spillovers, i.e. most founders have employees. Since startups can be engines for economic growth, 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.

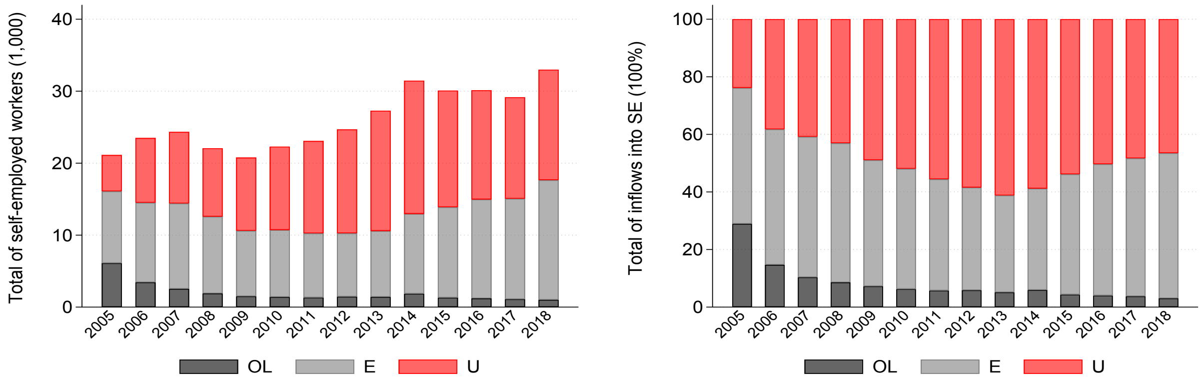
Figure 2: Composition of Outflows from Unemployment



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

Notes: These figures illustrate the yearly composition of transitions from Unemployment (U) (outflows) in Spain, in both (a) absolute and (b) relative 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 based on MCVL 2005-2018 data.

Figure 3: Composition of Inflows into Self-Employment (Excl. Stocks)



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

Notes: These figures illustrate the yearly inflows to self-employment in Spain, in both (a) absolute and (b) relative 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 C.7 to 2 for a representation of inflows to and outflows from other statuses.
Source: Authors' calculations based on MCVL 2005-2018 data.

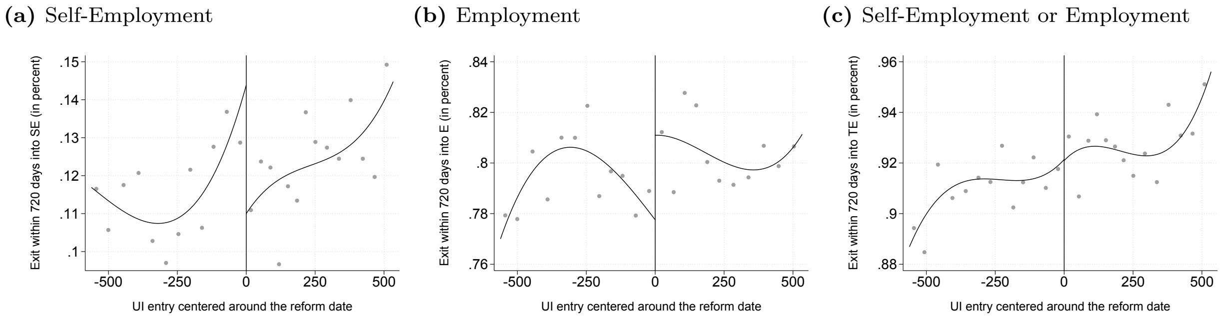
For a more extensive analysis of labor market flows in Spain over time (2005-2018), we refer to Appendix C, in which we also confirm our accuracy in constructing the dataset by showing that we are able to match key labor market facts as provided by official bodies such as the OECD or the Spanish National Statistics Institution (*Instituto Nacional de Estadística (INE)*). Moreover in Appendix C, we present further descriptive analyses on the personal characteristics of the self-employed (compared to the employed), the sector in which businesses are created, earnings, and on the labor force composition.

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 2.2). 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 setup to identify causal reform effects using an RDD. Our estimation sample consists of UI recipients who were displaced from a full-time job.[16] 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.

[16]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 A.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 4: 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 D.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 sector firm (see Section 4.1 for a description of detailed sample restrictions). *Source:* Authors' calculations based on MCVL 2005-2018 data.

In Figure 4 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.[17] 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.[18] The effect on the union of self-employment and employment appears to be rather small if we use a quadratic polynomial (in Appendix Figure D.1) and vanishes if we use a cubic one. Consequently, using different functional forms to verify the robustness of our results seems highly relevant. It is worth noting that the scale is different for each exit state because more individuals transition into employment rather than self-employment. 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 setup 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),

[17]Bins contain approximately the same number of observations but their length may differ (Cattaneo et al., 2019).
[18]Appendix Figure D.1 plots the graphs using different polynomials.

whereas 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 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[19] 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-sector[20] firm. We exclude individuals who contributed to a social security scheme[21] different from the general scheme right before they became unemployed, as well as disabled persons. Moreover, we restrict our sample to individuals aged between 25 and 52. As the 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.[22] In line with Fernandez-Navia (2020) we exclude UI benefit spells for which individuals potentially use the *option right*.[23] 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).

Table 2 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). The share of women and immigrants is higher for individuals who exit into employment as compared to those who exit into self-employment.[24] 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.[25] The self-employed workers earn, on average, only slightly higher monthly real incomes and are entitled to only slightly more PBD.[26] In general, differences between both groups are less severe as compared to the sample that we used for our descriptive analysis (Table C.1 in Appendix C), which is due to our sample restrictions that render treatment and control groups comparable.

[19] 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.

[20] Shortly after the reform's implementation, we find an increase in dismissals of public sector workers in the data. We also find evidence of imbalanced covariates if we include public sector 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 A.5).

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

[22] Compare Section 2.1 and Appendix A.2 for more details on the reform.

[23] 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 A.2).

[24] Similar to the comparison in Table C.1 in Appendix C regarding our descriptive analysis.

[25] 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.

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

Table 2: Summary Statistics 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-skilled occupation	0.495	(0.500)	0.577	(0.494)	0.565	(0.496)
Medium-skilled occupation	0.326	(0.469)	0.309	(0.462)	0.316	(0.465)
High-skilled 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. 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 based on MCVL 2005-2018 data.

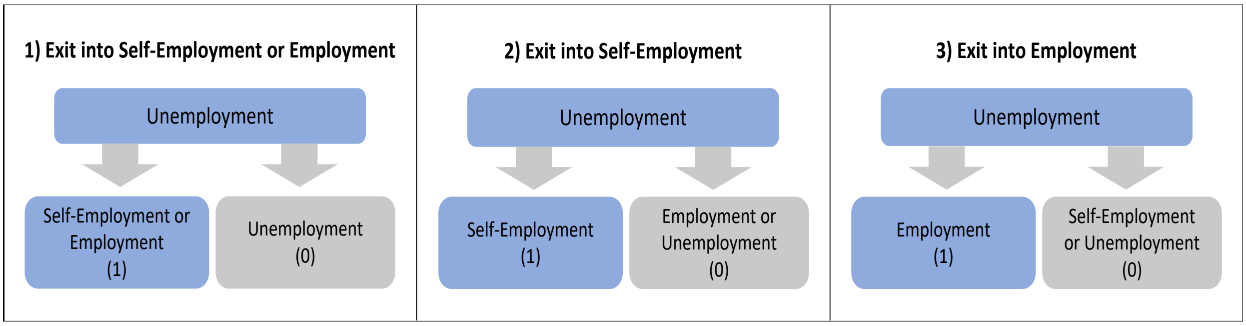
Estimation equation. We employ a non-parametric local polynomial estimation framework using a triangular kernel function, different polynomials as a sensitivity check, and a bandwidth that optimizes the mean squared error (MSE) as recommended by Cattaneo et al. (2019). Standard errors are clustered at UI entry date level to account for potential correlation in unobservable characteristics (Lee and Card, 2008).[27] 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. The first set of outcome variables intends 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 Section 4.1, 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 of zero if the individual remains unemployed or exits into the counterfactual state

[27] Our results stay essentially the same if we calculate standard errors based on the heteroskedasticity-robust plug-in residuals variance estimator without weights, corresponding to the Eicker-Huber-White (EHW) heteroskedasticity-robust standard errors that are recommended by Kolesár and Rothe (2018) for inference in RDDs with a discrete running variable. Nonetheless, we decided to show the more conservative clustered standard errors, which have been applied in similar RDDs by Rebollo-Sanz and Rodríguez-Planas (2020) and Fernandez-Navia (2020).

Figure 5: 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.

(Section 4.1, 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 D.1.

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) by the percentage change in the RR due to the reform (approximately 16.67%):

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

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

[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 of one and the *employment* outcome variables will always take the value of zero.

[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 for a maximum of five years. We guarantee that pre- and post-reform period spells potentially have the same maximum duration 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 the best available proxy. Self-employed individuals must choose a contribution basis 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 basis, 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: EUR 1,471.63. We define workers as being of high quality if they received a pre-unemployment monthly real wage above the median. If the probability that individuals who

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 once more. 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 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 D.2. The variables' detailed definitions can be inferred from Appendix B.4.

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 details are presented in Appendix D.

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

become (self-)employed are high quality workers increases due to the reform, this may indicate an increase in the (self-)employment quality.

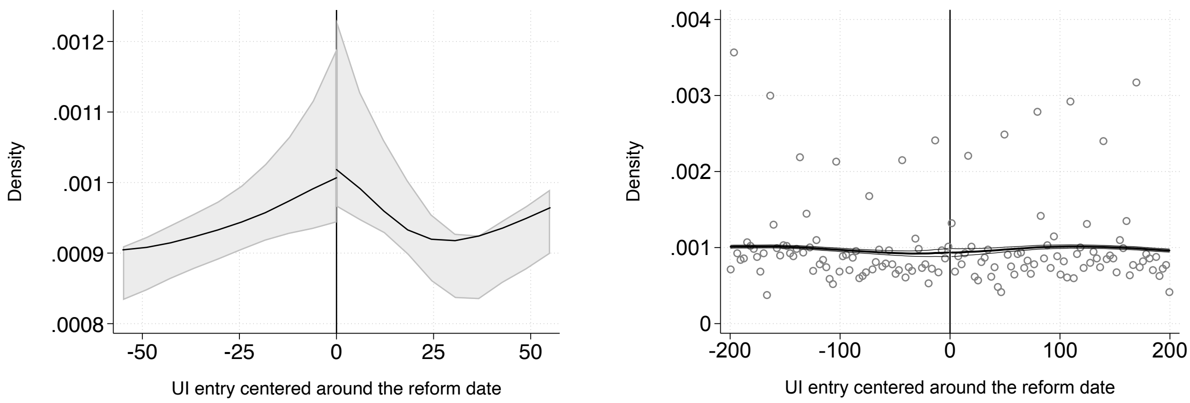
[32]Sector 1: Agriculture, extraction, primary manufacturing; Sector 2: Manufacturing and utilities; Sector 3: Construction; Sector 4: Trade; Sector 5: Transport and storage; Sector 6: Accommodation and food services; Sector 7: Information, communication, finance, insurance, real estate, and scientific 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 the inclusion of an immigrant variable defined by a person's nationality.

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 Appendix D.1 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, and so we find no visual evidence for *precise* manipulation.[34]

Figure 6: Continuity of the Running Variable

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



Notes: Figure (a) depicts the density of the running variable and its robust bias-corrected 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 based on MCVL 2005-2018 data.

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 6a plots the resulting density of the running variable and its robust bias-corrected 95% confidence intervals.[35] 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.[36] We plot the estimated density in Figure 6b using a bin size of three (results remain robust if different bin sizes are

[34] Complementary to our findings of visual continuity around the cutoff, Rebollo-Sanz and Rodríguez-Planas (2020) show that trends of monthly inflows into the UI system were similar during 2011 and 2012.

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

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

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.

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 D.2 shows the estimated reform effects on the covariates and their corresponding balancing plots in detail. We estimate a quadratic version of the running variable and include the remaining covariates on the right-hand side.[37] We find that 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. However, the estimates we present in the next section remain robust if we exclude immigrants. Hence, this slight imbalance does not affect our results.[38] 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 setup. The remaining 22 covariates are perfectly balanced which may also be inferred from the balancing plots (Appendix Figures D.3 to D.4). Overall, manipulation and balancing tests support the validity of our identification assumption.

