Public Funding and Non-Frontier Entrepreneurship*

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July 13, 2025

Abstract

While high-tech and VC-backed firms drive innovation, most employment originates from non-frontier incorporated entrepreneurs. Can public funding alleviate liquidity constraints for these entrepreneurs? We study a Portuguese program that allows unemployment insurance recipients to collect their benefits up front to start a business, provided they either forgo other work for three years or repay the amount received. Exploiting age-based discontinuities in benefit duration, we estimate an elasticity of incorporated entry with respect to funding of 0.65, more than three times that for unincorporated entry. The response is strongest among higher-wage workers, women, and during periods of financial stress. Consistent with a liquidity channel, program funding also increases firm size, capital intensity, productivity, and access to credit. Each €10,000 of funding generates 1.5 job-years by age 1 and 5.3 job-years by age 8. The evidence suggests that the repayment obligation for re-entering employment does not impose lasting financial hardship.

JEL classification: G38, H81, J24, J65, L26
Keywords: Entrepreneurship, Financial constraints, Unemployment insurance, Public funding

*This paper replaces a previous one titled "Which Entrepreneurs Are Financial Constrained". We thank Manuel Adelino, Michele Belot, Thomas Crossley, Nick Flamang, Cesare Fracassi, Juanita González-Uribe, Ralph De Haas, Johan Hombert, Andrea Ichino, Ron Kaniel, Gustavo Manso, Antoinette Schoar, David Thesmar, and Andrei Shleifer; and seminar participants at the European University Institute, INSEAD, Shangai Advanced Institute of Finance (SAFE), PBC School of Finance-Tsinghua University, 2020 SOLE/EALE/AASLE World Conference, III Workshop Online Investigación en Emprendimiento, 2021 ASSA Annual Meeting, 2021 RES Conference, and 2025 Alicante Workshop on Macroeconomics and Productivity for helpful comments. We gratefully acknowledge financial support from Fundação Francisco Manuel dos Santos and from Fundação para a Ciência e Tecnologia (Grant PTDC/EGE-OGE/4714/2021).

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1 Introduction

Markets under-provide innovation and innovation is central to economic growth (Romer, 1990). This rationale underpins the large sums devoted by governments around the world to promoting high-tech and venture capital-backed startups (Bai et al., 2021), as well as the growing research interest in these programs (e.g. Howell, 2017) and on innovation-driven entrepreneurship more generally (Botelho et al., 2023). A second rationale for intervention, however, arises from liquidity constraints that distort selection into entrepreneurship and the allocation of resources across firms, reducing aggregate productivity (Buera et al., 2011; Caselli and Gennaioli, 2013).

From this misallocation perspective, the focus on innovation is too narrow. Even in a frontier economy like the United States, high-tech and VC-backed firms account for just 6-7% of employment, and all patenting firms for 24%.[1] An overly broad focus is equally inappropriate: unincorporated sole proprietorships, a proxy for low-potential entrepreneurship (Levine and Rubinstein, 2017), make up over 70% of businesses but only 4% of jobs. Between these two extremes lies a "middle class" of non-frontier incorporated entrepreneurs who generate most employment, and thus potential misallocation. Yet little is known about whether public funding can ease liquidity constraints for these entrepreneurs. This paper examines that question.

The challenge for programs motivated by liquidity constraints lies in identifying the entrepreneurs who are inefficiently deterred from entry. Governments face the difficult task of distinguishing between market failures and poor investments without crowding out private capital or being captured by private interests. Numerous examples of squandered funds highlight these difficulties (Shleifer and Vishny, 1998; Lerner, 2009), and several authors have expressed skepticism about public funding that does not exclusively target innovative entrepreneurship (Shane, 2009; Acs et al., 2016).

We study a public program that combines financial support to entrepreneurs with a strict

[1] Appendix Table A.1 provides sources for all numbers reported in this paragraph and shows that gross job creation shares, where available, are similar to employment shares. The table also reports statistics for Europe.

but inexpensive screening mechanism. The program we study, called *Montante Único* (MU), allows any individual on unemployment insurance (UI) in Portugal to collect their benefits up front to start a business. In exchange, program participants must refrain from engaging in any other professional activity for three years or repay the full amount received. Outside the program, UI recipients can instead suspend their benefits and resume collecting them if their business fails. Entrepreneurs thus face a choice between up-front liquidity and insurance. Selecting into the program should be particularly attractive for those who genuinely need the liquidity to invest and expect returns that justify the risk of repayment. Comparable programs have been introduced in other European countries[2] and the U.S. briefly tested a similar model in Washington State during the early 1990s (Wilson and Adams, 1994).

Our analysis draws on high-quality administrative data covering 1.3 million UI recipients from 2005 to 2012, linked to their post-unemployment entrepreneurial trajectories through 2021. We find that 4.6% of workers transition from UI into entrepreneurship, but only 1.4% do so through the MU program. Program participants had pre-unemployment wages about 60% higher than other workers, suggesting they are relatively high-skilled and have strong outside options. Moreover, half of them incorporate their businesses, compared with just 19% of all new ventures in Portugal. Program firms, in turn, account for 5.3% of all incorporated businesses launched during the sample period and closely resemble firms created outside UI in terms of sectors and size. They also exhibit similar levels of quantity-based productivity (TFPQ), lower capital intensity, and higher revenue-based productivity (TFPR), with both TFPQ and TFPR measured following Hsieh and Klenow (2009). Taken together, these facts suggest that the program effectively channels support to liquidity-constrained non-frontier entrepreneurs.

To estimate the causal effect of program funding on entry, we exploit the fact that the duration of UI benefits, and hence the amount that potential entrepreneurs can receive up front, increases discontinuously at ages 30, 40 and 45. These increases average just over €3,000 but can exceed €12,000 depending on wages and experience. For context, the average initial equity

[2] France's *Aide à la Reprise ou à la Création d'Entreprise*, Italy's *NASpI Anticipata*, and Spain's *Pago Único*.

for incorporated businesses in Portugal in this period was approximately €20,000, while the average financial wealth of unemployed workers was below €7,000 (Banco de Portugal and INE, 2010). The shocks we exploit can therefore plausibly affect a potential entrepreneur's ability to start a business. Naturally, these discontinuities may also affect entry by UI recipients who suspend their benefits, or by those who simply exhaust them before entry. We investigate these effects as well.

We find that the fraction of UI beneficiaries who start an incorporated business through the program increases discontinuously at the age cutoffs. Instrumenting the amount that entrepreneurs are entitled to receive with these cutoffs in a regression discontinuity (RD) design, we estimate an elasticity of incorporated entry with respect to available funding of 0.65. This is over three times larger than our corresponding estimate for unincorporated entry. By contrast, for entrepreneurs who suspend their UI benefits, discontinuous increases in benefit duration affect unincorporated entry only, and among those who exhaust their benefits, we find no effect on entry, incorporated or not. The result on benefit suspension is consistent with Hombert et al. (2020), who find that a reform allowing UI recipients in France to suspend their benefits to start a business boosted entry in sectors with a higher prevalence of sole proprietorships relative to sectors where incorporated businesses are more prevalent.

Our identifying assumption is that potential confounders do not vary discontinuously at the age cutoffs where benefit duration increases. One concern is that prospective entrepreneurs close to the cutoffs might strategically time their dismissals to take advantage of longer UI durations, even though unemployment must be involuntary to qualify for UI. While we find evidence of such manipulation within two months of the cutoffs, it does not appear to be correlated with the propensity for entrepreneurship through the program. The estimates we obtain are very similar to our baseline when we either exclude these observations close to the cutoffs, as in Barreca et al. (2011), or impose narrow bandwidths around the cutoffs. Our results are also robust to alternative specifications, such as adding covariates, allowing different bandwidths on either side of the cutoffs, or employing higher-order local polynomials.

The response of incorporated entry to funding increases with pre-unemployment wages. We estimate no effect for the bottom tercile of the wage distribution, and elasticities of 0.71 and 0.86 for the middle and top terciles. Our estimates are also 2.5 times stronger for women than for men, in line with evidence that female entrepreneurs face tighter borrowing constraints (Morazzoni and Sy, 2022), and two-thirds larger in periods of financial stress than in normal times, namely during the global financial crisis of 2008 and the sovereign debt crisis that Portugal faced in 2011. The elasticity of unincorporated entry, on the other hand, remains low across the wage distribution, for both genders, and throughout our sample period. Overall, our findings point to liquidity over insurance as the key constraint on incorporated entry.

The effects of program funding extend well beyond the initial transition out of unemployment. Seven years after dismissal, we estimate an elasticity of 0.35 for incorporated entrepreneurship with respect to available funding. About three-quarters of this persistence comes from the original incorporated businesses launched through the MU program. The rest reflects later transitions from unincorporated to incorporated entrepreneurship, suggesting that the program may also put some entrepreneurs on a gradual path toward incorporation. In contrast, we find essentially no lasting effect on unincorporated entrepreneurship.

We next turn to the intensive margin effects of program funding on incorporated business outcomes. Isolating the intensive margin requires overcoming a selection problem arising from productivity differences between marginal entrepreneurs induced to enter by the higher liquidity and inframarginal ones who would have entered anyway. Adapting the selection correction developed by Chodorow-Reich et al. (2024) to our RD design, we restrict the sample to inframarginal entrepreneurs and find substantial positive effects of funding on firm size, TFPQ, capital intensity, and the average product of labor, along with weaker but positive effects on TFPR. We also estimate that every euro of available funding generates one euro of initial equity and nearly four euros of debt. These findings are consistent with a liquidity channel, and suggest the program has the potential to both improve firm-level efficiency and reduce misallocation.

We conclude with a cost-benefit analysis of the program. Using our RD design, we estimate cumulative job creation and output gains per euro of program funding. By age 1, the program generates 1.5 job-years per €10,000, implying a cost-per-job-year of €6,630, and €1.3 of value added per euro of funding. These effects grow substantially over time: by age 8, job creation rises to 5.3 job-years per €10,000, lowering the cost-per-job-year to €1,882, and value added exceeds €5 per euro of funding. Our cost-per-job estimates are broadly comparable to those from small business loan guarantee programs (e.g., Brown and Earle, 2017; Bonfim et al., 2023).

We also examine how the repayment obligation affects those whose businesses fail. Among early failures, repayment is common but we find no evidence of adverse effects on labor market reintegration or income. Most early exits return to work within three years and entrepreneurs who exit at ages 0–2, during the repayment window, experience improved post-exit income trajectories relative to age-4 exits, even after accounting for repayment.[3] Those who exit at age 3, immediately after the repayment window expires, experience temporary income declines relative to age-4 exits. This may reflect delayed exit to avoid repayment, but the gap closes within two years. Taken together, these findings suggest that early exits are often opportunistic rather than symptomatic of financial distress, and that the program's screening mechanism helps limit adverse selection without imposing significant costs on those who exit.

Our paper makes three main contributions. First, we add to the literature on public entrepreneurial finance in developed economies, which comprises two distinct strands.[4] One focuses on high-tech startups and venture capital, namely on the effects of public R&D grants (Howell, 2017), public–private co-investment schemes (Brander et al., 2015; Bai et al., 2021), tax benefits (Cumming and MacIntosh, 2006; Edwards and Todtenhaupt, 2020; Denes et al., 2023), and publicly-funded accelerators and new venture competitions (Gonzalez-Uribe and Leatherbee, 2018; Howell, 2020). The other examines active labor market policy programs supporting transitions from unemployment to self-employment. These studies find that allowing UI re-

[3]Repayment can be made in monthly installments over three years, and we assume this repayment schedule in our calculations.

[4]Other institutional or policy levers that affect constraints on private entrepreneurial finance have been more extensively studied, such as financial market depth (Guiso et al., 2004) and banking regulation (Black and Strahan, 2002; Kerr and Nanda, 2009, 2010).

cipients to receive their benefits monthly either while launching a business or in case of failure boosts entry by sole proprietorships (Hombert et al., 2020) or by businesses that tend to lag others in survival and growth (Caliendo et al., 2015, 2020; Gaillard and Kankanamge, 2024). The MU program also operates through UI but differs in two key aspects from those previously studied: it offers up-front funding, thereby facilitating investment, and it employs a simple screening mechanism, the temporary forgoing of other activities. We show that the combination of these two features can successfully target the kind of non-frontier incorporated entrepreneurship that accounts for the bulk of employment. Our focus on this group of entrepreneurs builds on Levine and Rubinstein (2017), who document the stark contrast in personal traits and outcomes between incorporated and unincorporated entrepreneurs.

Second, our results on business outcomes contribute to a growing literature on the real effects of public funding on firm performance, much of which has focused on loan guarantee programs.[5] Most studies find that these programs increase firm employment, either persistently (Brown and Earle, 2017) or temporarily (Bonfim et al., 2023), though their effects may have been muted during COVID-19 (Granja et al., 2022). Some studies also document adverse effects, including higher bankruptcy rates (Lelarge et al., 2010) and greater misallocation (Barrot et al., 2024). We find persistent gains in employment, output, and productivity, suggesting that targeting new entrepreneurial entry, and not just supporting existing businesses, can improve business dynamism and reduce misallocation.

Third, we contribute to the long-standing debate on the mechanisms underlying the relationship between wealth and business creation (Kihlstrom and Laffont, 1979; Evans and Jovanovic, 1989; Hurst and Lusardi, 2004). Recent studies suggest that liquidity constraints (Adelino et al., 2015; Schmalz et al., 2017; Chodorow-Reich et al., 2024) and risk aversion (Hombert et al., 2020; Barrios et al., 2022) both play a role, but do not shed light on their relative importance. In our setting entrepreneurs must *choose* between collecting their benefits up front, forgoing other professional activities for three years, or claiming them in case of failure.

[5] See De Haas and Gonzalez-Uribe (2024) for a review.

This allows us to directly compare the relative importance of the two channels. Our findings suggest that liquidity is the predominant constraint for incorporated entrepreneurship, while risk aversion plays a more important role for unincorporated ventures. These findings complement Levine and Rubinstein (2018) and Bellon et al. (2021), who also find that liquidity constraints primarily affect incorporated entrepreneurship. In line with (Chodorow-Reich et al., 2024), our results on business outcomes rule out non-pecuniary benefits as an alternative to a liquidity channel. Furthermore, we show not just that liquidity constraints matter, but that they can be relieved through a public program employing a low-cost screening mechanism.