5 Results

5.1 Reform Effects on the Extensive Margin

Our baseline results from local quadratic regressions without covariates are visualized in Figure 7. It plots the (discontinuous) cumulative exit probabilities before and after the cutoff date. The subfigures depict the effects on the cumulative probability of exiting from unemployment into (a) self-employment, (b) employment, and (c) self-employment or employment (i.e. *general 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 cumulative probability of exiting into self-employment (corresponding to the startup rate)[39] is consistently negative and is rather small on a short-term

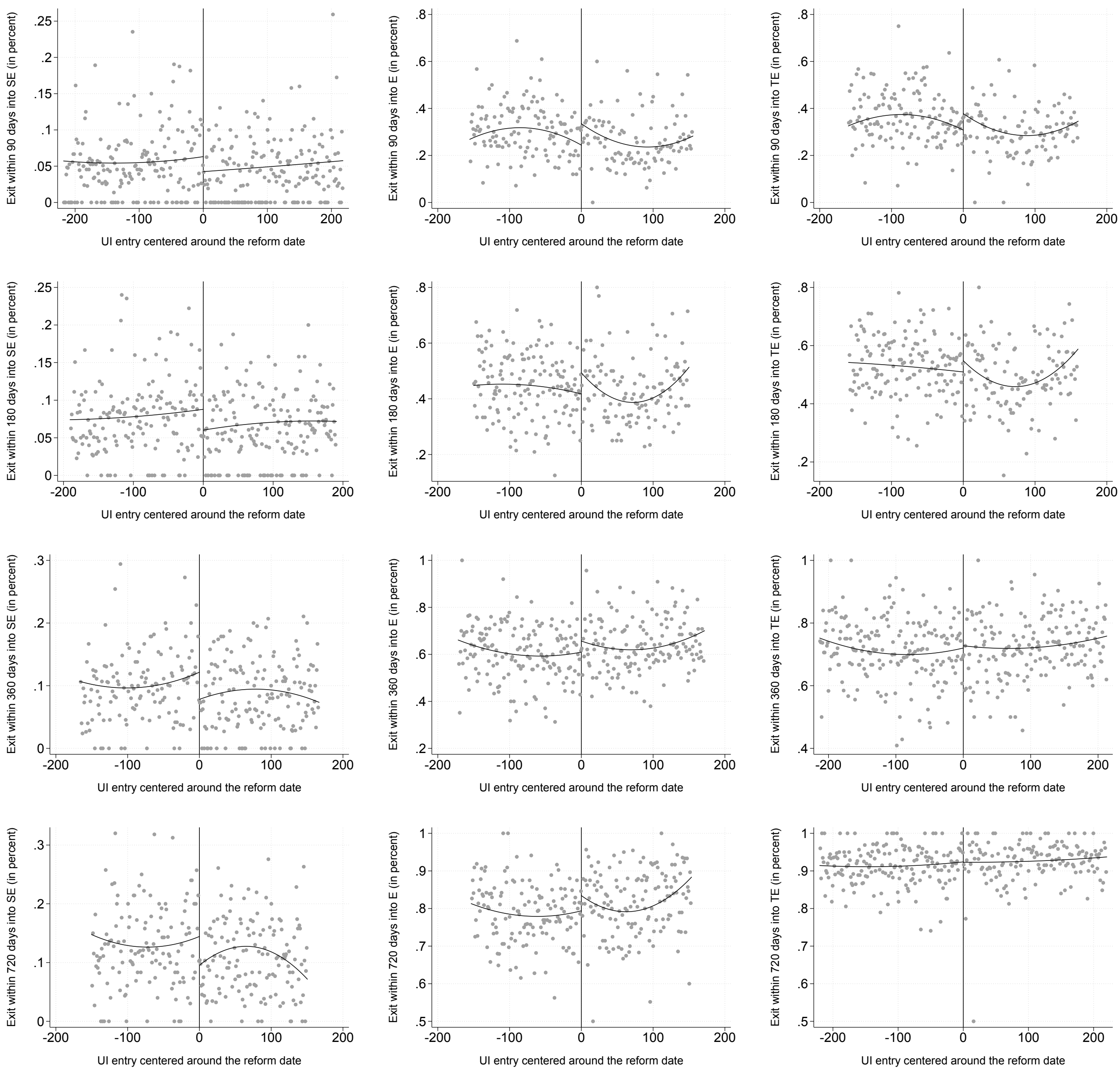
[37] Results remain robust if we exclude the covariates or if we choose a linear version of the running variable.

[38] We decided to include immigrants in our main setup because they make up an important share (16.5%) of self-employed individuals (Table 2).

[39] We explain this connection and why we prefer using cumulative rather than conditional exit probabilities as main

Figure 7: RD Plots by UI Exit State (Quadratic)

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



<u>*Notes:*</u> 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). Analogous graphics using linear and cubic specifications in the running variable look similar and are available from the authors upon request. <u>*Source:*</u> Authors' calculations based on MCVL 2005-2018 data.

basis but intensifies over time. By contrast, the cumulative probability of exiting into employment (corresponding to 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 similar if we use a linear or

outcome variables in Appendix E.1.

cubic polynomial.[40] Yet our analysis suggests that a second-order relationship between the running and outcome variable is appropriate, and thus we focus on quadratic (and cubic) relationships in the subsequent sections. Overall, we can confirm the visual findings of the raw data (compare Section 4) when conducting our RDD regression. We find a negative effect on the probability of exiting into self-employment and a positive effect on the probability of exiting into re-employment. These effects cancel each other out in the medium and long term, which means that the probability of exiting into self-employment or employment is not affected. Furthermore, the effect intensity varies over time, supporting our presumption of heterogeneous treatment effects.

Table 3 shows our estimated reform effects on the probability of exiting from unemployment into self-employment (SE, panel A) and on the probability of exiting into re-employment (E, panel B). Panel C refers to the estimated reform effect on the probability of exiting into self-employment or employment (SE or E), i.e. *general employment*. Alongside point estimates and p-values we show the estimated average reform effect on the outcome variable relative to its pre-reform mean (i.e. the relative effect size of a reduction in UI benefits). 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.

Our results on the startup rate in panel A of Table 3 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 percentage points (p.p.) in panel A1). It turns significant at the 10% level with a stronger magnitude (between -3.6 and -2.7 p.p. in panel A2) if we consider the 180-day period. This finding could be interpreted as 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. Our estimated negative effects on the startup rate are even stronger in the medium and long term (in panels A3 and A4, respectively). In the medium term our point estimate is significant at the 5% level when using a local quadratic regression without covariates (first row, panel A3). The significance level only slightly decreases if covariates are added (second row, panel A3). We find that the cut in UI benefits decreases the probability of exiting into self-employment in the medium term, on average, by 3.5 percentage points. Given a pre-reform self-employment mean probability of 9.6%[41], this corresponds to a decrease of 37%. In the cubic setting, the decrease corresponds to 46% and is significant at the 5% level even when covariates are added (last row, panel A3). Regardless of the polynomial order, estimates vary little if we add covariates. Overall, panel A3 suggests that lower UI benefits reduce the probability of exiting into self-employment by 35-50% (3.5-4.8 p.p.) in the medium term. Our long-term results in panel A4 point towards an even stronger negative effect on the startup rate in absolute terms (between -5.8 and -3.6 p.p.). In relative terms, the effect corresponds to a 31-50% decrease in the startup rate, similar to the reform effect in the medium term. Taken together, our results suggest a negative anticipation effect on the probability of exiting into self-employment in the short term and an even stronger negative effect in the medium and long term.

Conversely, in panel B of Table 3 we find consistently positive reform effects on the probability of

[40] Results for linear or cubic polynomials are available from the authors upon request.

[41] Row 3, column 2 in Table D.1 shows pre-/post-reform means of each outcome variable in our estimation sample.

Table 3: Reform Effects on the Extensive Margin

Outcome Variable	RD Estimate	Rel. Change	s.e.	p-value	Polynomial	Covs.	Bandwidth	N Left	N Right
(A) Self-Employment									
(A1) SE within 90 days	-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
(A2) SE within 180 days	-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
(A3) SE within 360 days	-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
(A4) SE within 720 days	-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
(B) Employment									
(B1) E within 90 days	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
(B2) E within 180 days	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
(B3) E within 360 days	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
(B4) E within 720 days	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
(C) Self-Employment or Employment									
(C1) SE or E within 90 days	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
(C2) SE or E within 180 days	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
(C3) SE or E within 360 days	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
(C4) SE or E within 720 days	-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 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 Table D.1. We use our RDD estimation sample (Section 4.1 describes the detailed sample restrictions). Results using a linear specification in the running variable are similar and available upon request.
Source: Authors' calculations based on MCVL 2005-2018 data.

exiting into re-employment. Within the first 90 and 180 days of unemployment (panels B1 and B2, respectively) our estimates are significant at the 5% level, regardless of the specification. Our point estimates indicate an average increase between 9.1 and 9.6 p.p. (7.6 and 8.6 p.p.) in the probability to get re-employed within the first 90 days (180 days) of unemployment, corresponding to a 31-33% (17-19%) increase in relative terms. In contrast to the reform effect on the startup rate, the effect on the job-finding rate is stronger in the short term (17-33%, panels B1 and B2) than in the medium (7-9%, panel B3) or long term (5-6%, panel B4). Adding to our evidence of heterogeneous treatment effects, the positive reform effect on the job-finding rate also turns insignificant if we follow the unemployment spell over a longer time period (panels B3 an B4).

From panel C1 of Table 3 we infer that the positive anticipation effect on re-employment surpasses the negative anticipation effect on self-employment, which means 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. Panel C2 shows that point estimates are halved and turn insignificant if we take the first 180 days of unemployment into consideration. Moreover, we find insignificant zero effects in the medium and long term (panels C3 and C4). As previously suggested by the visual findings of the raw data in Section 4, the negative effect on the startup rate and the positive effect on the job-finding rate cancel each other out 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 probability of exiting into self-employment is negative and its size intensifies in the medium and long term, whereas the effect on the probability of exiting into re-employment is positive and declines throughout the unemployment spell. Over different time horizons, the negative effect on self-employment (35-50%) is consistently stronger than the positive effect on employment (5-33%). The probability of exiting into self-employment or employment, i.e. *general employment*, is positively affected but only within the first 90 days of unemployment; we cannot confirm any medium- or long-term effects. 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. Since the reform leads to unintended consequences for self-employment, the *general employment* effect is smaller in the short term and nonexistent in the medium and long term. Thus, the isolated focus on the job-finding rate – the employment side of the coin – does not tell the full story, as the reform's negative effect on the startup rate is not taken into account. Considering both sides – self-employment and employment – is extremely important to correctly evaluate the reform's effect on the probability of exiting unemployment in general, i.e. its *general employment* effect.

5.1.1 Robustness Checks

The following subsection addresses the robustness of our estimated extensive margin effects in a concise way. We refer to Appendix E for thoroughly described robustness checks.

Cumulative vs. Conditional Exit Probabilities. In Appendix E.1, we explain why we focus on cumulative exit probabilities as main outcome variables rather than conditional exit probabilities. As our previous results are in line with findings based on conditional exit probabilities, we show that our outcome measures correspond to the notion of startup and job-finding rates.

Ignoring Self-Employment Leads to Bias. In Appendix E.2, we check the sensitivity of our estimates to the exclusion of data on individuals who exit into self-employment. The idea is that many researchers do not have access to data on self-employment and therefore have to restrict their sample in this respect.[42] We show that the estimated reform effect on the short-term job-finding rate (within the first 90 days of unemployment) is slightly upward biased if self-employment is excluded as a counterfactual outcome. Therefore, focusing on the short-term positive effect on the job-finding rate is not only a bad idea because it distracts from a much smaller *general employment* effect, but also because it overstates the effect on employment itself if self-employment is excluded.

Placebo Tests. As another robustness check, we analyze our extensive margin results using placebo tests. Appendix Table E.3 shows the results if we use a placebo treatment group of individuals whose RR did not drop after 180 days of UI benefit receipt because they either hit the ceiling or the floor of the UI benefit amount.[43] 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.[44] Consequently, this placebo test confirms the robustness of our estimated reform effects from Section 5.1.

Another placebo test we conduct consists of artificially changing the reform cutoff date. We use a notional reform 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 E.4-E.6. We find no evidence of a seasonal driven reform effect, since almost all estimated placebo effects are insignificant.[45] To conclude, placebo tests confirm the robustness of our main results.

No Impact of Other Reforms on Self-Employment. While policymakers did not intend to discourage self-employment through the reduction in UI benefits, other reforms adopted in 2013 have the explicit goal of promoting self-employment among young workers.[46] Since these reforms come with clear age criteria, we can infer individual eligibility from our data. In Appendix E.4 we test whether these self-employment reforms affect our results by interacting our UI reform cutoff

[42] Other researchers may restrict their sample on purpose in order to focus on a more homogeneous group, e.g. Rebollo-Sanz and Rodríguez-Planas (2020) exclude self-employed individuals from their sample.

[43] Table A.1 in Appendix A shows the minimum/maximum of UI benefits.

[44] 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.

[45] 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.

[46] For an overview of reforms, see Appendix A.6.

indicator with an eligibility indicator for the self-employment reforms. Our findings confirm that other reforms that took place in 2013 do not bias our estimated reform coefficients from Section 5.1.

Competing Risks Regressions. Finally, our estimation results could potentially be biased because our local polynomial regression framework does not take the duration structure of the data itself into account. A Competing Risks Regression (CRR) addresses this issue and thus could be the more suitable model candidate. In Appendix E.5, we show 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.

5.1.2 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).[47]

Appendix Table F.1 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, parents, 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[48] (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 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.

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

[48] 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 A.6.

There is also substantial heterogeneity in the reform effects on the short-term job-finding rate (within 180 days of unemployment). Table F.2 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 4 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 estimated duration elasticities for individuals who transition into self-employment are consistently negative, though statistically not significant. The estimated long-term UI and UE elasticities are similar and relatively large in absolute terms (between -1.5 and -1.2 in panels A1 and B1), whereas the short-term elasticities are closer to zero (between -0.5 and 0). Instead, the UI duration elasticity results for those who transition into re-employment (panel A2) are statistically significant and take values between 0.8 and 0.9. Their UE duration elasticity estimates in panel B2 are slightly smaller but still positive. Finally, as expected, the duration elasticity estimates regarding the union of both exit states (panels A3 and B3) are located between the elasticities with respect to self-employment and employment, yet always positive (0.3-0.7).[49]

Our duration elasticity results complement our previous findings by pointing to the following. Lower UI benefit levels appear 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 and Skandalis, 2019). Thereby,

[49] Results for linear and cubic polynomials are depicted in Tables F.4 to F.5.

Table 4: 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 F.3), 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 F.4 to F.5.
Source: Authors' calculations based on MCVL 2005-2018 data.

in line with Doris et al. (2020) the elasticity estimates regarding employment appear to be larger in absolute terms than common elasticity estimates which 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 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. In Appendix F.3 we conduct mean comparison tests of self-employment quality proxies in the pre- and post-reform period. While we find a significant increase in the high-skilled service sector that points to more *opportunity-driven* entrepreneurship in the post-reform period, increases in the trade, accommodation and food service sectors indicate that more individuals are also pushed into *necessity-driven* entrepreneurship. We also find that older individuals are less often self-employed than before, whereas the opposite can be observed with regard to the younger generation.[50] 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. Altogether, our descriptive evidence suggests that the dispersion in the quality of startups increased due to the reform.[51]

So far we have only considered correlations. In Table 5 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 Table 2).[52] 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.[53] 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

[50] Appendix Figure F.1 shows the composition of self-employment among different age groups pre- and post-reform.

[51] However, this is only descriptive evidence and could be caused by other reforms in 2013 which particularly encouraged young unemployed individuals to become self-employed. See Appendix A.6.2 and E.4 for more details.

[52] Detailed results for the linear and cubic specifications or for individuals who exit within the first 360 days of unemployment are provided upon request. As they look very similar, we refrain from showing them here.

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

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.

Table 5: 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. The median monthly real wage from social security data is EUR 1,471.63.
Source: Authors' calculations 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 D.2, 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.

A similar picture emerges if we look at our employment quality regressions in Table 5. When restricting our sample to individuals who exit to regular employment within the first 720 days

Table 6: 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

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 5 corresponds 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. Detailed results for the linear and cubic specifications or for individuals who exit within the first 720 days of unemployment are provided upon request.
Source: Authors' calculations based on MCVL 2005-2018 data.

of unemployment, our sample size is much larger (27,630 individuals, as illustrated in Table 2). Nonetheless, most of our point estimates are insignificantly different from zero. An increase in the job-finding rate is only statistically significant in the primary sector, indicating a slight decrease in employment quality.

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 6 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 has no significant effect on 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 limitations regarding 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 50%). Only individuals entitled to more than 180 days of UI benefits receipt were affected by this reform. This quasi-experimental setup 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, yet they attenuate and turn 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 can confirm an anticipation effect not only on the job-finding rate but also on the startup rate. Our findings differ also in terms of effect size: their RDD results point towards a LATE on the job-finding rate of 26%, while our corresponding estimates are in a range between 17-19%. 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 (5-33%). The probability of exiting into self-employment or employment, i.e. *general employment*, is positively affected but only within the short term; we cannot confirm any medium or long-term effects. Due to the reform's unintended consequences for self-employment, the *general employment* effect is much smaller compared to the effect on the job-finding rate. Thus, the isolated focus on the job-finding rate – the employment side of the coin – does not tell the full story, as the reform's negative effect on the startup rate is not taken into account. Considering both self-employment and employment is extremely important to correctly evaluate the reform's effect on the probability of exiting unemployment in general, i.e. its *general employment* effect. Furthermore, we find that the exclusion of data on individuals who exit into self-employment, e.g. due to bad data access or as a sample selection criterion, may lead to overestimation bias. Especially the short-term reform effect on the job-finding rate would be overstated, leading to wrong conclusions with regard to the reform's (general) employment effect.

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 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 -1.2 and -1.5) compared to those who exit into employment (0.8-0.9), implying that the joint elasticity into either exit state is in between (0.3-0.7). 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 had no significant impact on the quality of startups after unemployment, and only marginally worsened the quality of re-employment. Consequently, (self-)employment quality is barely affected by the cut in UI benefits.

Taking stock of the results derived in Camarero Garcia and Murmann (2020), too, 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 founders' actual unemployment duration. 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 (*anticipation effect*). 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, which is exactly what we discover in our short-term results. 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 fewer 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). The decrease in both 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 harder 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, as a result of the crisis on the heels of the COVID-19 pandemic, a reallocation shock may destroy many jobs (Barrero, Bloom, and Davis, 2020), potentially increasing the importance of transitions from unemployment to self-employment. Therefore, calls to optimize 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: Institutional Details

A.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".

A.2 Unemployment Insurance (UI)

UI Benefit Levels and Recipients. Table A.1 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 EUR 532.51 per month.

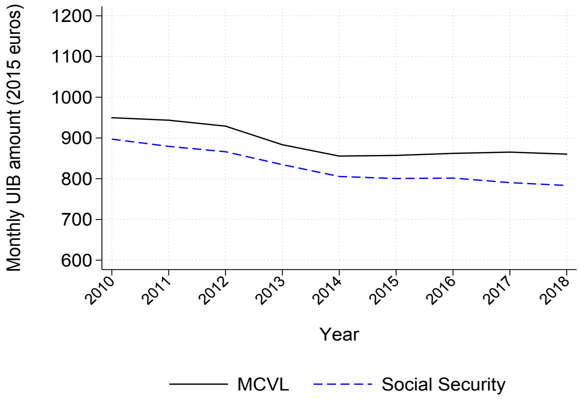
Table A.1: Minimum and Maximum UI Benefit Amount (valid 2010-2016)

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

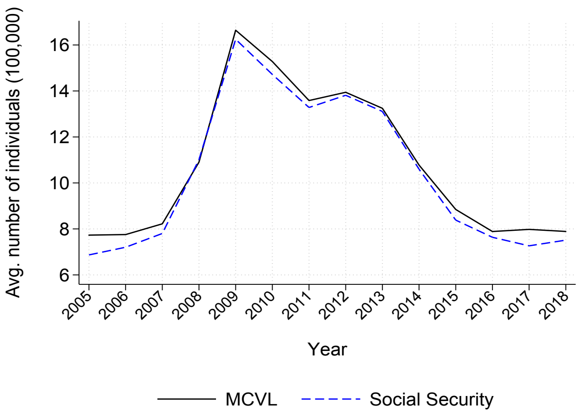
Notes: This table summarizes the computation of the legal maximum and minimum benefit amounts, depending on the family responsibilities (number of dependent children or descendants) and the value of the IPREM index in a given year.
Source: Authors' own illustration based on the SEPE (2019).

Figure A.1 illustrates the evolution of (a) yearly average UI benefit levels and the yearly average number of (b) UI and (c) UA beneficiaries. The solid line corresponds to our dataset, where the number of beneficiaries 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. Clearly, the evolution of all MCVL statistics move closely parallel to official statistics. Subfigure (a) points out that, during the period of our analysis, UI benefit levels were kept nominally constant in Spain.

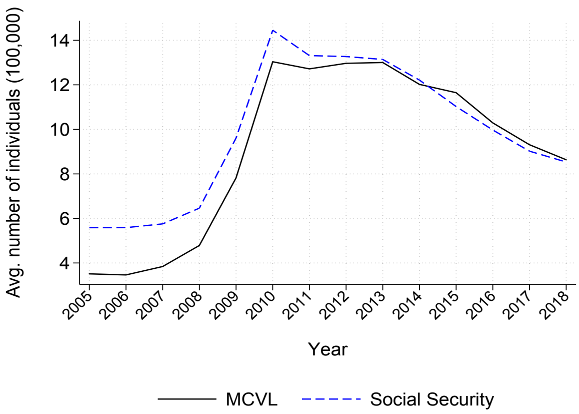
Figure A.1: Evolution of Unemployment Insurance Benefit Levels and Number of Beneficiaries



(a) UI Benefit Levels



(b) UI Beneficiaries



(c) UA Beneficiaries

Notes: The figures illustrate the evolution of (a) yearly average UI benefit levels and the yearly average number of (b) UI and (c) UA beneficiaries. The solid line corresponds to our dataset, where the number of beneficiaries 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 based on MCVL 2005-2018 data; and official statistics by Spain's Ministry of Labor (2020).

Option Right. 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, and 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 obtain any 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.

Part-Time Employment. 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, and 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 basis in both the lost and ongoing part-time contracts (SEPE, 2019).

Penalties. 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).

A.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.[54] 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).

[54]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.

A.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 basis 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*), 2.20% of his or her income is added to the minimum contribution basis. 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 that 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.

A.5 Budgetary Adjustments and Public Sector 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 sector workers until 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 and 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 and Uriel Jiménez, 2016).

When we include public sector 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 sector workers in the months right after the reform was implemented. The discontinuities disappear when we exclude public sector workers, thus fulfilling our identification assumptions.

A.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.

A.6.1 Unemployment Insurance System Reforms

In general, our UI entry date accounts for these reforms.[55] 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.

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

A.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 E.4 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).
- **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.

B Appendix: Data and Variables

B.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.[56]

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 us to detect all individuals who are added because they have been registered with the social security authorities, 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.[57] For more information on the Spanish social security data and its availability, we refer to the Dirección General de Ordenación de la Seguridad Social.

B.2 Other Data

Macroeconomic indicators 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 3.2. Our indicators are drawn from the *Selected indicators for*

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

Spain of the OECD (2018)[58] and the INE (2018)[59]. Official statistics on the number of beneficiaries and benefit levels have been extracted from Spain's Ministry of Labor (2020)[60].

B.3 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.

B.3.1 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) so 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*.

B.3.2 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*.

B.4 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***".

B.4.1 Outcome Variables

- **Extensive margin measures**: This is a set of binary outcome variables which take the value of one if individual i becomes self-employed, employed, or either one of them within

[58]OECD data for Spain can be retrieved from: `https://data.oecd.org/spain.htm`
[59]INE data for Spain can be retrieved from: `https://www.ine.es/dyngs/INEbase/en/listaoperaciones.html`
[60]Unemployment benefits statistics from Spain's Ministry of Labor (2020) can be retrieved from: `http://www.mitramiss.gob.es/estadisticas/PRD/welcome.htm`

a certain amount of days. The variable takes the value of 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.

B.4.2 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. We also add age squared.

– ***Education level***: Lower education, medium education, and higher education.[61]

– ***Presence of children dummy***: Presence of children in the household (1), no presence of children in the household (0).

– ***Immigrant dummy***: Immigrant (1), no immigrant (0). We define an immigrant as a person born in a country other than 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.[62]

  – ***Permanent contract dummy***: As specified above.

  – ***Sector of activity indicators***: As specified above.