Lastly, our paper contributes to the literature on the design of UI, which traditionally focuses on the trade-off between income smoothing and incentives to return to work (Baily, 1978; Chetty, 2006). The MU program sidesteps this trade-off by enabling prospective entrepreneurs to collect their full benefits up front. This is similar to the lump-sum severance payments studied by Gerard and Naritomi (2021), but less prone to consumption distortions caused by present bias, since the funds must be invested in the business. The program also complements traditional UI by relieving liquidity constraints that deter efficient self-employment. Moreover, rather than searching for jobs, program participants create new positions, expanding labor demand. These features may be particularly valuable during recessions, when credit constraints tighten and job creation slows. Consistent with this, we find that the responsiveness of incorporated entrepreneurship to funding is greater in periods of financial stress. Overall, our findings suggest that the program can enhance the role of UI as an automatic stabilizer both by sustaining demand and by enabling productive reallocation.

The key trade-off in the MU program lies in the repayment obligation: it offers a low-cost screening mechanism but may impose a substantial financial burden on entrepreneurs who fail early, when income is limited. We find that most of these individuals return to work quickly, and their subsequent income trajectories do not point to significant hardship relative to later failures, suggesting that the repayment requirement does not create severe disincentives or lasting distress.

2 Institutional Background

The Portuguese unemployment benefits system comprises unemployment insurance and unemployment assistance (UA). The latter applies to individuals who either exhaust their UI benefits (Subsequent UA) or fail to meet UI eligibility requirements (Initial UA). Both UI and Initial UA recipients may use their benefits to start a businesses through the MU program. In this section, we describe the rules governing these unemployment benefits and their use for business creation during our sample period, from January 2005 to March 2012. Reforms introduced in April 2012 and in July 2014 changed these rules in ways that invalidate our identification strategy, so we focus on the pre-reform period.[6] This also allows us to track post-entry trajectories for several years in a balanced panel.

2.1 Unemployment Insurance

To be eligible for UI or Initial UA, individuals must accumulate a certain number of monthly Social Security contributions before their involuntary dismissal. For UI, the minimum contribution period during the two years preceding dismissal was 15 months in our sample period.[7] Individuals who do not meet this requirement but have worked at least six months in the year before unemployment are entitled to Initial UA. In addition, Initial UA is means-tested, requiring that the household's per capita earnings not exceed the minimum wage.

Once eligible, unemployed individuals receive a monthly, tax-exempt payment. Initial UA beneficiaries receive a payment equal to their net wages for the first six of the eight months preceding their unemployment spell, up to 80% of the Social Support Index if living alone or 100% if living with others. The Social Support Index (SSI) is a reference value used to calculate

[6] The 2012 reform eliminated the discontinuity in benefit duration at age 45 and replaced it with a new discontinuity at age 50. This new discontinuity, however, only applies to the 20% of recipients aged 50 or older who had less than 24 months of contributions since their last UI spell. The reform also substantially reduced benefit duration across the board and the jumps in duration at the age 30 and 40 cutoffs. The 2014 reform, in turn, introduced a set of entrepreneurial subsidies and interest-free loans for UI recipients under the age of 30—the *Investe Jovem* program—thus invalidating the age 30 cutoff.

[7] Between January and June 2010, the minimum number of contributions temporarily dropped to 12 months.

social benefits and contributions, which equaled just under 500 euros in our sample period (we convert all nominal values throughout the paper to 2020 euros). The UI benefit is tied to wages earned during the first 12 of the 14 months before dismissal. Before July 2010, the replacement rate was 65% of gross wages; since then, it has been 75% of net wages. Throughout the sample period, individuals were guaranteed at least the SSI or 100% of their net wages, whichever was lower. At the upper end, the monthly amount was capped at three times the SSI. Since we treat them identically in our analysis, we use UI to refer to both Initial UA and UI from here onwards. UI payments are available for a predetermined duration ranging from nine months to over three years, depending on the individual's age at dismissal and total Social Security contributions, as summarized in Table 1.

2.2 Transition to Entrepreneurship

The MU program, introduced in 1985, enables UI recipients to receive their remaining benefits up front to start a business.[8] They may request this payment in the first month of their claim or later, and in full or in part. Any unused benefits are suspended, and may be resumed as monthly payments if the business fails.[9] Under the rules described above, the maximum up-front payment could reach €55,000.

The new venture may be either unincorporated or incorporated, launched individually or with partners. Local Social Security offices handle project approval, which should occur within 90 working days of submission, as well as subsequent monitoring. The funds must be fully invested into the business within one year of approval, and can finance the acquisition of fixed assets, with the exception of real estate, and working capital, which cannot exceed 30% of the total amount received. Participants must forgo any other professional activity that is normally remunerated, regardless of whether they draw income, for three years, or else repay the entire

[8] The funds can also be used to acquire a stake in an existing business under certain conditions, but we exclude these cases from our definition of entrepreneurhip below.

[9] Since the April 2012 reform at the end of our sample period, unincorporated entrepreneurs can also continue receiving unused benefits as monthly payments, instead of suspending them.

amount received. Thus, if the business fails, the individual must reimburse the funds to take a different job within that three-year window. Enforcement relies on mandatory third-party reporting of professional activities to Social Security. These reports are made by employers in the case of employment, by the tax authority in the case of unincorporated self-employment, and by the business registry in the case of incorporated self-employment. Social Security enforces repayment in court, if necessary (e.g., STA, 2018).

Instead of enrolling in the MU program, entrepreneurs can suspend their benefits and reclaim them if the venture fails, or exhaust their benefits before entry. Unincorporated entrepreneurs who suspend their benefits can continue to receive monthly payments under a partial UI regime. They receive either the full amount of their benefits or the difference between 135% of that amount and the income generated by their business, whichever is lower. If the business fails, they can claim any remaining unused benefits, including the amounts deducted on account of their business income.

3 Data

3.1 Data Sources

Our study uses administrative data collected by Social Security in Portugal covering the universe of UI recipients between January 2005 and March 2012. For each individual, we observe demographics, namely age and gender; employment histories—including dates of employment and unemployment, wages, and firm and industry identifiers; and detailed information on UI payments, including up-front amounts received through the MU program.

We link the Social Security data to administrative data on firms from *Informação Empresarial Simplificada* (IES), which provides detailed balance sheet and income statement information for the universe of non-financial incorporated businesses. This matched data set allows us to track outcomes up to 2021 for incorporated firms created by UI recipients who became unemployed between January 2007 and April 2012. We can observe these firms up to age 8 with minimal right-censoring, as 98% of them had been created by the end of 2013. We thus focus our analysis on firm outcomes up to age 8.

We employ two additional data sets in our descriptive analysis of the MU program. First, to compare program participants with the broader population of employed workers, we use administrative data on wages and demographics from *Quadros de Pessoal*, a matched employer–employee dataset covering all private-sector employees in Portugal. Second, to compare MU-funded firms with the population of firms, we draw on data from *Sistema de Análise de Balanços Ibéricos* (SABI), a database distributed by Bureau van Dijk, which includes information on ownership and management sourced from business registers and directly from firms. The ownership and management data allow us to identify entrepreneurial firms created outside UI, as we explain below.

3.2 Definition of Entrepreneur

Social Security classifies each work spell as employment, unincorporated self-employment, or membership in a firm's governing body. The third category applies to managers with decision-making authority in an incorporated company. We define a UI recipient as an *entrepreneur* if, between the start of their UI benefits and 30 days after these benefits end, they begin a spell classified as either unincorporated self-employment (*unincorporated entrepreneur*) or membership in the governing body of a newly established firm (*incorporated entrepreneur*), and that spell lasts at least six months. If an individual initiates more than one such spell, we take the first. We define *MU entrepreneurs* – whether incorporated and unincorporated – as individuals who meet the corresponding definition of entrepreneurship and receive their UI benefits up front through the MU program.

3.3 Summary Statistics

Table 2 reports summary statistics for UI recipients who select into entrepreneurship through each of the three available options—the MU program, benefit suspension, and benefit exhaustion—as well for overall UI recipients and for employed individuals. For UI recipients, we report demographics at the time of dismissal and from the last job before UI. For employed individuals, we present statistics for all worker-year observations in the matched employer–employee data in our sample period.

We find that 1.4% of UI recipients select into the MU program, and that an additional 3.2% become entrepreneurs outside the program, 1.8% by suspending benefits and 1.4% after exhausting them. Program participants are 38.6 years old on average, slightly older than overall UI recipients and similar to employed workers. They are also more likely to be male, in line with previous research indicating that entrepreneurship rates are higher among men (Fairlie and Robb, 2009). Their pre-unemployment wages exceed those of UI recipients and the wages of employed workers by about 60%, which suggests that they are relatively high-skilled. By contrast, entrepreneurs who suspend or exhaust their benefits are younger and less likely to be male. Their wages are also higher than those of UI recipients and employed workers, but lower than those of MU program participants.

On average, program participants can access just under €19,000 through the program, and over 38,000 euros at the 90% percentile. In addition, 49% of program entrepreneurs incorporate their businesses. This compares with an overall incorporation rate of 19% for businesses created in Portugal in our sample period (INE, 2025), and with incorporation rates of 7% and 32% for entrepreneurs who suspend and exhaust their benefits, respectively.

Table 3 presents business outcomes for incorporated entrepreneurs at age 8. We compare firms launched through the MU program with the population of same-aged entrepreneurial firms created outside UI during the same period, which we label non-UI firms. Using the SABI data, we define as entrepreneurial the firms that were majority-owned at entry by individuals who managed the firm and were not already owner-managers of another firm at the time of the new firm's creation. This definition excludes corporate subsidiaries and firms launched as part of the portfolios of existing entrepreneurs, which are unlikely to represent independent new ventures.

Firms created through the program account for 5.3% of new ventures in our sample period, and they are broadly similar to non-UI firms. Size-wise, both sets of firms sell just under €300,000 and employ 3.6 workers on average. At the 99^{th} percentile, sales equal €3.2 million for both groups, and program firms employ 29 workers, versus 30 for non-UI firms. These numbers suggest that the program generates high-growth entrants at rates comparable to those observed in the broader economy. Value added, wage bills, EBITDA (earnings before interest, taxes, depreciation, and amortization), and survival are also close. Two noteworthy differences are that program firms employ less capital, measured either by fixed or total assets, and less initial funding, both equity and debt.[10]

Turning to productivity, the average product of labor (value added/employment) is marginally lower for program firms, while the average product of capital (value added/fixed assets) is marginally higher. Following Hsieh and Klenow (2009), we also compare measures of revenue and quantity-based total factor productivity (TFPR and TFPQ).[11] We find that TFPR is 14% higher for program firms, while TFPQ is nearly identical in the two sets of firms. In the Hsieh and Klenow (2009) misallocation framework, TFPR would be equalized across firms in the absence of frictions preventing an efficient allocation of inputs, such as financial constraints. A higher TFPR implies that program firms remain inefficiently small relative to non-UI firms even after receiving the MU funding. In other words, reallocating additional inputs towards program firms would increase aggregate productivity.

Table 4 shows that businesses started through the program also resemble the overall population of new businesses in terms of industry composition. Program entrepreneurs are somewhat more likely to be in wholesale and retail trade, in professional, scientific, and technical activities, and in administrative and support services. They are less likely to be in construction

[10]We measure initial equity and debt at age 1, since entrepreneurs have up to one year to invest their MU funds into their businesses.

[11]TFPR is the geometric mean of the average products of labor and capital, weighted by the respective output shares. TFPQ is equal to TFPR divided by the firm's output price, which is inferred from the firm's valued added and the elasticity of substitution across firms. Following standard practice, we assume output shares of 2/3 for labor and 1/3 for capital, along with an elasticity of substitution of 4. Both measures are normalized by their sample mean across all firms.

and in healthcare, and about as likely to be in other sectors. Tables IA.1 and IA.2 in the Online Appendix report outcomes and sectors for firms created by UI recipients who suspend and exhaust their benefits.

4 Effect of UI Funding on Entry

4.1 Methodology

To estimate the effect of UI funding on entry, we exploit age-based discontinuities in the potential duration of unemployment benefits to generate exogenous variation in the amount available to prospective entrepreneurs, using a fuzzy regression discontuinity (RD) design. The discontinuous increases occur at ages 30, 40, and 45 (see Table 1). To combine variation around the three cutoffs into a single estimate, we pool our data (e.g., Pop-Eleches and Urquiola, 2013). We form three samples, one per cutoff, each including all UI recipients within five years of the cutoff. This five-year window on each side restricts each sample to one discontinuity. We then stack the three samples. Figure 1a illustrates our identification strategy by plotting the average amount available to UI recipients by distance to the cutoffs in the pooled sample, grouping observations by quarterly bins. As the plot shows, the amount evolves smoothly away from the cutoffs and increases discontinuously at the cutoffs by just over €3,000 on average.

Our fuzzy RD estimand instruments the log amount of funding available to UI recipients with the cutoffs:

$$\tau^{l,k} = \frac{\lim_{a \downarrow 0} \mathbb{E}[E^{l,k}_{i,c} | A_{i,c} = a] - \lim_{a \uparrow 0} \mathbb{E}[E^{l,k}_{i,c} | A_{i,c} = a]}{\lim_{a \downarrow 0} \mathbb{E}[\log \pi_{i,c} | A_{i,c} = a] - \lim_{a \uparrow 0} \mathbb{E}[\log \pi_{i,c} | A_{i,c} = a]}, \tag{1}$$

where $A_{i,c}$ is the age distance of recipient i to cutoff c at the time of dismissal, $E^{l,k}_{i,c}$ is an indicator for entry under legal form $l \in \{Inc., Uninc.\}$ (i.e. incorporated or unincorporated) and through option $k \in \{MU, Suspend, Exhaust\}$, while $\pi_{i,c}$ is the total amount of UI benefits available. The first-stage coefficient, given by the denominator of (1), measures the effect of the cutoffs on the log of available funding, and $\tau^{l,k}$ represents the semi-elasticity of l entry with respect to funding through option k. To recover the elasticity, we divide $\tau^{l,k}$ by the entry rate just below the cutoffs:

$$\varepsilon^{l,k} = \frac{\tau^{l,k}}{\lim_{a \uparrow 0} \mathbb{E}[E^l_{i,c} | A_{i,c} = a]} \tag{2}$$

where $E^l_{i,c}$ denotes l entry through any of the three options. Our parameter of interest is $\varepsilon^{l,k}$, motivated by the fact that the log rate of entry through the MU program increases linearly with the log of available funding. Figure 1b plots this relationship for incorporated entry, our primary outcome of interest.