- Unemployment characteristics:

  – ***Local unemployment rate***: Quarterly unemployment rate at province level.[63]

  – ***Potential benefit duration (PBD)***: Individuals' potential UI benefit duration in months.

[61]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).

[62]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 occupation includes 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).

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

C Appendix: Descriptive Analysis

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 (*INE*). 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.

Table C.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

Table C.1: Personal Characteristics

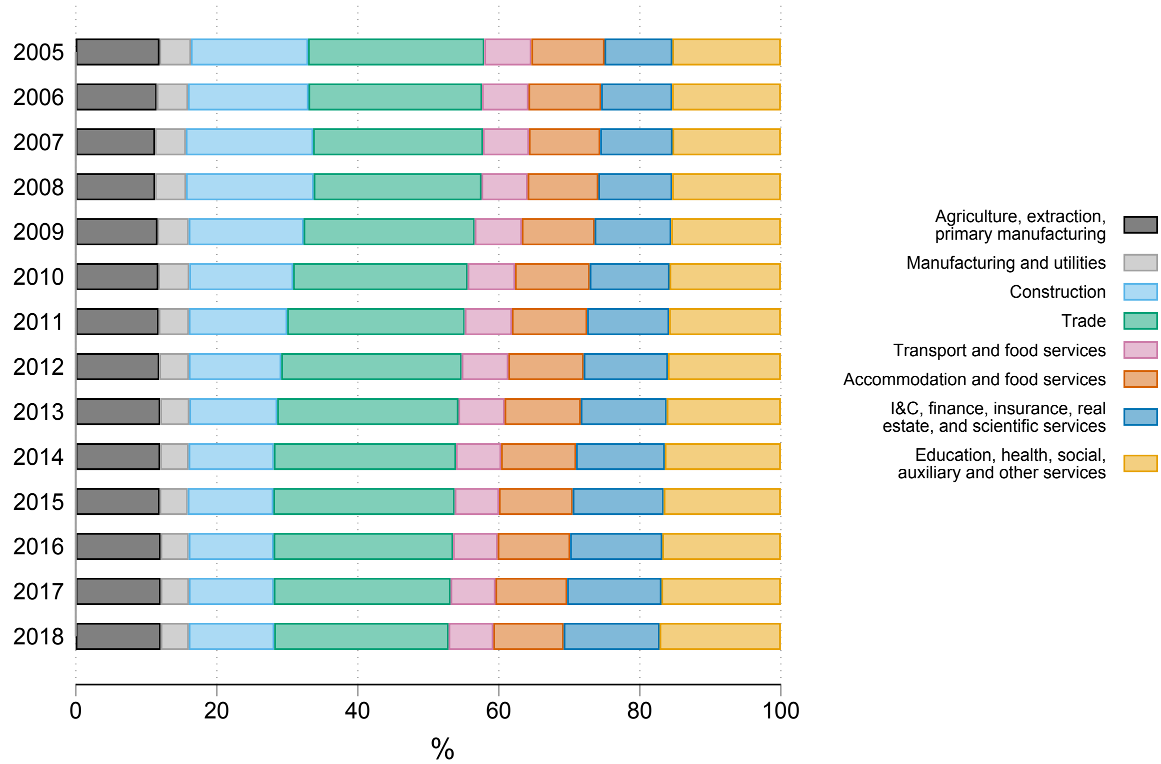
	Self-Employment		Employment		Total Sample	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Female	0.352	(0.478)	0.471	(0.499)	0.465	(0.499)
Age (years)	43.760	(12.016)	36.884	(12.261)	38.190	(12.719)
Lower education	0.632	(0.482)	0.599	(0.490)	0.623	(0.485)
Medium education	0.239	(0.427)	0.252	(0.434)	0.244	(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 based on the 2005-2018 MCVL data.

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, Appendix C 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.[64]

Figure C.1: Sector Distribution of the Self-Employed

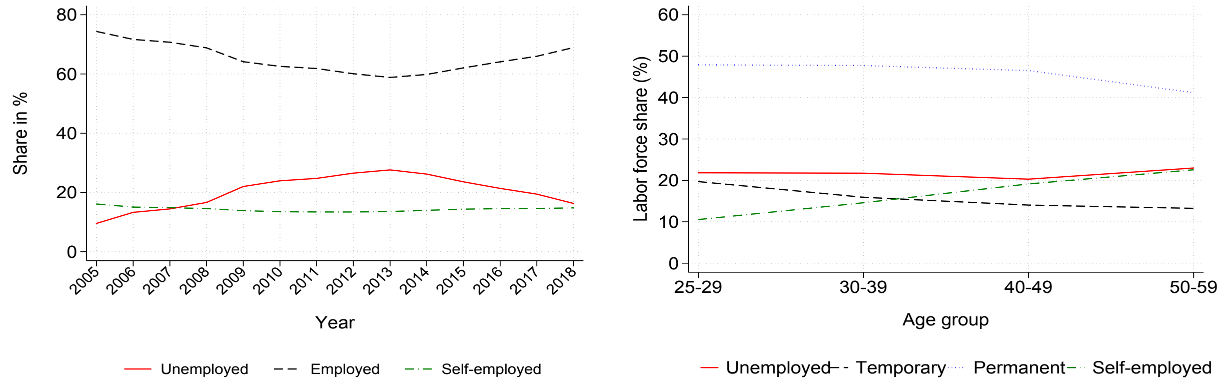


Notes: This figure illustrates the composition of self-employment in Spain, with respect to the sector variable in each year. The sample is restricted to individuals who are 25 to 52 years old.
Source: Authors' calculations based on MCVL 2005-2018 data.

Labor Force. The composition of the labor force is plotted in Figure C.2a. 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 C.2b 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

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

Figure C.2: The Spanish Labor Force



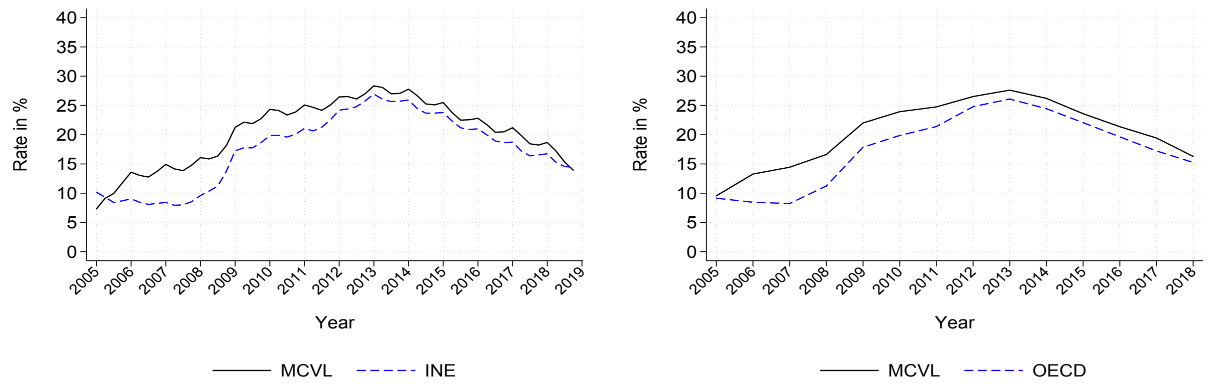
(a) Composition **(b)** Age Distribution

Notes: Figure (a) 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. Figure (b) 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 based on MCVL 2005-2018 data and official statistics provided by INE (2018) and OECD (2018).

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).[65]

Evolution of the Spanish Labor Market. In Figure C.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

Figure C.3: Unemployment Rate

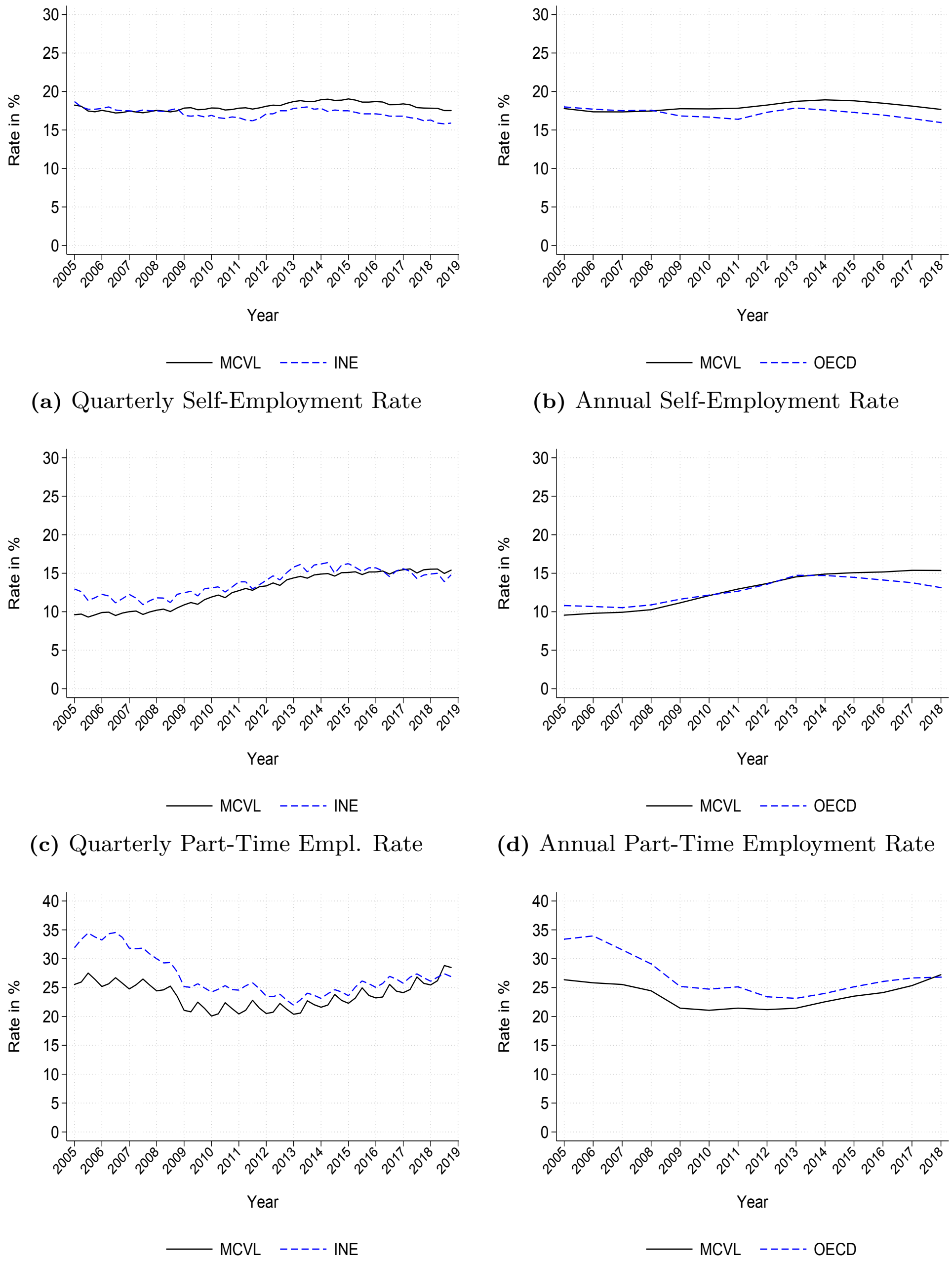


(a) Quarterly **(b)** Yearly

Notes: Figure (a) illustrates the evolution of the unemployment rates in Spain from 2005 to 2018 on a quarterly basis. Figure (b) 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 based on MCVL 2005-2018 data and official statistics provided by INE (2018) and OECD (2018).

[65] 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.