We estimate equation (1) and the denominator of equation (2) via local linear regression with a mean-squared-error (MSE) optimal bandwidth and a triangular kernel, following Calonico et al. (2014). Depending on the bandwidth, UI recipients aged 40-45 can be represented more than once in our stacked regression sample, so we cluster standard errors at the recipient level.

4.2 Validity of the RD design

The causal interpretation of our estimand relies on two assumptions. First, monotonicity requires that at the age cutoffs the amount of funding is non-decreasing for all UI recipients, which holds as long as Social Security complies with the legal rules governing UI benefits outlined in Table 1. Second, the exclusion restriction requires that no other determinants of entrepreneurship vary discontinuously with age at these cutoffs. This includes both personal characteristics, such as entrepreneurial talent, and other public policies that might affect entrepreneurship and employ the same cutoffs. To the best of our knowledge, no such policies existed in our sample period.

One concern is that some individuals might be able to time their dismissals to take advantage of the increase in UI generosity at the age cutoffs, even though dismissals must be involuntary to qualify for UI. If this does occur, we would expect to see discontinuities in the distribution of the running variable at the cutoffs. Figure 2a plots the number of observations in the pooled sample by distance to the cutoffs in monthly bins. The plot suggests that the distribution evolves smoothly except perhaps within one or two months of the cutoffs, where there might be some manipulation. A McCrary (2008) density test, using the implementation proposed by Cattaneo et al. (2020), indeed rejects continuity at the cutoffs ($p = 0.002$), but not when excluding observations within two months of the cutoffs ($p = 0.310$). Plots for the average pre-unemployment wage, the fraction of male recipients, and the other determinants of UI duration aside from age—months of contributions since the last UI spell and years of contributions in the previous 20 years—also suggest that if there is any manipulation it is highly localized (Figures 2b to 2e). Table IA.3 in the Online Appendix presents the corresponding continuity tests. We reject that pre-unemployment wages are continuous at the cutoffs ($p = 0.042$), but not when excluding observations within one month of the cutoffs ($p = 0.230$); we cannot reject continuity in any of the other covariates.

Even if manipulation is highly localized, if it is correlated with the propensity for entrepreneurship then the exclusion restriction is violated. We evaluate this possibility by comparing our baseline estimates with alternative specifications where we either drop the potentially problematic observations, using a donut regression discontinuity design (Barreca et al., 2011), or use progressively narrower bandwidths around the cutoffs, increasing the weight of the potentially problematic observations in the estimation. As we discuss below, we obtain very similar estimates in all cases, which suggests that whatever manipulation occurs is unrelated with selection into entrepreneurship.

4.3 Results

4.3.1 Baseline

Panel (a) in Figure 3 presents our main result on *incorporated* entry graphically. Grouping observations in the pooled sample into quarterly bins by distance to the age cutoffs, the plot shows that the fraction of UI recipients who start an incorporated business through the MU program jumps sharply at the cutoffs. By contrast, panels (b) and (c) reveal no such jump for those who suspend or exhaust their unemployment benefits before entry.

The corresponding RD estimates are reported in columns 1-3 of Table 5. For each regression, we report MSE-optimal point estimates and standard errors for the semi-elasticity $\tau^{l,k}$, with Significance levels based on robust bias-corrected p-values. We also report the implied elasticity $\varepsilon^{l,k}$, obtained from equation (2), along with coefficients and z-statistics for the first stage, from the denominator of equation (1). Lastly, we report the MSE-optimal bandwidth and the effective number of observations within that bandwidth.

The first stage is extremely strong in the three columns, as in all our regressions. Funding increases by 0.276 to 0.280 log points at the cutoffs, with the minor differences across columns driven by changes in the optimal bandwidth, which ranges from 1.4 to 1.8. In column 1, we obtain a highly significant ($p < 0.01$) semi-elasticity of incorporated entry to funding through the MU program of 0.010, and an elasticity $\varepsilon^{Inc.,MU}$ of 0.649. This implies that the average jump of just over €3,000 at the cutoffs increases entry by 0.27 percentage points.[12] For entry through benefit suspension and exhaustion, we estimate much smaller and insignificant semi-elasticities in columns 2 and 3, which imply $\varepsilon^{Inc.,Suspend} = -0.054$ and $\varepsilon^{Inc.,Exhaust} = 0.050$.

Figure 4 presents analogous results for *unincorporated* entry. In this case, panels (a) and (b) show that entry through the MU program and through benefit suspension both increase discontinuously at the cutoffs, while entry through benefit exhaustion in panel (c) exhibits no change. Columns 4-6 in Table 5 present the respective RD estimates. The semi-elasticity for en-

[12] This corresponds to the sharp RD estimate given by the numerator of equation (1), which we infer by multiplying our estimated $\tau^{l,k}$ and first-stage coefficients.

try through the program, reported in column 4, equals 0.007 ($p < 0.1$), which yields an elasticity $\varepsilon^{Uninc.,MU}$ of 0.189, less than a third of our estimate for incorporated entry in column 1. This corresponds to an increase of 0.19 percentage points in entry for the average jump in funding at the cutoffs. We obtain very similar results for entry through benefit suspension, which exhibits an elasticity to funding of 0.205, reported in column 5. For entry through benefit exhaustion, we find no effect in column 6, with an estimated elasticity of -0.005.

4.3.2 Robustness

We perform several robustness checks for $\varepsilon^{Inc,MU}$, our main parameter of interest. First, we examine whether manipulation in the timing of dismissal close to the age cutoffs might bias our estimates. Table 6 reports estimates from five alternative specifications. In column 1 to 3, we drop observations within one, two and three months of the cutoffs, respectively. Our estimates of $\varepsilon^{Inc.,MU}$ from these donut RDs range from 0.623 to 0.758, close to our baseline estimate. In columns 4 and 5, we perform the opposite exercise: we shrink the RD bandwidth, increasing the influence of observations near the cutoffs on our estimates. With a one-year bandwidth, we obtain $\varepsilon^{Inc.,MU} = 0.689$, again nearly identical to our baseline estimate. Even with a six-month bandwidth, which drops over two thirds of the observations within the MSE-optimal baseline bandwidth, we still obtain a similar elasticity of 0.576, although our estimate becomes insignificant at conventional levels. These results indicate that our estimates are not driven by manipulation near the cutoffs.

Table IA.4 in the Online Appendix presents additional robustness checks. In column 1, we add covariates to our estimation, namely pre-unemployment wages, gender, months of contributions since the last UI spell, and years of contributions in the previous 20 years. Column 2 employs separate MSE-optimal bandwidths below and above the cutoffs. Columns 3 and 4 use local quadratic and cubic polynomials, instead of linear. And column 5 implements the honest regression discontinuity approach proposed by Armstrong and Kolesár (2018), which inflates confidence intervals to account for the bias in local linear estimation as an alternative to the bias-correction procedure of Calonico et al. (2014). Across all specifications, our estimated elasticities range from 0.622 to 0.717, closely aligned with our baseline estimate.

4.3.3 Heterogeneity

We examine whether the response of entry to funding through the MU program varies with UI recipient characteristics. We start by splitting the sample into pre-unemployment wage terciles, as a proxy for entrepreneurial skill and outside options. Figures 5a and 5b plot the fraction of MU incorporated entrepreneurs among recipients in the bottom and top terciles of wages. An increase at the age cutoffs is clearly visible for the top tercile, but not for the bottom one. Columns 1-3 of Table 7 present RD estimates for each tercile. There is essentially no effect in the bottom tercile, where we obtain $\varepsilon^{Inc.,MU} = 0.052$, while for the middle and upper terciles we estimate elasticities of 0.709 and 0.861, respectively. Columns 1-3 of Table IA.5 in the Online Appendix report analogous estimates for unincorporated entry. These exhibit the opposite pattern, with elasticities declining from 0.238 in the bottom tercile to 0.138 in the top tercile.

Figures 5c and 5d display the fraction of MU incorporated entrepreneurs among men and women. There is a clear discontinuity in both cases, but a visibly larger jump for women relative to their base rate just below the cutoffs. Columns 4 and 5 of Table 7 confirm this. We estimate an elasticity $\varepsilon^{Inc.,MU}$ of 1.101 for women, 2.5 times larger than the elasticity for men (0.430). These findings are consistent with existing evidence that women face more severe credit constraints than men when attempting to start a business (Morazzoni and Sy, 2022). The corresponding elasticities for unincorporated entry, reported in columns 4 and 5 of Table IA.5 in the Online Appendix, are significantly lower and similar for men and women.

In addition, we investigate whether the effect of funding on entry varies across time. Portugal faced significant financial stress during the period we analyze, first from the global financial crisis of 2008 and then from the European sovereign debt crisis in 2011, which both resulted in sharp reductions in credit supply (Iyer et al., 2014; Bonfim et al., 2025). If our results on incorporated entry are driven by liquidity constraints, we would expect them to be stronger when financial stress is high. Figure IA.1 in the Online Appendix shows that Banco de Portugal's financial stress index (Banco de Portugal, 2025) was close to zero up until August 2007, at which point it rose dramatically, remaining elevated for the rest of the sample period.[13] We accordingly split our sample into recipients who became unemployed before August 2007 and from then onward. Figures 5e and 5f plot the fraction of MU incorporated entrepreneurs for the two periods, and columns 6 and 7 of Table 7 present RD estimates. In line with a liquidity channel, we obtain elasticities of 0.458 before August 2007 and 0.751 after that. For unincorporated entry, our estimates are very similar in the two periods (columns 6 and 7 of Table IA.5 in the Online Appendix).

4.3.4 Long-Run Entrepreneurship

We also ask whether UI benefits also affect entrepreneurship in the longer run, and not just immediately post-UI. We focus on entrepreneurship rates 7 years after the date of dismissal, which is the longest we can observe all UI-recipients in the sample without right-censoring. Table 8 shows that UI funding impacts incorporated entrepreneurship even at this 7-year horizon, and that the effect is entirely driven by participation in the MU program. The long-run semi-elasticity of entrepreneurship to funding through the program in column 1 equals 0.010 ($p < 0.01$), the same as the short-run estimate from column 1 of Table 5. The long-run elasticity equals 0.345, about half of the short-run elasticity, with the difference driven by the fact that the long-run entrepreneurship rate is nearly double the short-run rate immediately post-UI. Columns 2-4 show that the elasticities for UI recipients who originally selected into entrepreneurship through benefit suspension or exhaustion, as well as for those who did not initially select into entrepreneurship out of UI, are close to zero and insignificant.

In columns 5-7, we decompose the long-run effect through the MU program by running separate regressions for incorporated entrepreneurs who remain at the firm they founded through the program, for those who left their original firm and founded a new one, and for

[13]The index aggregates stress indicators for financial intermediaries and from the bond, equity, money, and foreign exchange markets. See Braga et al. (2014) for details.

those who initially started an unincorporated business and then transitioned into incorporated entrepreneurship. We find that about three-quarters of the effect is driven by entrepreneurs who remain at their original firm, with the remainder driven by transitions from unincorporated entrepreneurship. Figure 6 presents the corresponding graphical evidence. The fact that transitions account for a non-trivial share implies that focusing on the short-run effects of the program understates its impact on incorporated entrepreneurship.

By contrast, we find no significant lasting effects of UI funding on unincorporated entrepreneurship (see Table IA.6 in the Online Appendix). The long-run elasticity through the MU program is 0.046 (column 1), evenly driven by entrepreneurs who started as unincorporated and those who transitioned from incorporated to unincorporated entrepreneurship (columns 5 and 6). The corresponding estimates for individuals who selected into entrepreneurship via benefit suspension or exhaustion (columns 2 and 3) are even smaller. The largest long-run elasticity we estimate is for UI recipients who did not select into entrepreneurship out of UI (0.086), but it is not statistically significant at conventional levels.

5 Effect of UI Funding on Firm Outcomes

We next turn to the effect of UI funding on outcomes for incorporated businesses. Aside from their independent interest, these intensive margin estimates can also shed light on the mechanism underlying our results on entry. As Chodorow-Reich et al. (2024) show, a positive effect of personal wealth on firm performance is consistent with a liquidity channel, but not with the alternative view that wealth simply increases the non-pecuniary benefits of entrepreneurship and thus the preference for it.

5.1 Methodology

To estimate the effect of UI funding on incorporated business outcomes, we must confront an additional selection problem. We only observe outcomes for UI recipients who choose to become incorporated entrepreneurs, and discontinuities in available funding that affect entry may cause discontinuities in the *ex ante* quality of entrants at the cutoffs. In fact, in standard models of entrepreneurial selection (Evans and Jovanovic, 1989) the marginal entrepreneurs induced to enter by a wealth shock have lower productivity than inframarginal ones. This implies that simple RD estimates of the effect of funding on outcomes among all entrants in the sample are biased downward.

We recover the intensive margin effects of funding by applying the selection correction developed by Chodorow-Reich et al. (2024). Their method relies on two mild monotonicity assumptions to identify inframarginal entrepreneurs: that both firm size and the return to entrepreneurship relative to employment increase with entrepreneurial productivity. In a general model of entrepreneurial selection, these assumptions imply that shocks to personal wealth preserve entrepreneurial size ranks: even if the shock causes entrepreneurs to grow their businesses, the n^{th} ranked inframarginal entrepreneur in terms of size before the shock will retain the same rank after the shock. In particular, marginal entrepreneurs induced to enter or to incorporate by the shock will rank below n. The selection bias when comparing two sets of entrepreneurs with exogenously different levels of personal wealth can thus be overcome by identifying a size threshold $\bar{k}$ such that dropping all firms with $k < \bar{k}$ in the high-wealth group equalizes the rate of incorporated entrepreneurship in the two groups.

To apply this correction to each option k in our setting, we need one additional assumption: that the increase in funding at the cutoffs does not cause inframarginal entrepreneurs to switch into a different k, e.g., from suspending their benefits to collecting them up front through the MU program. This ensures size rankings are preserved within each k. If we instead focus on the MU program alone, a milder assumption is sufficient: that there are no switchers out of MU and that any switchers into MU rank below inframarginal MU entrepreneurs. This is a plausible assumption since MU entrepreneurs are likely to need the up-front funding and to have higher potential than those who suspend or exhaust their benefits, given their higher incorporation rates and the repayment risk they take on.

We start by estimating the fraction of incorporated entrepreneurs above the cutoffs who would not have entered under option k without the additional liquidity:

$$\theta^k = 1 - \frac{\lim_{a\uparrow 0} \mathbb{E}[E_{i,c}^{Inc.,k} | A_{i,c} = a]}{\lim_{a\downarrow 0} \mathbb{E}[E_{i,c}^{Inc.,k} | A_{i,c} = a]}. \tag{3}$$

We then drop the smallest $\theta^k \times 100$ percent of firms above the cutoffs. Following Chodorow-Reich et al. (2024), our measure of size is total assets. We use cumulative total assets up to age 8, which captures productivity differences reflected in firm growth trajectories, not just initial size (Pugsley et al., 2021). Finally, we estimate equation (1) in the resulting sample of firms, replacing $E_{i,c}^{Inc.,k}$ with firm outcomes.

One concern with this method is that firm size might also reflect post-entry productivity shocks and other forms of residual heterogeneity, and not just the *ex ante* productivity differences that determine entry decisions. Pugsley et al. (2021) show that most of the size variation across firms reflects *ex ante* heterogeneity, particularly among young firms, but any residual variation might cause changes in size rankings. Chodorow-Reich et al. (2024) show that their methodology is robust to such residual heterogeneity as long as it is uncorrelated with initial wealth and productivity, and if the sample is further trimmed to exclude firms close to the size threshold. We follow their approach and present robustness checks where we drop the bottom 10% of firms by size, both above and below the cutoffs, after applying the selection correction procedure.

5.2 Results

Figure 7 plots average log outcomes by distance to the age cutoffs, after applying the selection correction. We measure these outcomes cumulatively up to age 8, the upper age limit in our firm panel, which has two advantages. First, it summarizes the entire trajectory of these firms over the available horizon. Second, it naturally accounts for firm exit and avoids issues with survivor bias, since log outcomes are defined provided firms report at least one positive value between ages 0 and 8.

Value added, TFPQ, employment, and capital (fixed assets) all display sharp increases at the cutoffs (Panels (a)–(d)). The jumps in value added and capital are visibly larger than those in employment, leading to an increase in the average product of labor (Panel (e)), with no discernible change in the average product of capital (Panel (f)). Table 9 presents RD estimates for these and other firm outcomes. For log outcomes, $\tau^{Inc.,MU}$ can be interpreted as an elasticity with respect to UI funding. Panel A reports elasticities of 2.131 for sales, 2.149 for value added, 1.076 for employment, and 1.666 for capital, along with semi-elasticities of 0.532 for EBITDA margins (EBITDA/Sales) and 0.397 for survival. Panel B reports elasticities of 0.973 for the average product of labor, 0.820 for TFPR, 1.418 for TFPQ, and a smaller, statistically insignificant elasticity of 0.505 for the average product of capital.

These results indicate that the MU program facilitates capital deepening and firm growth, while also triggering productivity-enhancing (TFPQ) investments that allow firms to expand without reducing TFPR. In the canonical misallocation framework of Hsieh and Klenow (2009), firms with higher TFPR face frictions that keep them inefficiently small, such as financial constraints, and reallocating inputs toward these firms increases aggregate productivity. Combined with the fact that program firms have above-average TFPR levels (Table 3), our findings not only support a liquidity channel, but also suggest that the MU program funding may improve aggregate productivity through both within-firm productivity growth and reduced misallocation.

Panel B of Table 9 also presents estimates for the elasticities of initial equity (1.186) and debt (0.921) with respect to funding. The effect on equity is not mechanical, since UI recipients are not required to invest the full amount of their benefits. They can choose how much to withdraw through the program and suspend the remainder, which they can access if the business fails. Evaluated at the respective means, i.e. multiplying by the mean of the corresponding outcome and dividing by the mean of available funding, our estimates imply that each euro of funding available through the MU program generates €1.04 of initial equity and €3.73 of initial debt. Rather than crowding out private investment, the program facilitates access to additional funding.

Chodorow-Reich et al. (2024) also estimate a pass-through of one from stock market wealth into initial equity, which facilitates a comparison with their findings despite the different nature of the underlying shock. They report that each Norwegian krone (NOK) of stock wealth increases annual sales, value added, and capital by 18.9, 8.5, and 0.9 NOK, respectively.[14] Our estimates imply that each euro of UI benefits has smaller effects on sales and value added (€14.6 and €4.2), but a larger effect on capital (€2.5).[15] While Chodorow-Reich et al. (2024) do not report estimates for TFPR or TFPQ, they find no effect on the average product of labor, which may partly reflect the relatively weaker effect on capital in their setting. It bears emphasizing that Chodorow-Reich et al. (2024) study a shock to private wealth, not a public funding program. The fact that our estimates are in the same ballpark reinforces the notion that the program's screening mechanism effectively mitigates adverse selection issues.

Table IA.7 in the Online Appendix shows that our estimates are robust to excluding the bottom 10% of firms by size, both below and above the cutoffs, after applying the selection correction. As discussed in Section 5.1, this exclusion helps mitigate potential bias from residual firm-size heterogeneity unrelated to *ex ante* productivity. The similarity of coefficients with and without these firms suggests that such bias is limited. Table IA.7 in the Online Appendix presents an additional robustness check in which size ranks for the selection correction are defined using assets at age 1 rather than cumulative assets up to age 8, again dropping the bottom 10% of firms after the correction. This further reduces the influence of *ex post* shocks, but also discards information from *ex ante* differences in growth paths (Pugsley et al., 2021). While the resulting estimates are somewhat smaller and noisier, the qualitative patterns remain unchanged.

By contrast, we find no effects of funding on business outcomes without applying the selec-

[14] We divide the coefficients reported for each outcome by the coefficient for equity in their Table 5, given a pass-through of one.

[15] We multiply our estimated elasticities for each outcome and for initial equity by the respective sample means, taking annual averages of cumulative outcomes, and then divide the effect on each outcome by the effect on initial equity, given a pass-through of one.

tion correction (Table IA.9 in the Online Appendix), or among entrepreneurs who suspend or exhaust their benefits before entry (Tables IA.10 and IA.11 in the Online Appendix). For these groups, no selection correction is necessary, since UI funding does not affect incorporated entry in the first place (see columns 2 and 3 of Table 5).

6 Cost-Benefit Analysis

6.1 Effect of Program on Job Creation and Output

Our RD design lends itself well to a direct evaluation of the overall costs and benefits of the program. We include in the costs the amount of UI benefits claimed upfront by MU entrepreneurs, net of repayments. In terms of benefits, we focus on job creation and output among the businesses created through the program. We do not account for subsequent businesses created by program participants, namely for the transitions from unincorporated to incorporated entrepreneurship documented in Table 8. We also do not account for spillovers or general equilibrium effects on other businesses, so we caution that the aggregate impact of the program is not fully captured by our estimates. We restrict our analysis to incorporated businesses, since we do not observe outcomes for unincorporated businesses.

Letting Y^{MU} denote an outcome of interest for MU entrepreneurs and π^{MU} the amount of funding taken through the program, we estimate:

$$\tau_Y^{MU} = \frac{\lim_{a\downarrow 0} \mathbb{E}[Y_{i,c}^{MU}|A_{i,c}=a] - \lim_{a\uparrow 0} \mathbb{E}[Y_{i,c}^{MU}|A_{i,c}=a]}{\lim_{a\downarrow 0} \mathbb{E}[\pi_{i,c}^{MU}|A_{i,c}=a] - \lim_{a\uparrow 0} \mathbb{E}[\pi_{i,c}^{MU}|A_{i,c}=a]}, \tag{4}$$

in the full sample of UI recipients, assigning $Y^{MU} = \pi^{MU} = 0$ to non-participants in the program and to unincorporated MU entrepreneurs. Equation (4) expresses the change in Y^{MU} at the cutoffs, through both extensive and intensive-margin effects, as a ratio of the change in funding used by incorporated program entrepreneurs.

Table 10 summarizes the cumulative effects of program funding on job creation and output.

By age 1 (i.e. after two years), we find that the program creates 1.508 job-years per €10,000 euros of funding, which implies a cost-per-job-year of €6,630, and that it generates €4.543 of sales and €1.279 of value added per €1 of funding. The program's effects are persistent, which translates into substantially higher returns at longer horizons. By age 8, job creation increases to 5.313 job-years per €10,000 euros of funding, which implies a cost of €1,882 per job-year. We also obtain substantially larger point estimates for the effects on output—€21.267 for sales, and €5.422 for value added—although these are not statistically significant at conventional levels.[16]

To the best of our knowledge, no similar estimates for entrepreneurship programs have been reported in the literature, but we can compare our estimates with those for other types of public spending. The closest studies that report such estimates are those on loan guarantees for small businesses. These studies tend to focus on the costs-per-job created or preserved, and compute the costs from defaults on guaranteed loans. Our estimates are perhaps most comparable with those from Bonfim et al. (2023), who study a loan guarantee program in Portugal in the same period.[17] Eligible firms must have at least three years of financial statements, and meet a series of minimum requirements in terms of size, profitability and solvency. At the two-year horizon, they estimate a cost-per-job of €11,788, or €5,894 per job-year, similar to our estimate of €6,630 at the same horizon. However, they show that the effect of the guarantee on employment does not last beyond the first two years, whereas in our setting the effects persist, yielding a substantially lower cost by age 8.

Brown and Earle (2017) study the effect of loans guaranteed by the Small Business Administration in the U.S., and estimate a cost-per-job of $21,000–$25,000 over three years, or $7,000–$8,333 per job-year. We obtain a lower estimate of €4,828 at the same horizon. Barrot et al. (2024) perform a somewhat different exercise using a loan guarantee program created in France

[16] Imputing outcomes for unincorporated businesses using population averages dampens these effects, as might be expected given their lower potential, but not by much, since the amount of funding taken by unincorporated MU entrepreneurs is also lower. For each surviving business, we use the average employment, compensation, sales, and value-added for our sample period (INE, 2024). The results are presented in Table IA.12 in the Online Appendix. Our estimates decline by 20% on average and the cost per job-year rises to €7,380 at age 1 and €2,247 age 8.

[17] They report estimates for 2008-2013 and 2014-2018. We focus on the 2008-2013 estimates, which closely overlap with those from our Social Security-IES linked data for UI recipients in 2007-2012.

during the 2008 financial crisis: they analyze the effect of the program on employment at the worker level, tracking the movement of workers across firms. They estimate a particularly low cost-per-job-year of €425 over seven years. Our firm-level estimate at the same horizon equals €2,201. One important contrast, however, is that Barrot et al. (2024) find that the loan guarantees prevent the reallocation of workers towards more productive firms, reducing aggregate productivity. We find the opposite: MU firms have higher TFPR than firms created outside UI (see Table 3), and program funding causes firms to grow in both size and TFPQ, without decreasing TFPR (see Table 9). These findings suggest that the MU program reduces misallocation, instead of increasing it.

6.2 Impact of Repayment Obligation on Those Who Fail

The evidence presented thus far points to the effectiveness of the MU program's screening mechanism in stimulating job creation and output. However, this mechanism relies on a risky proposition for prospective entrepreneurs: they must forgo other professional activities for three years or repay the amount received through the program. This raises the question of what happens to program participants whose business fails within this three-year window. Do they find other jobs that pay enough to offset the repayments? Do they access other forms of public support?

Social Security offers several measures to ease the transition. First, repayments can be made in monthly installments, with a maximum term of 36 months during the period we study. Second, failed entrepreneurs are eligible for UI if they worked for at least 15 months after starting their business (6 months for UA, and 12 months for UI from July 2012 onwards). In addition, Portugal's minimum income program guaranteed around €200 per adult and €100 per child in each household at the time, and these amounts were exempt from seizure for debt collection purposes, including repayment of MU funds.

To evaluate the impact of the repayment obligation, we compare the trajectories of entrepreneurs who exit within versus after the three-year window. Table 11 presents descriptive statistics by year of failure. Only 2.1% of all MU entrepreneurs exit in the first year (i.e., at age 0). The share rises to 5.4% at age 1 and 4.8% at age 2, totaling 12.3% of failures within three years. Another 8.7% fail at ages 3 and 4, respectively. Entrepreneurs who exit in the first three years are slightly younger and more likely to be female than those who fail later. They also have 15–30% lower pre-unemployment wages and MU funding, and 15–40% lower incorporation rates.

Among those who fail at age 0, 43% repay the amount received. This share drops to 27% for age-1 exits and to 19% for age-2 exits. Assuming repayment is made in 36 monthly installments, the average monthly amount equals €245–290. The remaining entrepreneurs who fail at ages 0-2 have no record of repayment demanded by Social Security. This could be because they do not engage in other normally remunerated activities during the repayment window, or because enforcement is not pursued. Interestingly, 12% and 7% of those who fail at ages 3 and 4, respectively, also repay, perhaps because business performance was disappointing and/or an attractive outside opportunity arose, making repayment worthwhile while maintaining the business. For these cases, monthly repayments average €335–390.

The remaining rows in Table 11 describe post-exit trajectories. We focus on the first three years after failure to avoid right-censoring. During this period, 78–80% of those who fail within the first three years return to work. About two-thirds of those who return to work do so within the first six months after failure, and over a quarter receive UI. Among those who do not return to work, 2–4% retire or die, 1–2% receive government support—either UI or guaranteed minimum income—and 15–16% are missing from Social Security records altogether.

Those who fail at ages 3 and 4 have somewhat lower employment rates (72–77%), higher rates of retirement or death (4–5%), similar levels of government support (2%), and a higher share missing from the Social Security system (17–20%). While we have no direct information on these missing individuals, one possibility is that they emigrated, as emigration rose substantially following the sovereign debt crisis in 2011. Another possibility is that they turned to informal employment. In any case, the repayment obligation is unlikely to explain these missing cases, since the fraction is higher among those who fail after three years.