Figure C.4: (Self-)Employment Rates



(a) Quarterly Self-Employment Rate

(b) Annual Self-Employment Rate

(c) Quarterly Part-Time Empl. Rate

(d) Annual Part-Time Employment Rate

(e) Quarterly Temporary Empl. Rate

(f) Annual Temporary Employment Rate

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

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, while 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.[66] In spite of these differences, the computed

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

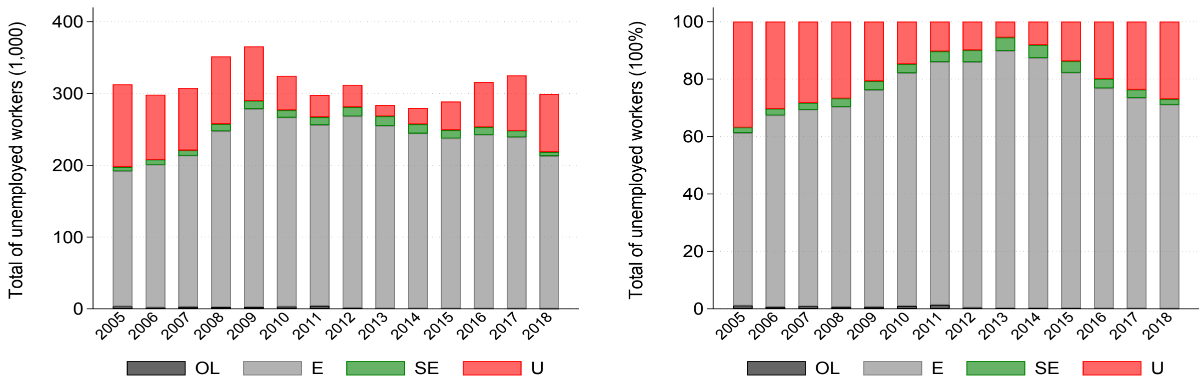
unemployment rate using MCVL data is very similar to the quarterly unemployment rate reported by INE (left panel figure) and also matches OECD's annual unemployment rate (right panel figure).

Concerning the self-employment rate, measured in terms of total employment, Figure C.4 confirms that our data cleaning process and the construction of our dataset from the MCVL data enable us to match (a) quarterly statistics from INE, as well as (b) annual statistics from OECD data. Specifically, it shows that self-employment has been slowly rising until reaching its peak in 2014 at nearly 20% and then declining again.

Also our calculated part-time employment rates, in subfigures (c)-(d), and temporary employment rates, in subfigures (e)-(f), match official statistics quite well. While the part-time rate has continuously increased from 10% in 2005 to 15% by 2018, the temporary employment rate reflects a U-shaped 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 2 in the main text depicts the yearly composition of transitions from unemployment in Spain. It illustrates that the share of individuals who transition from unemployment to (self-)employment remains relatively stable during the years surrounding the 2012 labor market reform. Even though the share of individuals who transition to self-employment is relatively larger around the reform than at the beginning of the sample period, the outflows from unemployment are clearly dominated by employment. Moreover, Figure C.5 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.

Figure C.5: Composition of Inflows into Unemployment



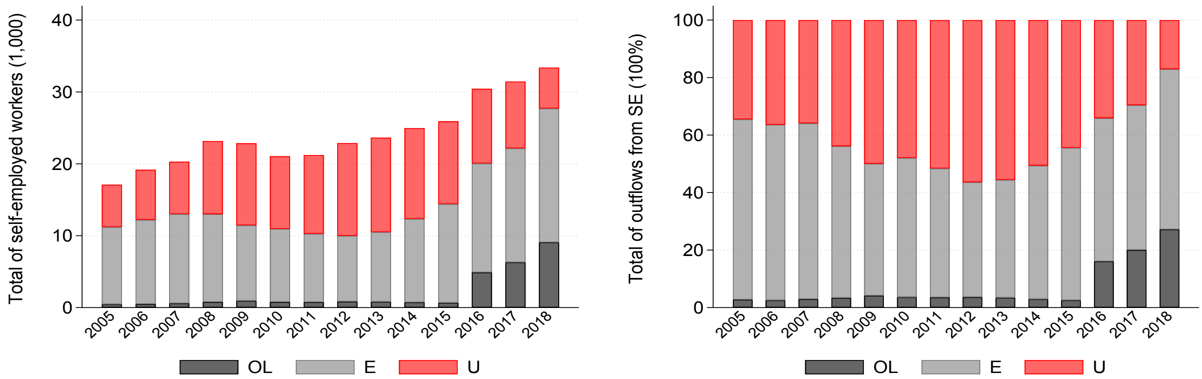
(a) Yearly Inflows **(b)** Yearly Inflows in %

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 based on MCVL 2005-2018 data.

At first glance, the channel from unemployment into self-employment seems negligible. However, if we examine it from the perspective of self-employment inflows, the picture changes tremendously. Figure 3 in the main text illustrates the yearly inflows to self-employment in Spain, in both (a) absolute and (b) relative terms, excluding the stock of self-employed individuals. It 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. Given that Spain's self-employment rate is among the highest in the EU – it varied between 16.4% and 17.9% during the 2010s (OECD, 2018) – the inflow from unemployment into self-employment is important. We find that it makes up 30-50% of all new self-employed individuals every year. 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 2 in the main text), there are usually job spillovers, i.e. most founders have employees. Since startups can be engines for economic growth, 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.

Figure C.6 shows the yearly outflows from self-employment excluding the self-employment stock dimension. In general outflows from self-employment target either employment or unemployment. It is not surprising that during the crisis period self-employment outflows are especially dominated by transitions into unemployment. Inactivity becomes more prevalent by the end of the sample period, indicating that most individuals already exhausted their benefit entitlement.

Figure C.6: Composition of Outflows from Self-Employment Excl. Stocks



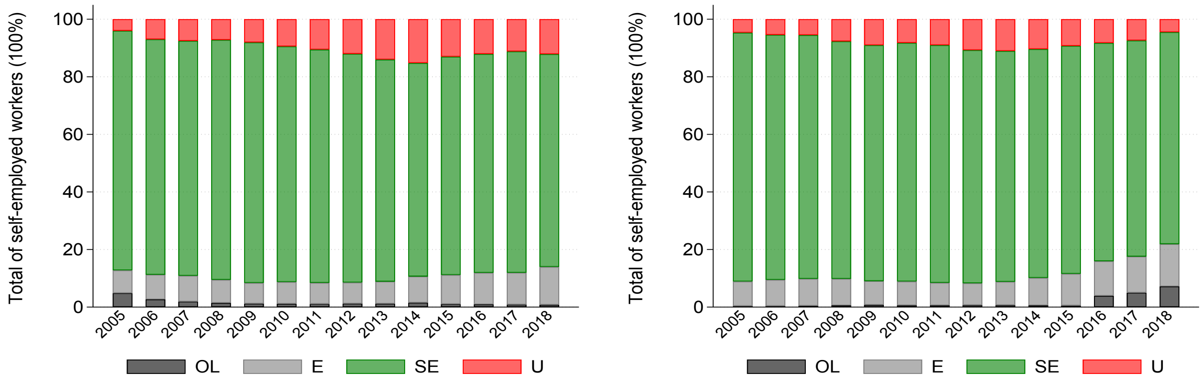
(a) Yearly Outflows

(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 flip side of the coin: the inflows are shown in the main text in Figure 3.
Source: Authors' calculations based on MCVL 2005-2018 data.

Figure C.7 shows the yearly inflows and outflows including the self-employment stock dimension. The graphs show that around 80% of the self-employed remain in self-employment in the following year (less during the crisis period). The left-hand figure confirms 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 C.7: Composition of Self-Employment Inflows and Outflows Incl. Stocks

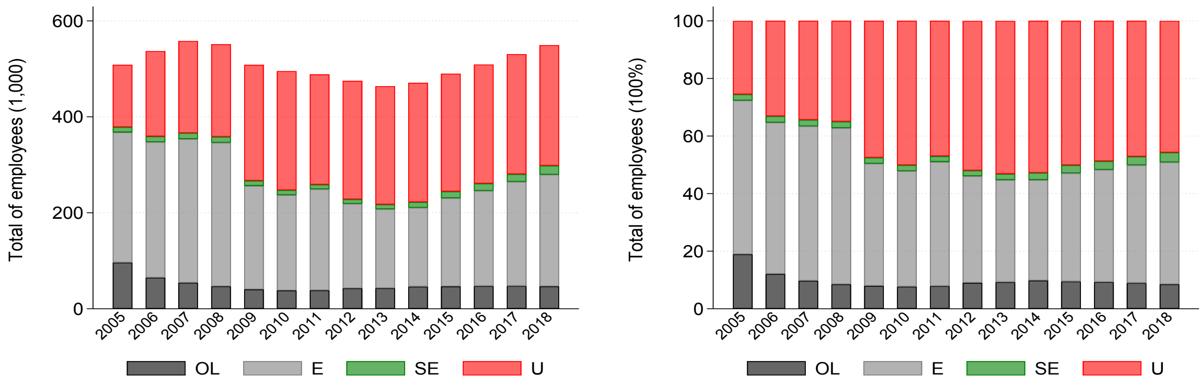


(a) Yearly Inflows in % **(b)** Yearly Outflows in %

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 3 for the composition of inflows into SE excluding stocks.
Source: Authors' calculations based on MCVL 2005-2018 data.

It is worth noting that the role of self-employment for the inflows into employment (Figure C.8) appears not to change much over time. This is also true for the outflows from employment to self-employment (Figure C.9), but is different to the patterns observed when analyzing the outflows from unemployment.

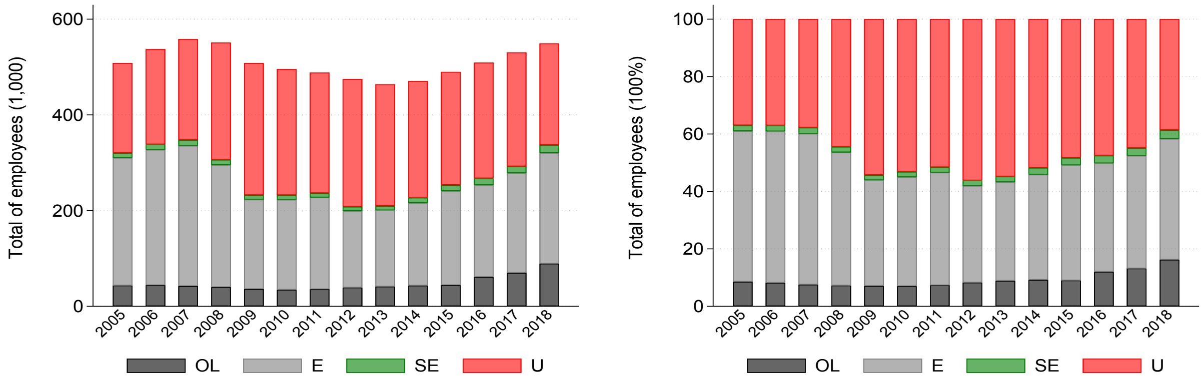
Figure C.8: Composition of Inflows into Employment



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

Notes: These figures illustrate the yearly composition of transitions to Employment (E) (inflows) in Spain, in both (a) absolute and (b) relative 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.
Source: Authors' calculations based on MCVL 2005-2018 data.

Figure C.9: Composition of Outflows from Employment

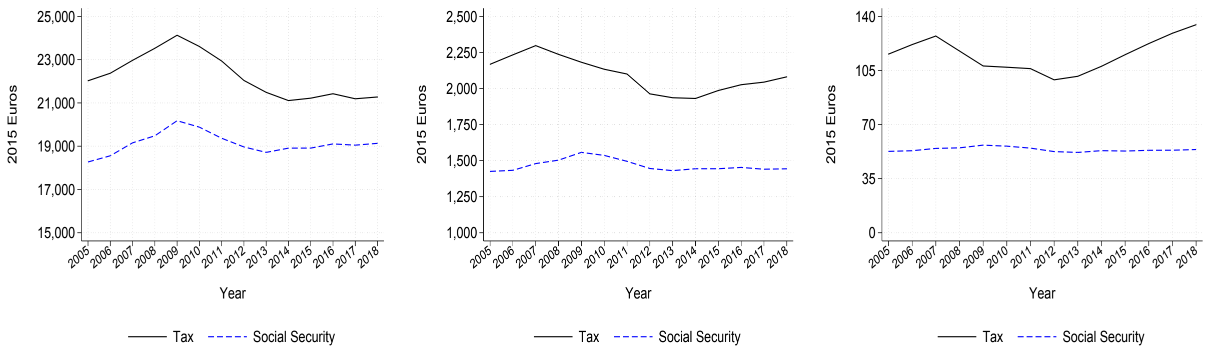


(a) Yearly Outflows **(b)** Yearly Outflows in %

Notes: These figures illustrate the yearly composition of transitions from Employment (E) (outflows) in Spain, in both (a) absolute and (b) relative 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.
Source: Authors' calculations based on MCVL 2005-2018 data.

Earnings. Figure C.10 compares the evolution of average annual, monthly, and daily real earnings from tax and social security data. Earnings from both sources move parallel to one another: annual 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. Monthly and daily average earnings evolve similarly but with a less pronounced pattern in the social security data. 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. Appendix C 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.