We also examine the impact of early failure on personal income, accounting for repayments. For each entrepreneur, we compute income as the sum of labor earnings reported to Social Security and any government support, such as UI, pensions, or guaranteed minimum income, assigning zero to those with no income reported. For those who are required to repay the funds, we subtract the corresponding monthly installment, assuming repayment occurs over 36 months. We then estimate differences in income trajectories for entrepreneurs who fail at age f, for $f \in [0, 3]$, relative to those who fail at age 4:

$$\Delta w_{i,t} = \sum_{f=0}^{3} \sum_{t=0}^{2} D_t \left(\beta_{f,t} F_{f(i)} + \gamma_t\right) + \epsilon_{i,t}, \tag{5}$$

where $\Delta w_{i,t}$ denotes the change in income between the year before unemployment and t years after failure, D_t is an indicator for time t, and $F_{f(i)}$ is an indicator for failure at age f. The $\beta_{f,t}$ coefficients capture income changes for individuals who fail at ages 0-3 relative to those who fail at age 4. We include age-3 exits in the event study—and use age-4 exits as the comparison group—to allow for the possibility that some entrepreneurs keep poorly performing businesses alive through age 3 to avoid repayment. Age-4 exits, by contrast, occur at least one year after the repayment window closes and are therefore less likely to be influenced by repayment considerations. To examine whether entrepreneurs who exit at different horizons exhibit differences in income trends prior to unemployment, we also re-estimate equation (5) substituting $j \in [-4, -1]$ for t, with j denoting years before unemployment.

Figure 8 plots point estimates and 95% confidence intervals for the $\beta_{f,j}$ and $\beta_{f,t}$ coefficients. We find no trend differences before unemployment. Post-failure, the results suggest that exits at ages 0–2 are followed by more favorable income dynamics compared to age-4 exits, even after subtracting repayments. The earlier the exit, the more pronounced the relative gains. By the third year post-exit, individuals who exited at ages 0–2 have income gains of around €100–250 per month relative to those who exited at age 4. Those who exit at age 3, by contrast, experience a relative income loss of around €100 per month in the year after exit, but the gap closes by the third year. When we restrict the sample to entrepreneurs who exit at ages 0–3 and are subject to repayment, while retaining all age-4 failures in the comparison group, we find that age-3 exits also experience a relative post-failure income gain, and that the gains for earlier exits are slightly stronger (Figure IA.2 in the Online Appendix).

Taken together, these findings suggest that the repayment obligation does not impose significant hardship. Early exits appear to be often opportunistic: entrepreneurs who fail early tend to find alternative sources of income relatively quickly and are able to meet their repayment obligations without significant reductions in net income. The evidence does indicate that some individuals who exit at age 3 experience temporary income declines, possibly because they delay their exit to avoid repayment, but the effect appears short-lived.

7 Conclusion

This paper shows that a public program that combines up-front funding for UI recipients with a simple screening mechanism, the temporary forgoing of other professional activities, can encourage the type of non-frontier incorporated entrepreneurship that drives most job creation.

Our results indicate that the program successfully targets liquidity-constrained entrepreneurs and also unlocks additional private sector funding. This suggests that there is a role for government funding in mitigating the misallocation of talent and capital caused by financial frictions. Moreover, the fact that the effect of MU funding is larger in periods of financial stress suggests that this type of UI expenditure can be particularly effective as an automatic stabilizer, since it stimulates labor demand in addition to supporting the unemployed. A full examination of these implications requires a general equilibrium framework, which we leave for future research.

Most OECD countries offer financial support for transitions from unemployment to self-employment (OECD and European Commission, 2021), and our findings have important implications for the design of these programs. The program's screening mechanism offers several advantages. It does not burden government officials with the difficult and skill-intensive task of evaluating new ventures, and it leaves little room for favoritism and corruption. It also avoids heavy administrative costs with project-level due diligence and appraisal. An interesting avenue for future research is whether similar mechanisms could be effectively applied in other public programs beyond UI, such as investment subsidies or loan guarantee schemes.

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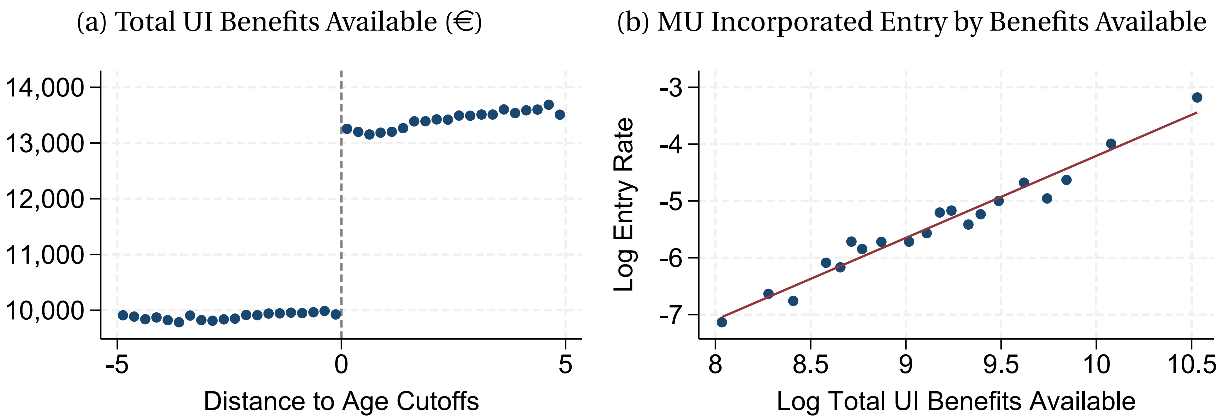
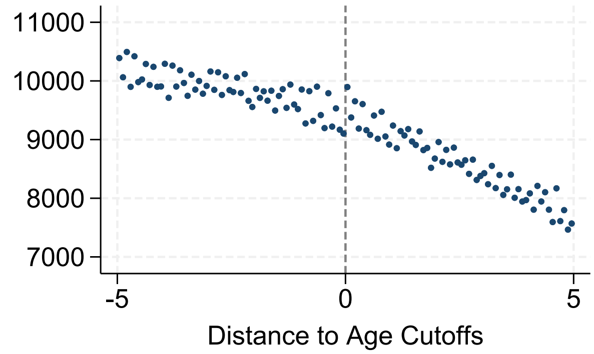
Figure 1: Identification Strategy



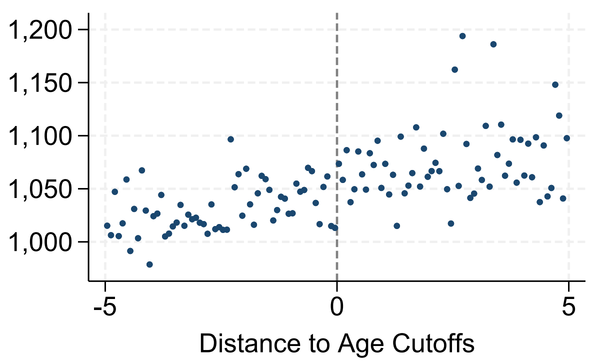
Panel A plots the average total benefits available to UI recipients in quarterly bins by distance to the age cutoffs in the pooled sample. Panel B presents a binned scatter plot of the log rate of incorporated entry through the MU program as a function of the log of total UI benefits available. We sort observations by benefits into 20 equal-sized bins, and we plot the log entry rate and the log average benefit amount for each bin, along with a regression line fitted on these points.

Figure 2: RD Validity

(a) Number of Observations

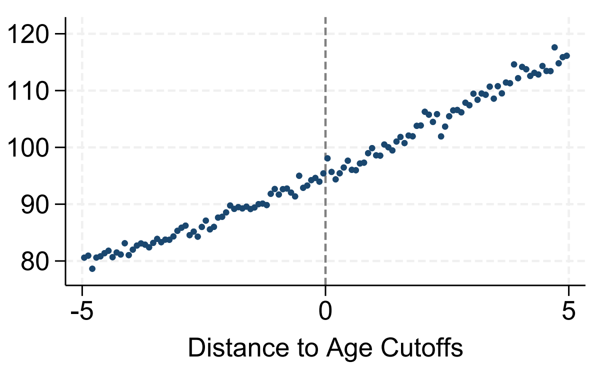


(b) Average Pre-Unemployment Wage (€)

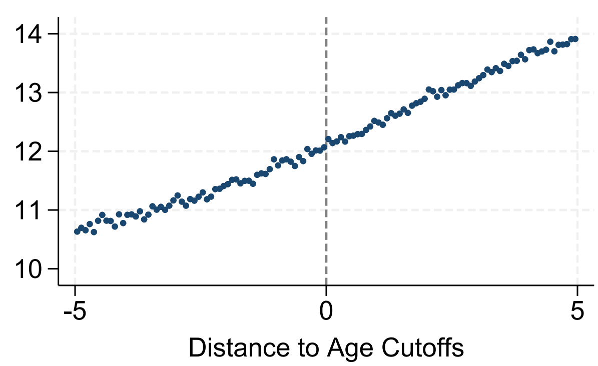


(c) Male Fraction

(d) Months of Contributions Since Last Spell

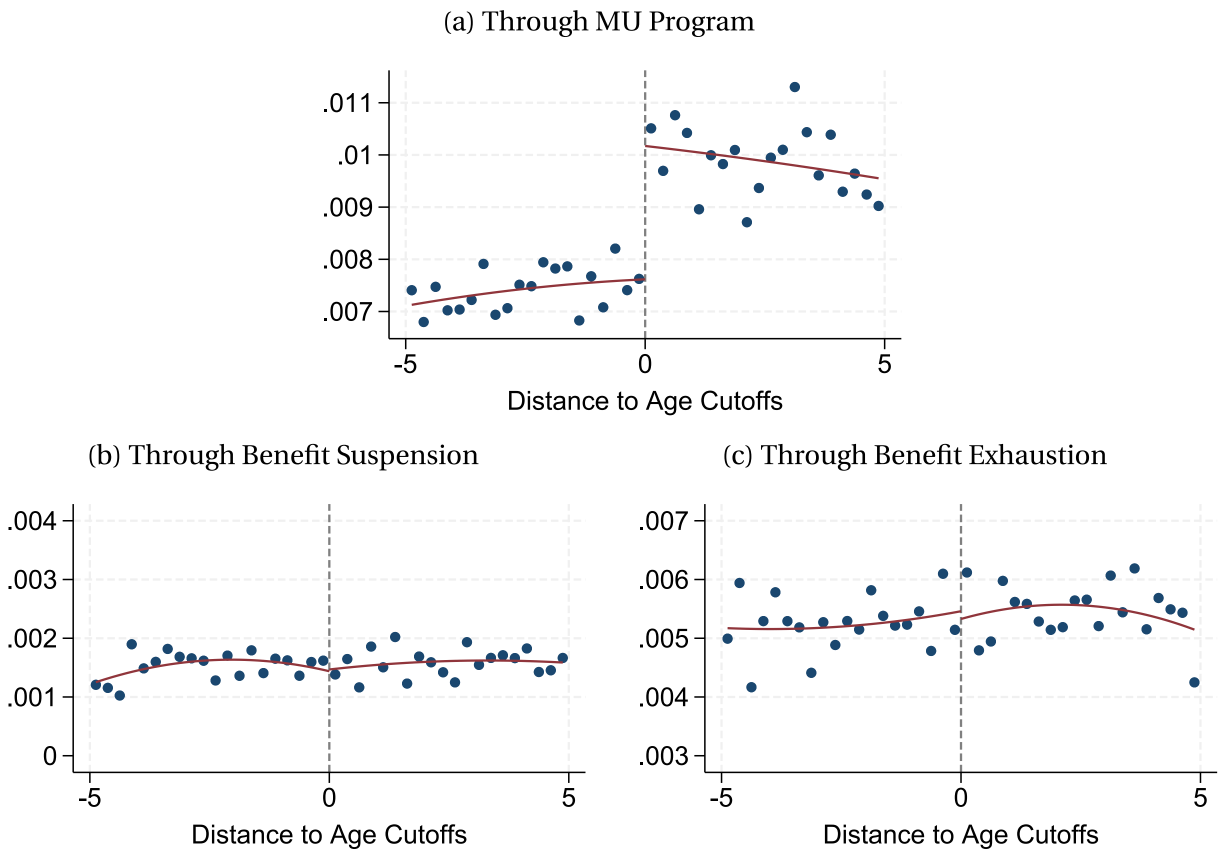


(e) Years of Contributions in Last 20



This figure plots predetermined covariates in quarterly bins by distance to the age cutoffs in the pooled sample. Panels A to E respectively plot the number of observations, the average pre-unemployment wage, the fraction of male UI recipients, the number of months with social security contributions since the last UI spell, and the number of years with contributions in the last 20 years.

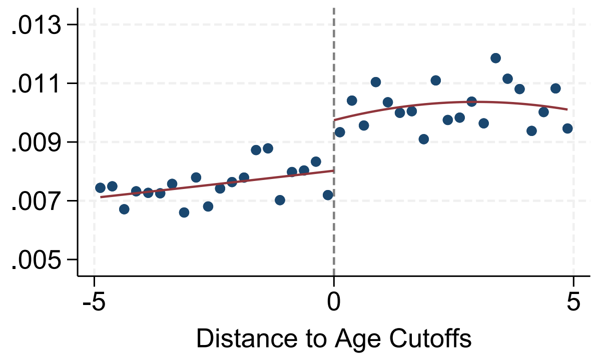
Figure 3: Selection Into Incorporated Entrepreneurship



This figure plots the fraction of incorporated entrepreneurs through each of the options available to UI recipients. Observations are sorted into quarterly bins by distance to the age cutoffs in the pooled sample. Panels A to C respectively focus on those entrepreneurs who select into the MU program, those who suspend their uI benefits, and those who exhaust their benefits.

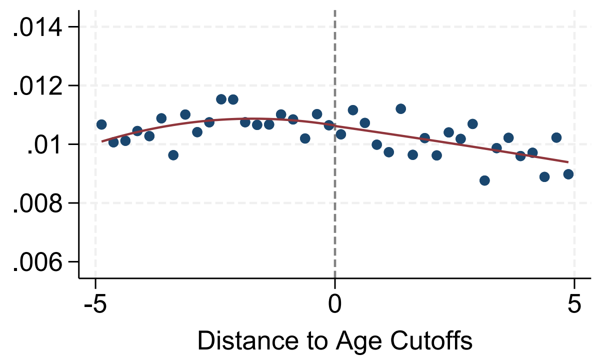
Figure 4: Selection Into Unincorporated Entrepreneurship

(a) Through MU Program



(b) Through Benefit Suspension

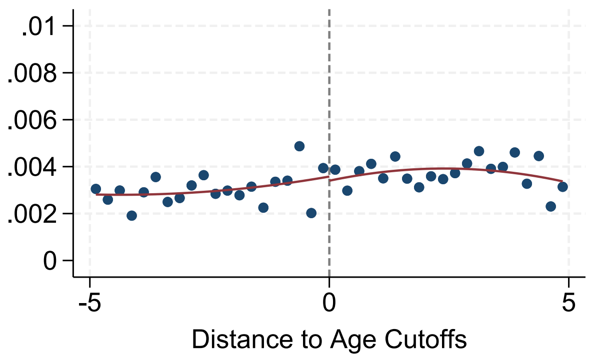
(c) Through Benefit Exhaustion



This figure plots the fraction of unincorporated entrepreneurs through each of the options available to UI recipients. Observations are sorted into quarterly bins by distance to the age cutoffs in the pooled sample. Panels A to C respectively focus on those entrepreneurs who select into the MU program, those who suspend their uI benefits, and those who exhaust their benefits.

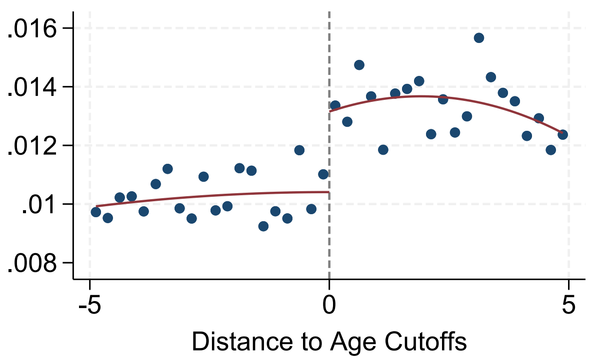
Figure 5: Heterogeneity in Selection Into MU Incorporated Entrepreneurship

(a) Bottom Tercile of Pre-UI Wage



(b) Top Tercile of Pre-UI Wage

(c) Men



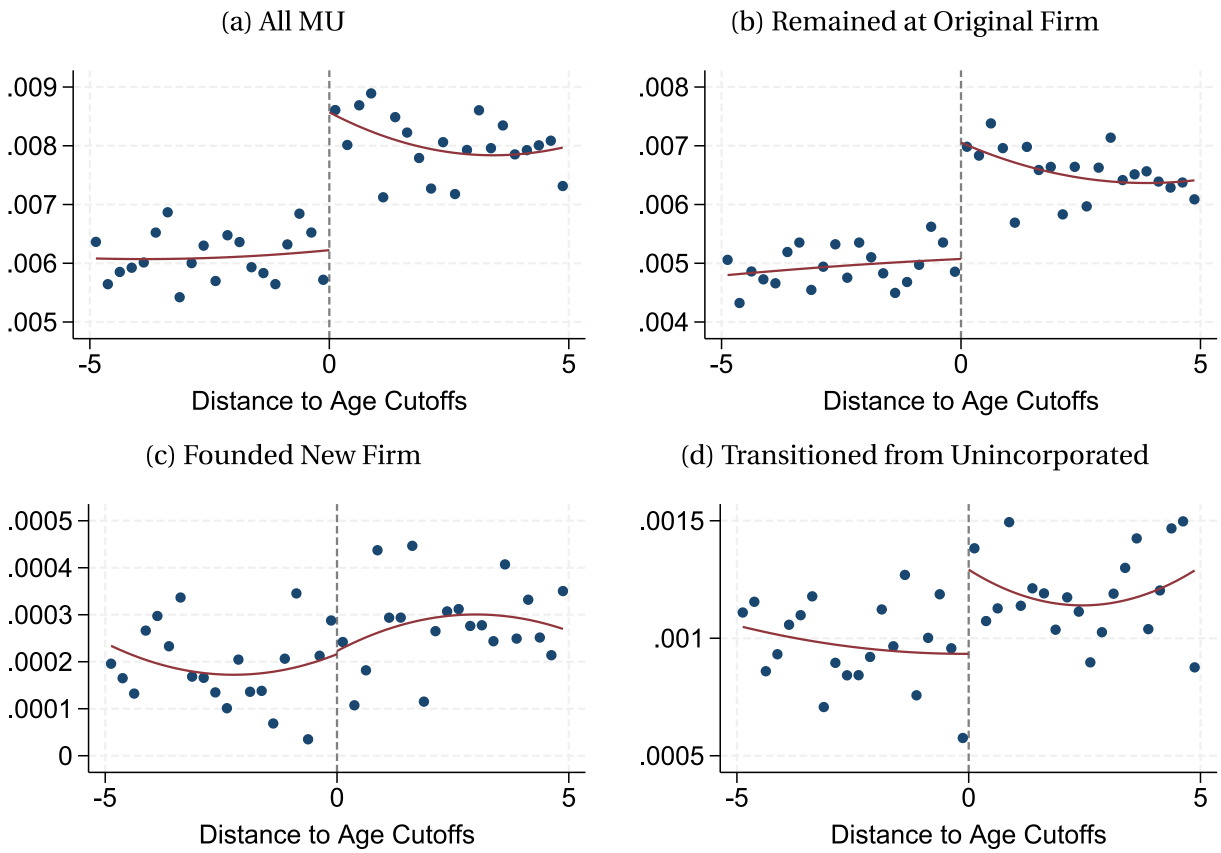
(d) Women

(e) Low Financial Stress (Before 08/2007)

(f) High Financial Stress (08/2007 onwards)

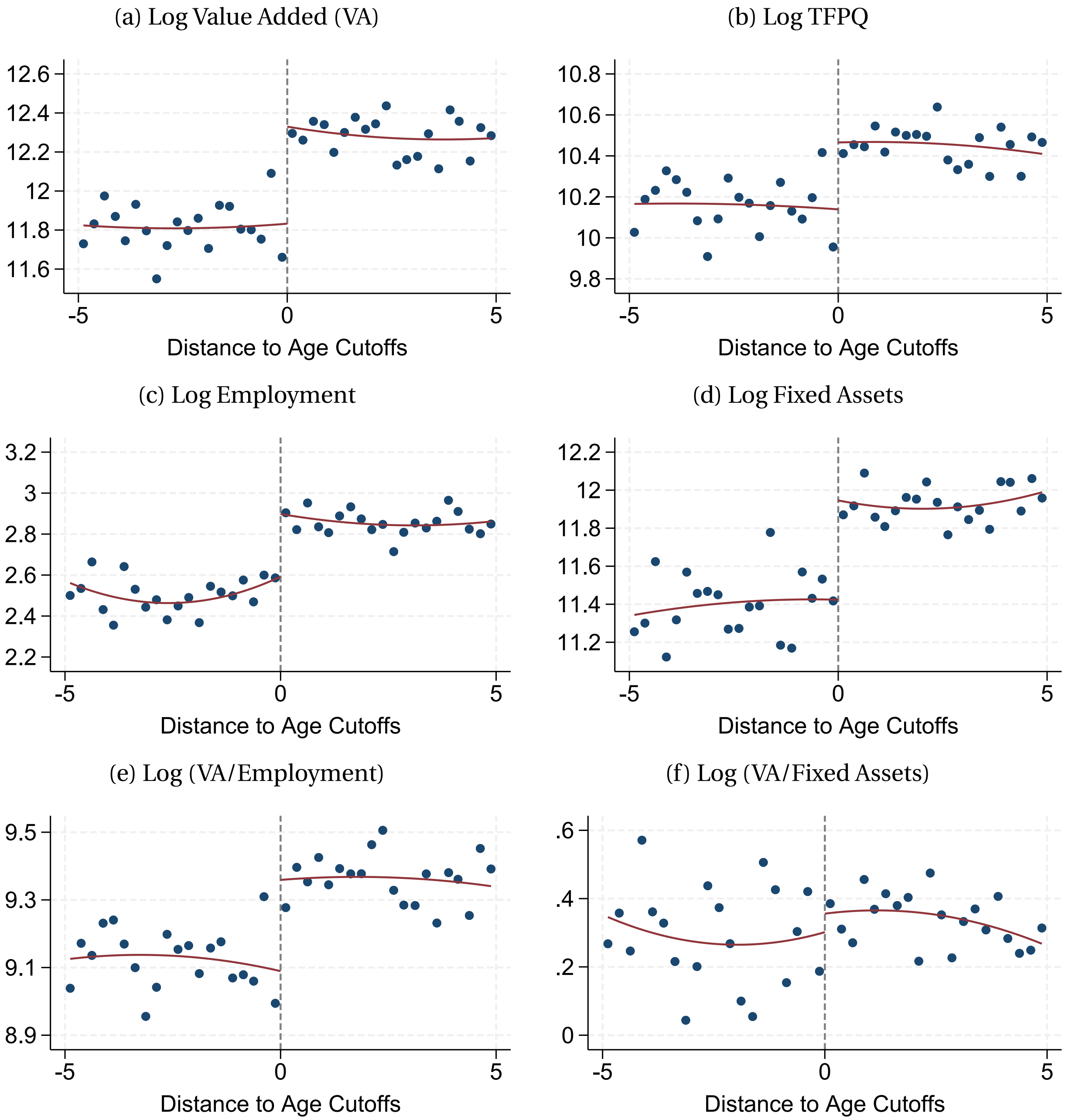
This figure plots the fraction of incorporated entrepreneurs through the MU program for different sub-samples. Observations are sorted into quarterly bins by distance to the age cutoffs in the pooled sample. Panels A and B focus on UI recipients in the bottom and top terciles of pre-unemployment wages. Panels C and D focus on men and women. Panels E and F split recipients by the level of financial stress at the time of dismissal, using the ICSF index of financial stress from Banco de Portugal (Braga et al., 2014).

Figure 6: Long-Run Incorporated Entrepreneurship through MU



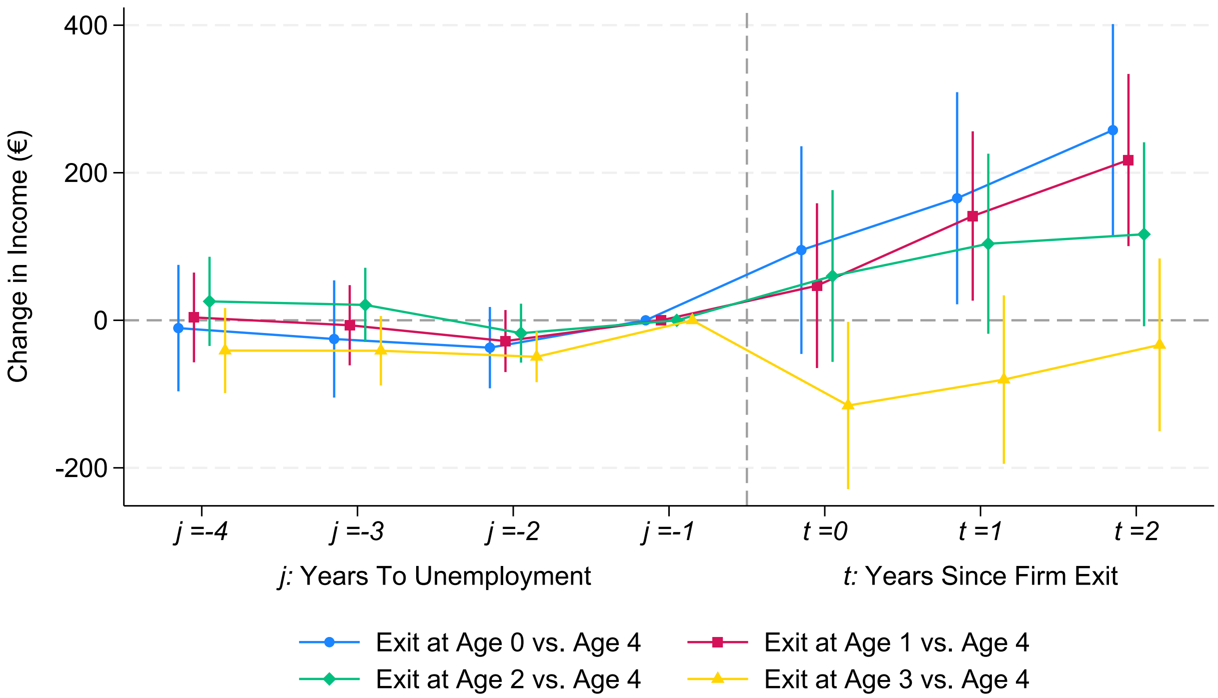
This figure plots the fraction of UI recipients who selected into the MU program and who were incorporated entrepreneurs 7 years after dismissal. Observations are sorted into quarterly bins by distance to the age cutoffs in the pooled sample. Panel A shows the overall rate of incorporated entrepreneurship. Panels B–D decompose this rate into three components: Panel B shows entrepreneurs who remain with their original incorporated firm; Panel C shows those who exited their original incorporated firm and started a new one; and Panel D shows those who transitioned from unincorporated to incorporated entrepreneurship.

Figure 7: Business Outcomes for Inframarginal MU Incorporated Entrepreneurs



This figure plots business outcomes for inframarginal incorporated entrepreneurs through the MU program. Observations are sorted into quarterly bins by distance to the age cutoffs in the pooled sample. Applying the methodology proposed by (Chodorow-Reich et al., 2024) to our setting, we restrict the sample to inframarginal entrepreneurs by dropping the smallest firms above the cutoffs, with size measured by cumulative assets up to age 8, such that the MU incorporated entrepreneurship rate above the cutoffs equals the rate immediately below the cutoffs.