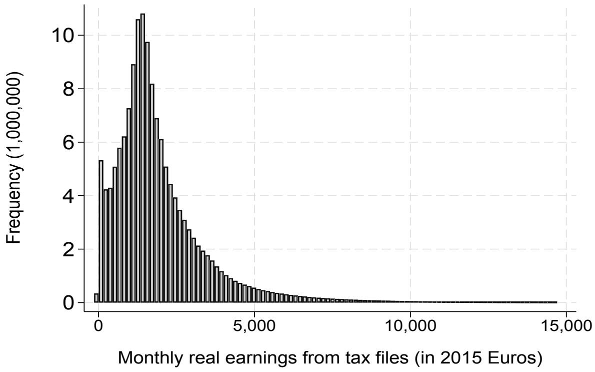
Figure C.10: Evolution of Average Annual, Monthly, and Daily Real Earnings



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

Notes: These figures illustrate the evolution of average (a) annual, (b) monthly and (c) daily real earnings in Spain, 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 based on MCVL 2005-2018 data.

Figure C.11: Distribution of Monthly Earnings (Tax Data)

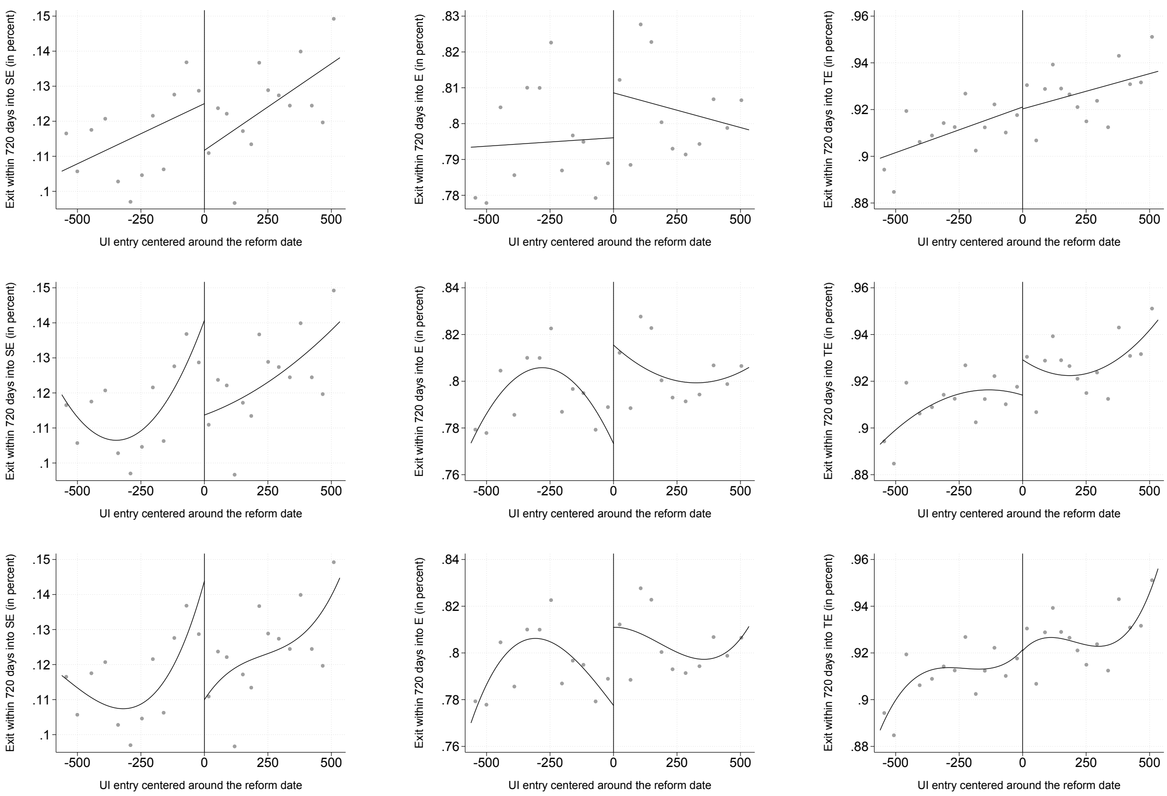


Notes: This figure illustrates the distribution of monthly real earnings in Spain with a mean value of EUR 1,981.81 and a median of EUR 1,564.67, according to the tax files. The sample is restricted to individuals who are 18 years of age or older.
Source: Authors' calculations based on MCVL 2005-2018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

D Appendix: RDD Analysis

Figure D.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 sector firm (see Section 4.1 for a description of detailed sample restrictions). Figure 4 shows the main effects using only a cubic polynomial.
Source: Authors' calculations based on MCVL 2005-2018 data.

Table D.1: 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 based on MCVL 2005-2018 data.

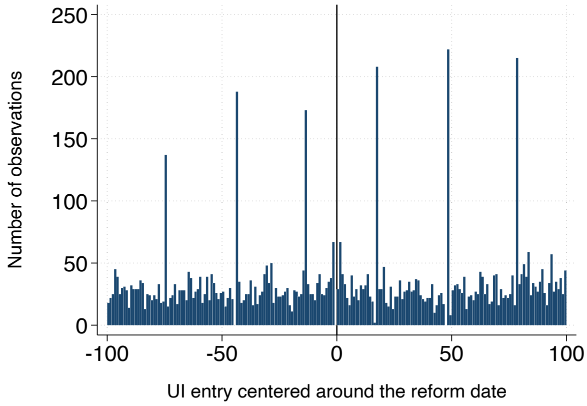
Table D.2: Summary Statistics – Covariates

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 based on MCVL 2005-2018 data.

D.1 Continuity of the Running Variable

Figure D.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 detect any visual evidence of *precise* manipulation. The histogram is constructed using the `rddensity` routine in Stata (Cattaneo et al., 2018).

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

D.2 Balancing Tests

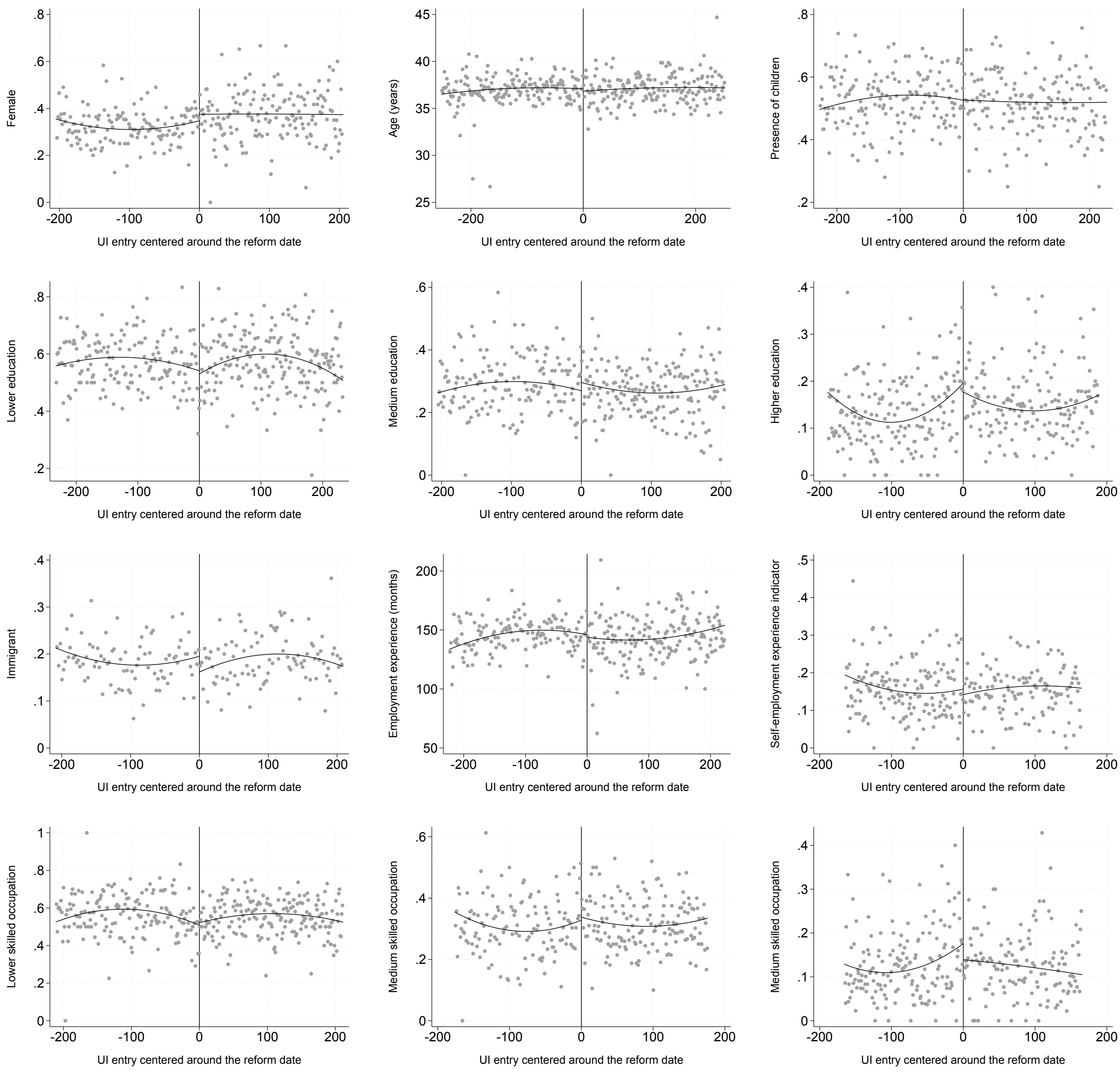
Table D.3: Balancing Table (quadratic, including all covariates)

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 D.2. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).

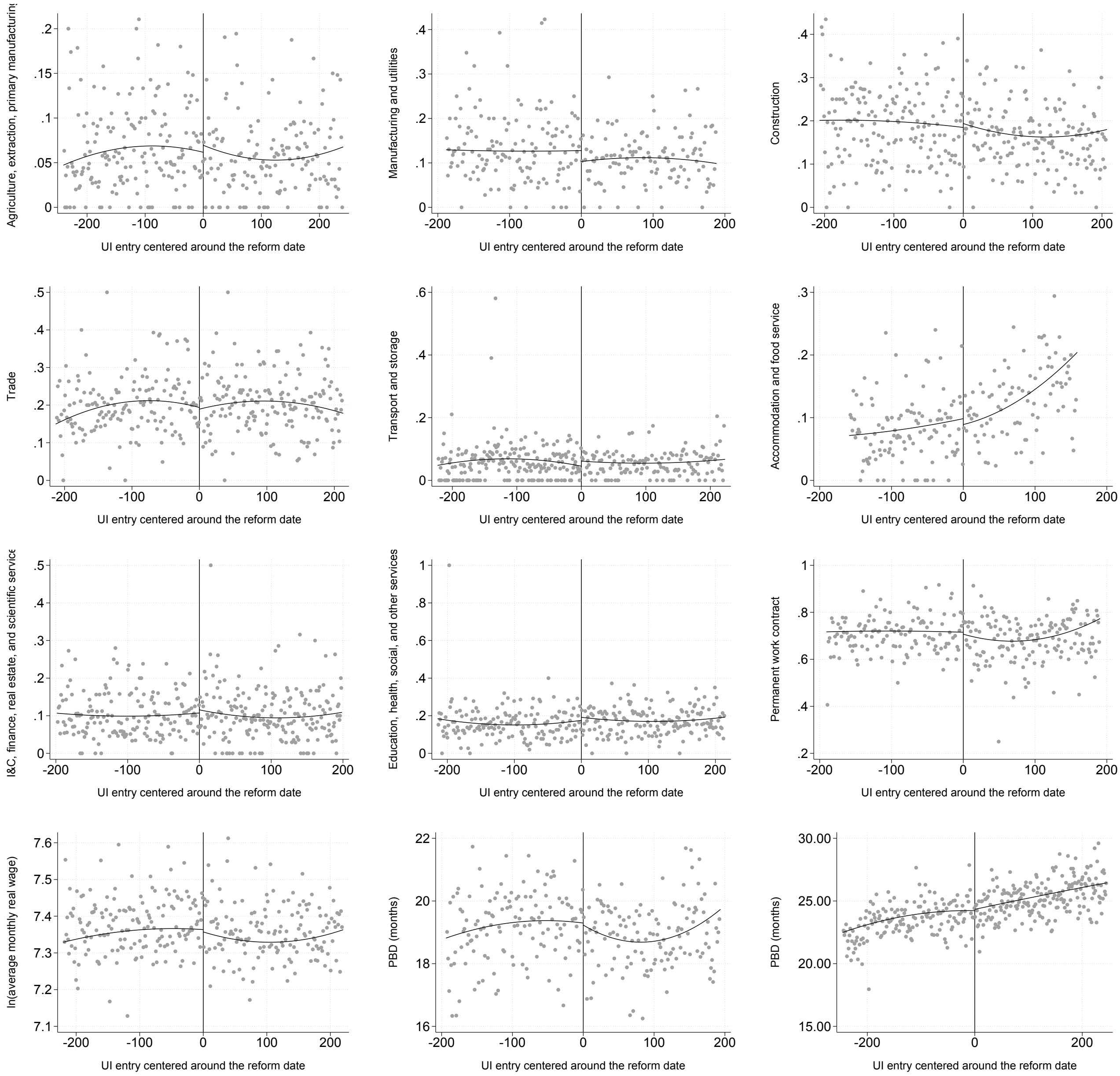
Source: Authors' calculations based on MCVL 2005-2018 data.