Figure 8: Income Dynamics for MU Entrepreneurs Who Exit Early



This figure plots point estimates and 95% confidence intervals for differences in income trajectories between entrepreneurs who exit between ages 0-3 and those who exit at age 4. Changes in income are measured relative to the year before unemployment ($j = -1$), and are net of repayment. The area to the left of the dashed line shows differences before unemployment, and the area to the right shows differences following firm exit.

Table 1: Potential Duration of Unemployment Benefits

Age	Months of Contributions Since Last UI Spell	Number of Days	Extra Days Per 5 Years of Contributions in the Last 20 Years
<30	≤ 24	270	—
	> 24	360	30
[30,40[	≤ 48	360	—
	> 48	540	30
[40,45[	≤ 60	540	—
	> 60	720	30
≥ 45	≤ 72	720	—
	> 72	900	60

This table reports the duration of UI benefits in days during our sample period. Duration is a function of age at the time of dismissal, the number of months with social security contributions since the last UI spell, and the number of years with contributions in the last 20 years.

Table 2: Summary Statistics for Individuals

	UI Recipients				
	Entrepreneurs				
	MU (1)	Suspend (2)	Exhaust (3)	All (4)	Employed (5)
Age (years)					
mean	38.62	34.00	34.83	36.37	38.44
p10	28.58	24.80	25.39	24.06	25.00
p50	38.03	32.44	33.24	34.74	37.00
p90	49.51	45.94	47.16	51.51	54.00
p99	56.35	55.25	56.77	58.81	64.00
Male	0.63	0.47	0.49	0.48	0.56
Wage (€)					
mean	1,628	1,176	1,357	1,027	1,017
p10	518	518	525	504	524
p50	886	792	881	665	707
p90	2,887	1,773	2,077	1,571	1,795
p99	10,781	6,469	8,492	5,975	4,751
Total UI benefits (€)					
mean	18,868	11,728	13,724	11,393	—
p10	6,244	4,296	4,879	4,284	—
p50	15,094	9,414	10,443	9,414	—
p90	38,188	22,678	28,129	20,577	—
p99	54,624	48,874	52,080	47,356	—
Incorporation Rate	0.488	0.073	0.323	—	—
Observations	18,033	23,131	18,004	1,275,901	20,155,016

This table presents summary statistics for UI recipients and employed workers. The first three columns report numbers for individuals who transition from UI to entrepreneurship: column 1 for those who select into the MU program, column 2 for those who suspend their benefits, and column 3 for those who exhaust them. Columns 4 and 5 respectively characterize the populations of UI recipients and employed workers. For UI recipients, wages are the last base wage reported to Social Security in the last job before unemployment, age is measured at the time of dismissal, and total UI benefits are the product of the daily UI rate and potential UI duration in days. For employed workers, all variables are worker-year averages in our sample period.

Table 3: Summary Statistics for Firms (Age 8)

	Mean		p50		p99	
	MU	Non-UI	MU	Non-UI	MU	Non-UI
	(1)	(2)	(3)	(4)	(5)	(6)
Size						
Sales	289,108	292,877	94,772	96,750	3,227,418	3,201,807
Value Added (VA)	85,568	90,362	39,008	37,979	791,246	846,971
Wage Bill	63,018	63,052	27,912	27,448	569,736	550,738
EBITDA	17,522	23,003	5,606	6,029	268,869	329,993
Employment	3.589	3.611	2.000	2.000	29.000	30.000
Fixed Assets	57,074	93,399	13,664	14,980	788,651	1,256,289
Total Assets	191,146	271,254	72,218	85,616	2,152,807	2,822,268
Productivity						
VA/Employment	20,089	22,131	16,099	16,403	99,049	133,353
VA/Fixed Assets	17.418	16.994	2.997	2.462	423.068	430.995
TFPR	1.134	0.993	0.710	0.709	6.054	6.134
TFPQ	1.001	1.004	0.560	0.565	6.205	6.873
Initial Funding						
Equity	16,757	21,614	5,823	5,760	143,252	174,763
Debt	70,283	85,884	37,342	27,872	509,474	887,408
Survival	0.538	0.575	1.000	1.000	1.000	1.000

This table presents summary statistics for incorporated businesses started through the MU program ($N = 6,317$) and for the population of incorporated businesses started outside UI in our sample period ($N = 106,760$). All outcomes are measured at age 8 except initial equity and debt, which are measured at age 1. VA is value added and EBITDA is earnings before interest, taxes, depreciation, and amortization.

Table 4: Industry Composition

	Firms		Employment	
	MU (1)	All (2)	MU (3)	All (4)
Wholesale and retail trade	0.313	0.254	0.282	0.218
Professional, scientific and technical activities	0.175	0.108	0.132	0.068
Accomodation and food service activities	0.105	0.095	0.124	0.114
Manufacturing	0.071	0.079	0.134	0.152
Administration and support service activities	0.065	0.044	0.061	0.046
Construction	0.062	0.116	0.079	0.195
Information and communication	0.043	0.036	0.040	0.024
Other services	0.041	0.036	0.040	0.033
Human health and social work activities	0.024	0.067	0.023	0.040
Arts, entertainment and recreation	0.021	0.021	0.018	0.012
Real estates activities	0.021	0.038	0.017	0.015
Transportation and storage	0.020	0.029	0.014	0.022
Education	0.018	0.018	0.013	0.012
Agriculture, forestry and fishing	0.009	0.037	0.011	0.034
Water supply, sewerage, waste management	0.004	0.002	0.004	0.002
Mining and quarrying	0.000	0.001	0.000	0.001

This table compares the sector distribution of incorporated businesses created through the MU program with that of the population of incorporated businesses started outside UI in our sample period. Columns 1 and 2 report the fraction of firms created by NACE sector at the section level. Columns 3 and 4 report the corresponding share of employment at entry. Sectors with less than 0.1% of employment in the overall population are not reported.

Table 5: Effect of UI Funding on Entry

	Incorporated			Unincorporated		
	MU	Suspend	Exhaust	MU	Suspend	Exhaust
	(1)	(2)	(3)	(4)	(5)	(6)
$\tau^{l,p}$	0.010***	-0.001	0.001	0.007*	0.007*	-0.000
	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)
$\varepsilon^{l,p}$	0.649	-0.054	0.050	0.189	0.205	-0.005
First Stage	0.279	0.280	0.276	0.281	0.276	0.281
First-stage z	59.6	56.8	64.8	54.2	65.0	53.4
Bandwidth	1.523	1.357	1.820	1.209	1.858	1.208
Eff. Observations	343,050	305,931	409,552	272,462	418,095	271,842

This table reports RD estimates of the effect of log total UI benefits on entry obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. In columns 1-3, the dependent variable is an indicator for incorporated entry by UI recipients who select into the MU program (column 1), suspend their benefits (column 2) or exhaust them (column 3). Columns 4-6 present analogous estimates for unincorporated entry. Estimates of $\varepsilon^{l,k}$, the elasticity of entry with respect to funding, are obtained from equation (2) by dividing $\tau^{l,k}$ by the entry rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Sensitivity to Observations Near the Cutoff

	Donut RDs			Narrow Bandwidths	
	1 month	2 months	3 months	1 year	6 months
	(1)	(2)	(3)	(4)	(5)
$\tau^{Inc.,MU}$	0.011***	0.009***	0.010***	0.010**	0.008
	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)
$\varepsilon^{Inc.,MU}$	0.758	0.623	0.703	0.689	0.576
First Stage	0.274	0.264	0.264	0.283	0.290
First-stage z	53.1	49.8	55.6	40.6	28.9
Bandwidth	1.601	1.856	2.674	1.000	0.500
Eff. Observations	341,854	379,948	542,431	225,113	112,889

This table reports RD estimates of the effect of log total UI benefits on incorporated entry through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Columns 1-3 drop observations within 1, 2 and 3 months of the cutoffs, respectively, employing MSE-optimal bandwidths. In columns 4 and 5 the bandwidth is manually set to 1 year and to 6 months, respectively. Estimates of $\varepsilon^{Inc.,MU}$, the elasticity of entry with respect to funding, are obtained from equation (2) by dividing $\tau^{Inc.,MU}$ by the entry rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneous Effects on Incorporated Entry

	Pre-Unemployment Wage					Financial Stress	
	Bottom Tercile (1)	Middle Tercile (2)	Top Tercile (3)	Men (4)	Women (5)	Low (6)	High (7)
$\tau^{Inc.,MU}$	0.000	0.008*	0.020***	0.009	0.011***	0.008	0.010***
	(0.003)	(0.004)	(0.006)	(0.004)	(0.003)	(0.005)	(0.003)
$\varepsilon^{Inc.,MU}$	0.052	0.709	0.861	0.430	1.101	0.458	0.751
First Stage	0.270	0.269	0.286	0.279	0.280	0.289	0.273
First-stage z	38.2	40.9	29.6	37.7	46.8	33.9	52.1
Bandwidth	1.325	1.556	1.166	1.367	1.560	1.606	1.687
Eff. Observations	94,219	115,205	93,263	144,844	186,241	99,290	275,672

This table reports RD estimates of the effect of log total UI benefits on incorporated entry through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Columns 1-3 split the sample by pre-unemployment wage tercile, columns 4 and 5 by gender, and columns 6 and 7 by date of dismissal into a low financial stress period (before August 2007) and a high financial stress period (from August 2007 onwards), using the ICSF index of financial stress from Banco de Portugal (Braga et al., 2014). Estimates of $\varepsilon^{Inc.,MU}$, the elasticity of entry with respect to funding, are obtained from equation (2) by dividing $\tau^{Inc.,MU}$ by the entry rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Long-Run Incorporated Entrepreneurship

	First Post-UI Spell				MU Decomposition		
	MU (1)	Suspend (2)	Exhaust (3)	Non Entrep. (4)	Same Firm (5)	New Firm (6)	Uninc. to Inc. (7)
$\tau^{Inc.,MU}$	0.010***	-0.002	0.001	0.000	0.007***	-0.000	0.003**
	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)	(0.000)	(0.001)
$\varepsilon^{Inc.,MU}$	0.345	-0.058	0.038	0.005	0.253	-0.016	0.094
First Stage	0.281	0.278	0.280	0.278	0.279	0.280	0.282
First-stage z	54.9	61.5	57.7	60.4	59.4	56.0	51.8
Bandwidth	1.221	1.596	1.429	1.590	1.512	1.293	1.066
Eff. Observations	274,947	359,609	321,894	358,336	340,537	291,008	239,929

This table reports RD estimates of the effect of log total UI benefits on incorporated entrepreneurship 7 years after the date of dismissal obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Columns 1-3 present estimates for UI recipients who originally selected into entrepreneurship through the MU program (column 1), benefit suspension (column 2), and benefit exhaustion (column 3), while column 4 reports results for UI recipients who did not select into entrepreneurship in their first post-UI spell. Columns 5-7 in Panel A present estimates for MU entrepreneurs who remain at the incorporated firm they founded through MU (column 5), who left their original firm and founded a new one (column 6), and who originally started an unincorporated business but transitioned to incorporated entrepreneurship (column 7). Estimates of $\varepsilon^{Inc.,MU}$, the elasticity of entrepreneurship with respect to funding, are obtained from equation (2) by dividing $\tau^{Inc.,MU}$ by the 7-year entrepreneurship rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effect of MU Funding on Business Outcomes

Panel A

	Sales (1)	Value Added (VA) (2)	Employment (L) (3)	Fixed Assets (K) (4)	EBITDA Margin (5)	Survival (6)
$\tau^{Inc.,MU}$	2.131**	2.149***	1.076***	1.666**	0.532*	0.397*
	(0.692)	(0.717)	(0.348)	(0.652)	(0.230)	(0.166)
First Stage	0.284	0.269	0.291	0.284	0.282	0.299
First-stage z	3.7	3.6	4.1	3.8	3.6	4.0
Bandwidth	1.594	1.684	1.876	1.766	1.439	1.625
Eff. Observations	2,126	2,056	2,528	2,331	1,933	2,204

Panel B

	VA/L (1)	VA/K (2)	TFPR (3)	TFPQ (4)	Initial Equity (5)	Initial Debt (6)
$\tau^{Inc.,MU}$	0.973**	0.505	0.820*	1.418**	1.186**	0.921*
	(0.409)	(0.645)	(0.419)	(0.606)	(0.439)	(0.484)
First Stage	0.268	0.266	0.270	0.269	0.292	0.287
First-stage z	3.4	3.4	3.4	3.5	4.4	4.0
Bandwidth	1.595	1.612	1.552	1.636	1.937	1.802
Eff. Observations	1,931	1,928	1,850	1,954	2,571	2,376

This table reports RD estimates of the effect of log total UI benefits on business outcomes for firms started through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. We restrict the sample to inframarginal entrants using the selection correction procedure of Chodorow-Reich et al. (2024). In columns 1-4 of both Panels the dependent variables are log cumulative outcomes up to age 8. In column 5 of Panel A, EBITDA margin is the ratio of cumulative EBITDA to cumulative sales up to age 8, winsorized at the 5th and 95th percentiles. In column 6 of Panel A, the outcome is survival up to age 8. In columns 5 and 6 of Panel B the outcomes are log equity and log debt at age 1. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Effect of MU Funding on Job Creation and Output

	By Age 1			By Age 8		
	Job-years per €10k	Sales per €1	Value Added per €1	Job-years per €10k	Sales per €1	Value Added per €1
	(1)	(2)	(3)	(4)	(5)	(6)
τ_Y^{MU}	1.508***	4.543**	1.279**	5.313**	21.267	5.422
	(0.362)	(2.083)	(0.601)	(2.271)	(14.745)	(4.686)
First Stage	0.008	79.794	80.247	0.008	79.193	79.481
First-stage z	4.9	3.7	3.7	4.2	4.4	4.1
Bandwidth	1.785	1.005	0.989	1.401	1.462	1.291
Eff. Observations	312,027	175,939	173,589	245,342	255,852	226,158

This table reports RD estimates of the effect of MU funding on cumulative job creation and output from incorporated businesses launched through the MU program, obtained from equation (4). MU funding is instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. In columns 1 and 4, MU funding is expressed in tens of thousands of euros; in the remaining columns, it is expressed in euros. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on MU funding, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Early Failures: Characteristics and Post-Exit Outcomes

	Firm Age at Exit				
	0	1	2	3	4
Fraction of MU entrepreneurs	0.021	0.054	0.048	0.087	0.087
Entrepreneur characteristics					
Age	36.78	38.04	38.34	39.73	38.96
Male	0.555	0.516	0.564	0.577	0.593
Pre-UI Wage (€)	1,139	1,326	1,350	1,532	1,555
Funding Amount (€)	9,274	10,325	11,568	13,297	12,995
Incorporation Rate	0.224	0.323	0.333	0.356	0.383
Repaid MU Funding	0.431	0.270	0.191	0.122	0.069
Monthly Repayment (€)	244.7	289.9	289.1	335.1	391.6
Post-exit outcomes (3 Years)					
Working	0.809	0.807	0.783	0.766	0.722
Within 6 Months	0.550	0.546	0.538	0.491	0.499
Within 1 Year	0.663	0.649	0.616	0.587	0.582
Within 2 Years	0.765	0.749	0.732	0.703	0.673
Received UI	0.256	0.226	0.203	0.252	0.214
Retired or Deceased	0.019	0.016	0.041	0.044	0.053
On Government Support	0.024	0.017	0.014	0.020	0.022
Unobserved	0.148	0.159	0.162	0.170	0.203

This table summarizes baseline characteristics and post-exit outcomes for MU entrepreneurs who exit their businesses at ages 0-4. The first row reports the distribution of exits across ages, expressed as a share of all MU entrepreneurs. “Repaid MU Funding” shows the fraction of entrepreneurs at each exit age who repaid the MU funding received. The average monthly repayment amount assumes a 36-month installment plan. “Working” indicates the share of entrepreneurs who begin a new employment or entrepreneurial spell within three years of exit. “Within X months/years” reports the fraction who return to work within the corresponding time frame. “Retired or deceased” refers to individuals who either file for retirement or die within three years of exit, without having returned to work. “Government support” captures the share who receive unemployment insurance or guaranteed minimum income during this period, again without resuming work. “Unobserved” denotes individuals with no Social Security record in the three years following exit.