Figure D.3: 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 based on MCVL 2005-2018 data.

Figure D.4: 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 based on MCVL 2005-2018 data.

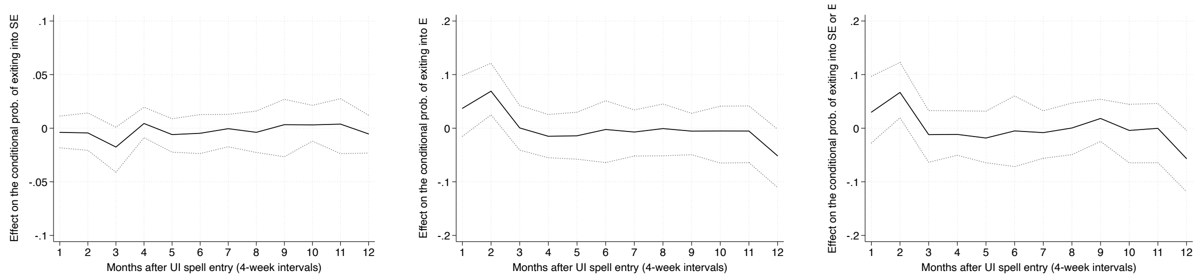
E Appendix: RDD Robustness Checks

E.1 Reform Effect on the Conditional Probability of Exiting into (Self-)Employment

In our empirical analysis, the outcome is the probability that the unemployment spell ends by a given time with an exit either to self-employment or employment. We refer to these cumulative exit probabilities as the *job-finding rate* and *startup rate*. In a strict sense these terms could be somewhat misleading because in labor economics, the job-finding rate is used as a synonym to the job-finding hazard, i.e. the conditional probability of finding a job at a given point in the unemployment spell, and analogous for the startup hazard. The cumulative probability of exiting into destination d (either self-employment or employment) by a given time depends on both the job-finding hazard and the startup hazard through the survivor function. It follows that if, for example, the UI reform increased the job-finding hazard without affecting the startup hazard, the reform changed the cumulative probability of exiting into self-employment nevertheless. Thus, our estimated reform effect on the cumulative probability of exiting into self-employment cannot tell us whether the startup hazard actually changed or not. In the following, we justify that the conclusions from our analysis are valid and that our terminology is useful.

Figure E.1: Reform Effect on the Conditional Exit Probabilities

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



Notes: The outcome variables are binary and indicate whether the person transitioned into a (self-)employment spell in month m, measured in four-week intervals after unemployment entry. The solid line corresponds to the local polynomial point estimates, calculated using the MSE-optimal bandwidth suggested by Calonico et al. (2014) and a triangular kernel. Dotted lines correspond to robust bias-corrected 95% confidence intervals. We use our RDD estimation sample (Section 4.1 describes the detailed sample restrictions).
Source: Authors' calculations based on MCVL 2005-2018 data.

Figure E.1 depicts our re-estimated extensive margin reform effects using the conditional exit probabilities as outcome variables, i.e. startup hazard and job-finding hazard. The outcome variables are binary and indicate whether the person transitioned into a (self-)employment spell in month m, measured in four-week intervals after unemployment entry. In other words, the outcome is set missing if the person is no longer unemployed and already switched into (self-)employment. As a result the sample size changes in every estimated regression. We find that the reform effect on the startup hazard tends to be negative, especially three months after UI entry. In contrast, the reform significantly increases the job-finding hazard two months after UI entry. Thus, we find that the conditional probability of exiting into self-employment is reduced, while the conditional probability of finding a job is increased due to the reform. Both of these reform effects emerge roughly at the same time, before the actual drop in an UI recipient's replacement rate kicks in. This finding confirms that UI recipients anticipate the reduction in the replacement rate (*anticipation effect*) and adjust their search behavior already before the actual benefit reduction takes place. The magnitude of the plotted point estimates coincides with our main results when using cumulative exit probabilities as outcome measure (compare Table 3).

Our graphical evidence suggests the following. Firstly, the timing of reform effects is surprisingly similar with respect to both self-employment and employment. Thus, tracking and differentiating the exact point during the UI spell when the startup hazard and the job-finding hazard are affected seems negligible, as they coincide. Secondly, the direction and magnitude of our estimated reform effects on the conditional exit probabilities completely coincide with our main findings from Section 5.1, using cumulative exit probabilities as outcome variables instead. Given that our estimated effects on the cumulative exit probabilities mirror the effects on startup hazard and job-finding hazard, it would be redundant to show results for both outcome measures. Lastly, using conditional exit probabilities as outcome variables involves negative side effects when it comes to power and comparability. The job-finding hazard is defined by the probability that an individual finds a new job in the interval $[m-1, m)$ given that he or she is still unemployed at time $m-1$ (the length of the ongoing unemployment spell). Conditioning on the unemployment status in $m-1$ implies that observations after the transition into (self-)employment ($t > m$), are dropped, as opposed to the cumulative exit probability which indicates outcomes in $t > m$ as one. As a result, the sample size erodes in the conditional exit probability case. The erosion is stronger, the longer UI spells are tracked over time, leading to potential power issues and less comparable results than in the setup with cumulative exit probabilities as outcome variables.

Altogether, we are convinced that the conclusions from our analysis with cumulative exit probabilities are just as valid, as in a setting with startup hazard and job-finding hazard as outcomes. Given the benefits in terms of a larger and more consistent sample, as well as our finding that reform effects on conditional exit probabilities coincide with effects on cumulative probabilities in terms of magnitude and direction, we decided to focus on cumulative exit probabilities in our main analysis and refer to them as corresponding to *startup* and *job-finding rates*.

E.2 The Exclusion of Self-Employment as Sample Selection Criterion

In Table E.1 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 E.2).

As can be seen from our illustration of extensive margin outcome variables in Figure 5, 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) in Table E.1.

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 which is equal to one. Since the startup rate is negatively affected by the reform (compare

Table E.1: 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 D.1. 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 based on MCVL 2005-2018 data.

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). This suggests that the exclusion of data on self-employment is not an innocuous sample selection criterion.

Table E.2: 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 D.1. We use our RDD estimation sample (see Section 4.1 for a description of detailed sample restrictions).
Source: Authors' calculations based on MCVL 2005-2018 data.

E.3 Placebo Tests

Table E.3: 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 a (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 based on MCVL 2005-2018 data.

Table E.4: Placebo Test for Self-Employment – Notional 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 notional 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 byCalonico 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 based on MCVL 2005-2018 data.

Table E.5: Placebo Test for Employment – Notional 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 notional 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 based on MCVL 2005-2018 data.

Table E.6: Placebo Test for Self-Employment or Employment – Notional 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 notional 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 based on MCVL 2005-2018 data.

E.4 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 35 and men younger than 30. Details about the reforms may be inferred from Appendix A.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 and 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 E.1.

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

If this were 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 E.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.

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

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 E.7. 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.

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 E.2 if we use the self-employment exit indicator as an

Table E.7: 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 based on MCVL 2005-2018 data.

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 absent 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 were huge, the inconsistency of the estimated UI reform effect would be very small.[67] Thus, our estimated UI reform effects on self-employment can be considered to be very conservative.

Second, we consider potential inconsistencies in the employment context. According to equation E.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 E.8. 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 E.9. 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.

[67] 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 E.8: 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 based on MCVL 2005-2018 data.

Table E.9: 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 based on MCVL 2005-2018 data.

E.5 Competing Risks Regression

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:

$$\lambda_k(t; \boldsymbol{X}) = \lambda_{k0}(t) \exp(\boldsymbol{X}^T \beta_k) \tag{E.3}$$

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, and 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.

Table E.10 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. Considered in more detail, our estimates for both self-employment (panel A) and employment (panel B) are robust to the inclusion of predetermined covariates and duration dependence controls. The effects' sizes seem to be stable over different time horizons, i.e. heterogeneity over time vanishes in the competing risks framework. Lastly, the effects on the probability of exiting into the union of self-employment and employment (panel C) are rather insignificant and close to zero. 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 E.2 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 E.10: Competing Risks Regression Results for (Self-)Employment

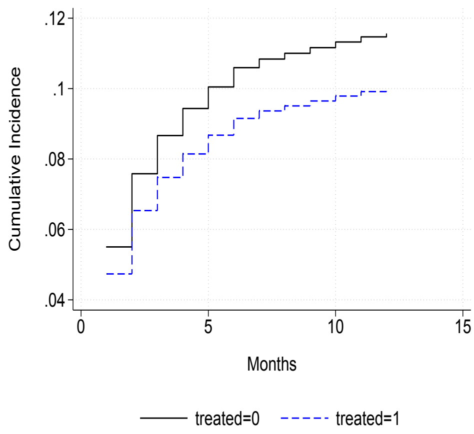
Event of Interest	Hazard Ratio	Estimate	Rate	s.e.	Polynomial	Covs.	Dur. Dep.
(A) Self-Employment							
(A1) SE within 90 days	0.851	-0.162	-14.9%	0.108	quadratic	✓	
	0.866	-0.144	-13.4%	0.105	quadratic	✓	✓
	0.787	-0.240	-21.3%	0.148	cubic	✓	
	0.774*	-0.256	-22.6%	0.144	cubic	✓	✓
(A2) SE within 180 days	0.863	-0.148	-13.7%	0.097	quadratic	✓	
	0.858	-0.153	-14.2%	0.097	quadratic	✓	✓
	0.793*	-0.232	-20.7%	0.133	cubic	✓	
	0.775*	-0.255	-22.5%	0.133	cubic	✓	✓
(A3) SE within 360 days	0.857*	-0.154	-14.3%	0.092	quadratic	✓	
	0.865	-0.145	-13.5%	0.093	quadratic	✓	✓
	0.794*	-0.230	-20.6%	0.126	cubic	✓	
	0.794*	-0.231	-20.6%	0.127	cubic	✓	✓
(A4) SE within 720 days	0.873	-0.136	-12.7%	0.089	quadratic	✓	
	0.875	-0.134	-12.5%	0.090	quadratic	✓	✓
	0.807*	-0.214	-19.3%	0.122	cubic	✓	
	0.797*	-0.227	-20.3	0.123	cubic	✓	✓
(B) Employment							
(B1) E within 90 days	1.035	0.034	3.5%	0.045	quadratic	✓	
	1.032	0.031	3.2%	0.039	quadratic	✓	✓
	1.076	0.073	7.6%	0.061	cubic	✓	
	1.063	0.061	6.3%	0.054	cubic	✓	✓
(B2) E within 180 days	1.088**	0.084	8.8%	0.036	quadratic	✓	
	1.046	0.045	4.6%	0.039	quadratic	✓	✓
	1.098*	0.093	9.8%	0.050	cubic	✓	
	1.085	0.082	8.5%	0.053	cubic	✓	✓
(B3) E within 360 days	1.067*	0.065	6.7%	0.034	quadratic	✓	
	1.050	0.049	5.0%	0.038	quadratic	✓	✓
	1.063	0.061	6.3%	0.047	cubic	✓	
	1.085	0.081	8.5%	0.052	cubic	✓	✓
(B4) E within 720 days	1.069**	0.067	6.9%	0.033	quadratic	✓	
	1.048	0.047	4.8%	0.038	quadratic	✓	✓
	1.070	0.068	7.0%	0.045	cubic	✓	
	1.083	0.079	8.3%	0.051	cubic	✓	✓
(C) Self-Employment or Employment							
(C1) SE or E within 90 days	0.992	-0.008	-0.8%	0.040	quadratic	✓	
	0.960	-0.041	-4.0%	0.025	quadratic	✓	✓
	1.006	0.006	0.6%	0.054	cubic	✓	
	0.929	-0.073	-7.1%	0.034	cubic	✓	✓
(C2) SE or E within 180 days	1.040	0.039	4.0%	0.033	quadratic	✓	
	0.977	-0.023	-2.3%	0.054	quadratic	✓	✓
	1.031	0.031	3.1%	0.045	cubic	✓	
	1.011	0.011	1.1%	0.074	cubic	✓	✓
(C3) SE or E within 360 days	1.026	0.025	2.6%	0.031	quadratic	✓	
	0.984	-0.017	-1.6%	0.064	quadratic	✓	✓
	0.999	-0.001	-0.1%	0.042	cubic	✓	
	1.064	0.062	6.4%	0.090	cubic	✓	✓
(C4) SE or E within 720 days	1.035	0.035	3.5%	0.029	quadratic	✓	
	0.982	-0.019	-1.8%	0.069	quadratic	✓	✓
	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 exiting into (A) self-employment, (B) employment, or into (C) the union of self-employment and employment within 90, 180, 360 or 720 days. The competing failure event is exiting into (A) re-employment, (B) self-employment, or (C) staying unemployed 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.

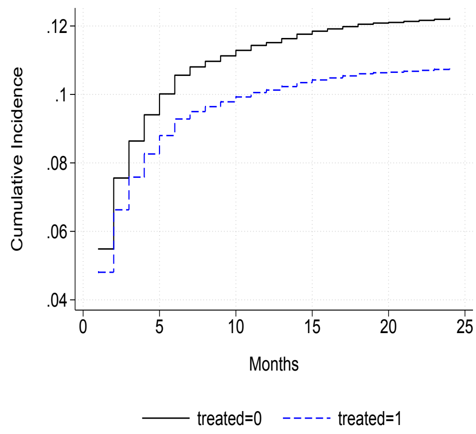
Source: Authors' calculations based on MCVL 2005-2018 data.

Figure E.2: Cumulative Incidence Functions – Quadratic

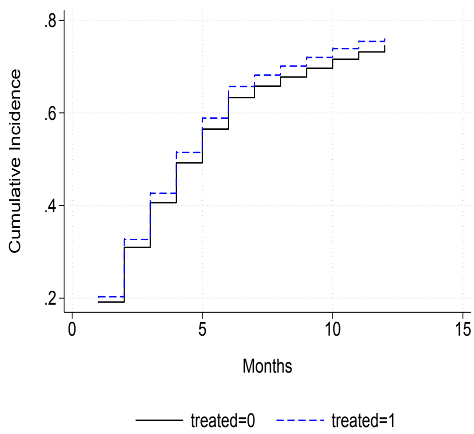
(a) SE within 360 days



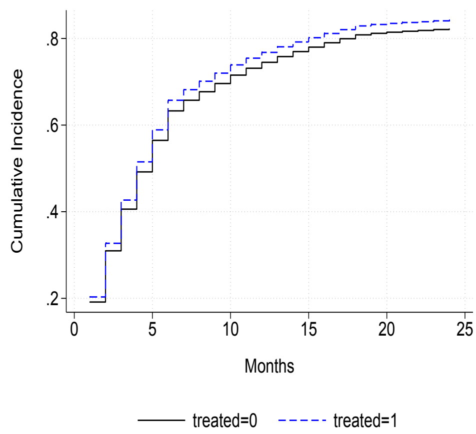
(b) SE within 720 days



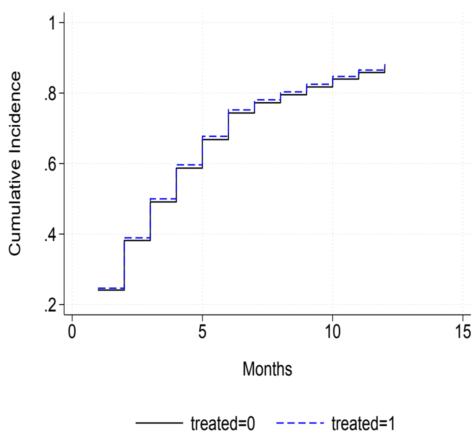
(c) E within 360 days



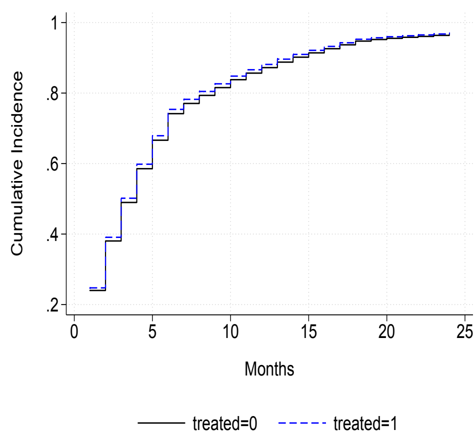
(d) E within 720 days



(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 based on MCVL 2005-2018 data.

F Appendix: Supplementary RDD Analysis

F.1 Subgroup Analysis of Extensive Margin Effects

Table F.1: Subgroup Analysis – Self-Employment within 360 Days (Including Covariates)

Sub Sample	RD Estimate	s.e.	p-value	Polynomial	Bandwidth	N Left	N Right
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
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
Women	-0.057	0.032	0.042	quadratic	161.119	1634	2039
	-0.063	0.034	0.036	cubic	243.132	2633	3057
Men	-0.029	0.026	0.224	quadratic	209.357	4792	4559
	-0.031	0.027	0.227	cubic	295.563	6615	6116
Permanent contract	-0.034	0.026	0.135	quadratic	192.851	4536	4578
	-0.040	0.027	0.097	cubic	285.764	6664	6696
Temporary contract	-0.027	0.030	0.266	quadratic	199.959	1937	2006
	-0.051	0.036	0.092	cubic	213.809	2057	2174
Children	-0.050	0.026	0.030	quadratic	190.843	3403	3469
	-0.049	0.027	0.040	cubic	294.857	5379	5140
No Children	-0.016	0.032	0.497	quadratic	190.212	2845	3035
	-0.029	0.035	0.323	cubic	237.485	3596	3801
Immigrant	-0.041	0.037	0.172	quadratic	154.735	805	910
	-0.079	0.041	0.028	cubic	183.960	1003	1085
No immigrant	-0.036	0.021	0.060	quadratic	182.951	4999	5210
	-0.042	0.022	0.037	cubic	273.473	7511	7573
Lower education	-0.023	0.029	0.384	quadratic	224.984	4263	4389
	-0.022	0.032	0.447	cubic	275.410	5372	5283
Medium education	-0.058	0.034	0.049	quadratic	186.739	1783	1735
	-0.068	0.037	0.036	cubic	277.145	2615	2508
Higher education	-0.078	0.032	0.007	quadratic	134.560	569	685
	-0.101	0.035	0.001	cubic	175.520	773	910
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
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 based on MCVL 2005-2018 data.

Table F.2: Subgroup Analysis – Employment within 180 Days (Including Covariates)

Sub Sample	**RD Estimate**	**s.e.**	**p-value**	**Polynomial**	**Bandwidth**	**N Left**	**N Right**
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
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
Women	0.019	0.059	0.581	quadratic	146.418	1498	1874
	0.025	0.066	0.607	cubic	202.532	2195	2596
Men	0.114	0.043	0.003	quadratic	163.706	3603	3494
	0.116	0.045	0.005	cubic	272.936	6112	5750
Permanent contract	0.048	0.044	0.185	quadratic	172.051	4116	4214
	0.053	0.047	0.179	cubic	272.751	6416	6462
Temporary contract	0.136	0.058	0.007	quadratic	155.867	1380	1595
	0.155	0.070	0.018	cubic	193.903	1797	1964
Children	0.073	0.052	0.100	quadratic	187.597	3364	3416
	0.084	0.058	0.117	cubic	257.651	4731	4598
No children	0.109	0.048	0.010	quadratic	131.111	1843	2081
	0.109	0.050	0.013	cubic	215.132	3272	3464
Immigrant	0.024	0.081	0.572	quadratic	182.046	997	1084
	0.106	0.113	0.268	cubic	190.694	1050	1112
No immigrant	0.077	0.043	0.043	quadratic	163.960	4407	4583
	0.076	0.043	0.053	cubic	291.908	8063	7977
Lower education	0.059	0.049	0.148	quadratic	172.370	3273	3476
	0.067	0.052	0.152	cubic	261.406	5118	5092
Medium education	0.102	0.057	0.038	quadratic	158.903	1486	1459
	0.121	0.063	0.036	cubic	199.206	1956	1828
Higher education	0.096	0.080	0.145	quadratic	162.266	714	807
	0.119	0.091	0.137	cubic	203.496	933	1056
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
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 based on MCVL 2005-2018 data.

F.2 Unemployment Duration Analysis

Table F.3: 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 F.5 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 based on MCVL 2005-2018 data.

Table F.4: 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
	0.170	-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
	0.990	-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
	0.814	-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
	0.292	-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
	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
	0.862	-0.573	-14.4	0.384	0.081	cubic		5697	5902
	0.889	-0.591	-14.8	0.357	0.061	cubic	✓	5104	5352

<u>Notes:</u> 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 F.3), 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 4.
<u>Source:</u> Authors' calculations based on MCVL 2005-2018 data.

Table F.5: UI and UE Duration Elasticities (720 Days)

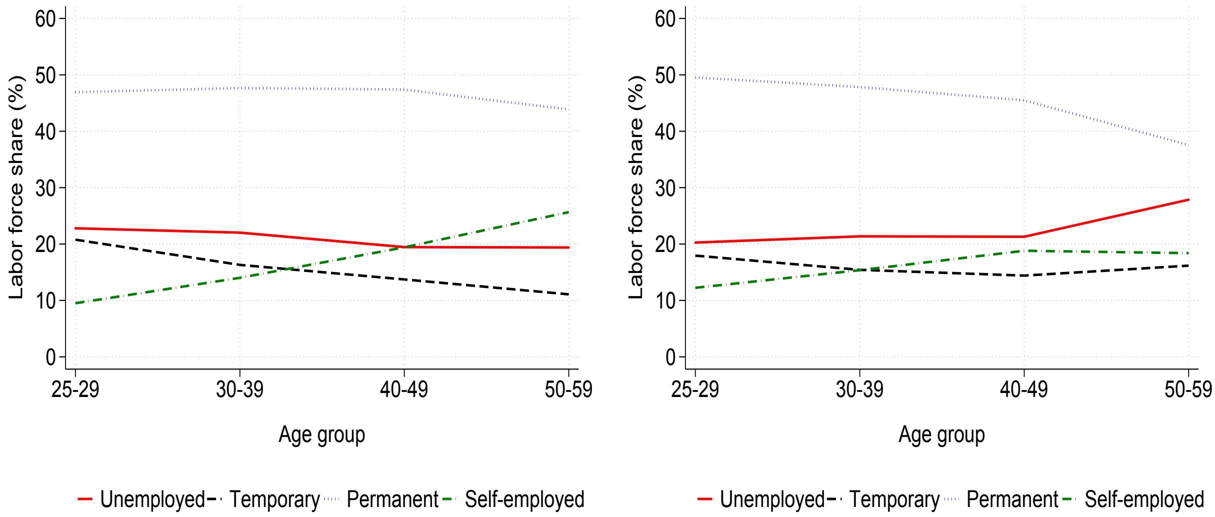
Outcome Variable	Duration Elast. (η)	RD Est.	% Change in Duration	s.e.	p-value	Polyn.	Covs.	N Left	N Right
(A) UI Duration									
(A1) Self-Employment	-1.199	1.006	20.0	1.045	0.296	linear		743	718
	-1.453	1.219	24.2	1.067	0.204	linear	✓	640	611
	-1.430	1.199	23.8	1.389	0.356	cubic		1088	1073
	-1.725	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
	0.991	-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
	0.663	-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
	-1.034	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
	0.716	-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
	0.469	-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 F.3), 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 4.
Source: Authors' calculations based on MCVL 2005-2018 data.

F.3 Reform Effect on the Self-Employment Quality – Descriptive Analysis

In this section we provide a complementary descriptive analysis of the effects on self-employment quality. Figure F.1 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 et al. (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.

Figure F.1: 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 based on MCVL 2005-2018 data.

Other descriptive evidence can be inferred from mean comparison tests in Table F.6. 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 F.6 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. 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 A.6.2 and E.4 for more details). A causal analysis is provided in the main text in Section 5.3.

Table F.6: 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 based on MCVL 2005-2018 data.