Appendix

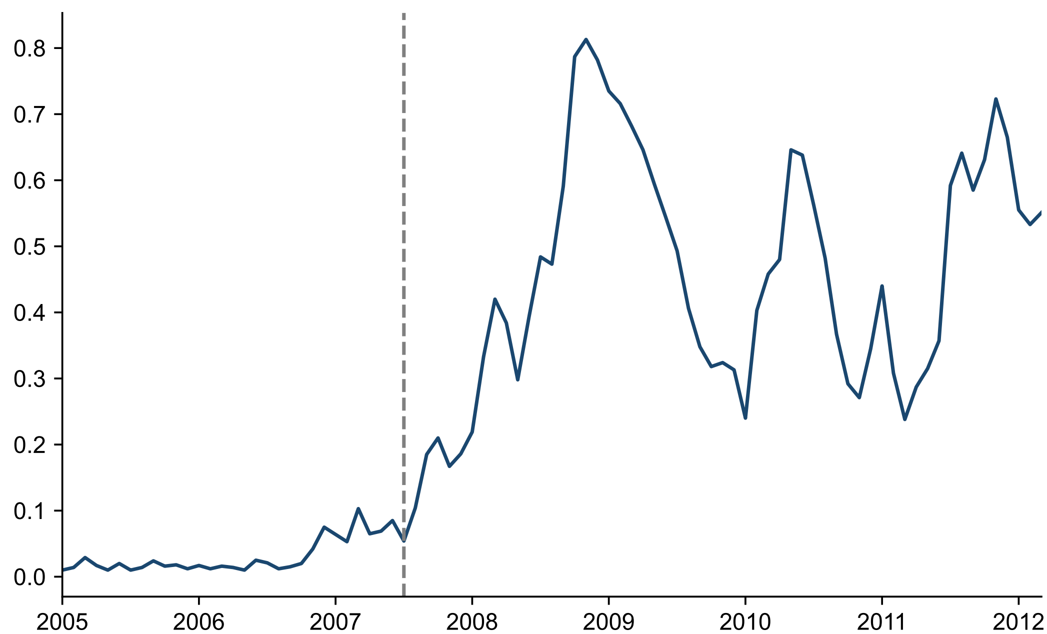
Table A.1: Firm and Job Shares (%)

	Firms	Employment	Gross Job Creation	Sources
United States				
High-Tech	4.61	5.65	6.14	U.S. Census Bureau (2022c,e)
VC-backed	0.16	7.30	—	Puri and Zarutskie (2012)
Patenting	0.54	23.55	18.26	U.S. Census Bureau (2022b)
Sole Proprietorships	71.13	4.47	—	U.S. Census Bureau (2022d,e)
Europe				
High-Tech	6.83	5.71	—	Eurostat (2022b)
High-Tech (Eurostat)	4.37	5.95	—	Eurostat (2022b)
Sole Proprietorships	60.95	8.97	—	Eurostat (2022a)

All figures refer to 2022 except those for VC-backed firms, which are 2001–05 averages. U.S. Census Bureau (2022c) defines high-tech sectors as those with a Science, Technology, Engineering, and Math (STEM) employment share at least five times the national average. These are NAICS 3341 (Computer and Peripherals Manufacturing), 3342 (Communications Equipment Manufacturing), 3344 (Semiconductor and Other Electronics Manufacturing), 3345 (Navigational, Measuring, Electromedical, and Control Instruments Manufacturing), 3364 (Aerospace Manufacturing), 5112 (Software), 5182 (Data Processing, Hosting, and Related Services), 5191 (Other Information Services), 5413 (Architectural, Engineering, and Related Services), 5415 (Computer Systems Design and Related Services), and 5417 (Scientific Research and Development Services). VC-backed firms include those that have ever received VC or acquired VC-financed establishments. We obtain a similar estimate for VC-backed employment (5%) by taking the 2.3 million workers employed in 2019 by all firms that received venture capital and went public between 1995 and 2018, as reported by Lerner and Nanda (2020), and dividing it by total employment in 2019 at firms created between 1994 and 2017, from U.S. Census Bureau (2022a). Patenting firms are those that were granted a patent in years t, $t-1$, or $t-2$ for 2022. The U.S. Census Bureau (2022b) data cover employer firms only. The High-Tech (Eurostat) row uses Eurostat's definition of high-tech sectors, instead of the U.S. Census', which relies on the ratio of R&D expenditure to value added in smanufacturing and the share of college-educated workers in services. This definition includes NACE 21 (Pharmaceuticals manufacturing), 26 (Computer, electronic and optical products manufacturing), 59 to 63 (Motion picture, video, television, and music production; Programming and broadcasting activities; Telecommunications; Computer programming and consultancy; Information service activities), and 72 (Scientific research and development).

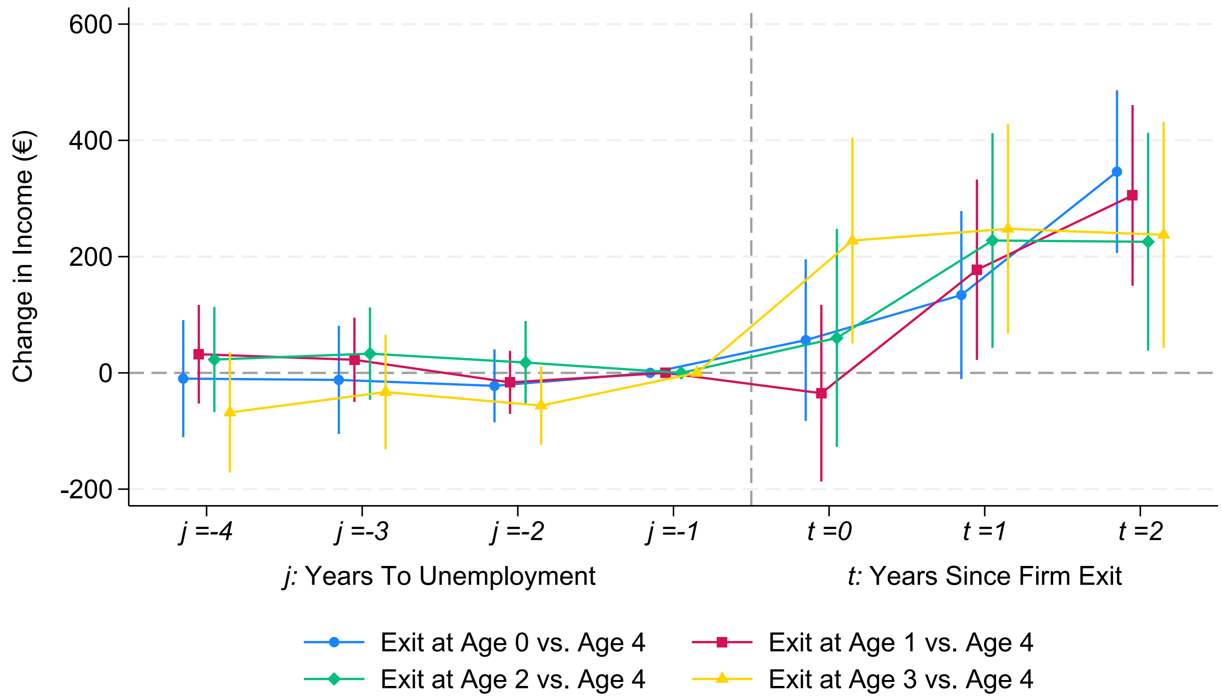
Online Appendix

Figure IA.1: Banco de Portugal's Financial Stress Index (ICSF)



This figure plots the evolution of Banco de Portugal's ICSF index of financial stress during the sample period (Braga et al., 2014). The dashed line corresponds to August 2007, the point we use to divide the sample into low and high stress periods.

Figure IA.2: Income Dynamics for MU Entrepreneurs Who Exit Early and Repay Funding



This figure plots point estimates and 95% confidence intervals for differences in income trajectories between entrepreneurs who exit between ages 0-3 *and* repay their MU funding, and those who exit at age 4. Changes in income are measured relative to the year before unemployment ($j = -1$), and are net of repayment. The area to the left of the dashed line shows differences before unemployment, and the area to the right shows differences following firm exit.

Table IA.1: Summary Statistics for Firms – Benefit Suspension and Exhaustion (Age 8)

	Mean		p50		p99	
	Suspend (1)	Exhaust (2)	Suspend (3)	Exhaust (4)	Suspend (5)	Exhaust (6)
Size						
Sales	381,025	348,200	107,879	109,084	5,096,615	4,998,972
Value Added (VA)	115,030	106,047	38,693	39,253	1,490,767	1,035,378
Wage Bill	81,381	79,128	28,953	29,676	1,204,855	717,380
EBITDA	28,568	20,979	5,170	4,705	390,515	335,816
Employment	4.920	4.364	2.000	2.000	48.000	38.000
Fixed Assets	77,582	92,133	13,885	15,944	1,052,768	1,267,900
Total Assets	245,493	243,155	71,511	77,479	2,856,049	2,904,367
Productivity						
VA/Employment	18,683	18,731	14,992	15,560	101,363	99,134
VA/Fixed Assets	18.358	18.124	3.073	2.680	349.492	375.173
TFPR	1.024	0.952	0.711	0.675	5.759	4.781
TFPQ	0.928	0.872	0.605	0.538	6.609	5.127
Initial Funding						
Equity	14,284	17,867	5,760	5,760	143,793	171,455
Debt	68,291	83,693	31,892	36,457	828,192	844,544
Survival	0.465	0.472	0.000	0.000	1.000	1.000

This table presents summary statistics for incorporated businesses created by UI recipients who suspend ($N = 1,311$) or exhaust ($N = 3,839$) their benefits. All outcomes are measured at age 8 except initial equity and debt, which are measured at age 1. VA is value added and EBITDA is earnings before interest, taxes, depreciation, and amortization.

Table IA.2: Industry Composition – Benefit Suspension and Exhaustion

	Firms		Employment	
	Suspend	Exhaust	Suspend	Exhaust
	(1)	(2)	(3)	(4)
Wholesale and retail trade	0.298	0.276	0.240	0.233
Professional, scientific and technical activities	0.079	0.103	0.046	0.076
Accomodation and food service activities	0.147	0.158	0.132	0.151
Manufacturing	0.110	0.092	0.176	0.175
Administration and support service activities	0.051	0.058	0.091	0.054
Construction	0.092	0.080	0.138	0.119
Information and communication	0.018	0.033	0.013	0.025
Other services	0.043	0.044	0.028	0.039
Human health and social work activities	0.018	0.022	0.012	0.017
Arts, entertainment and recreation	0.016	0.020	0.008	0.011
Real estates activities	0.021	0.024	0.013	0.016
Transportation and storage	0.044	0.035	0.025	0.023
Education	0.019	0.022	0.014	0.017
Agriculture, forestry and fishing	0.024	0.018	0.052	0.033
Water supply, sewerage, waste management	0.005	0.003	0.005	0.002
Mining and quarrying	0.000	0.000	0.000	0.000

This table compares the sector distribution of incorporated businesses created by UI recipients who suspend or exhaust their benefits. Columns 1 and 2 report the fraction of firms created by NACE sector at the section level. Columns 3 and 4 report the corresponding share of employment at entry. Sectors with less than 0.1% of employment in the overall population are not reported.

Table IA.3: Predetermined Covariate Tests

Panel A. All Observations

	Pre-UI Wage (1)	Male (2)	Contribution Months (3)	Years in Last 20 (4)
Discontinuity	34.634**	-0.004	0.915	0.059
	(16.195)	(0.003)	(0.770)	(0.046)
Robust p-value	0.042	0.324	0.170	0.147
Bandwidth	1.403	1.819	1.029	1.047
Eff. Observations	315,680	408,960	231,811	236,182

Panel B. Excluding Observations within 1 Month of the Cutoffs

	Pre-UI Wage (1)	Male (2)	Contribution Months (3)	Years in Last 20 (4)
Discontinuity	22.167	0.001	-0.529	-0.002
	(19.235)	(0.004)	(0.656)	(0.040)
Robust p-value	0.230	0.788	0.556	0.906
Bandwidth	1.445	1.420	1.746	1.758
Eff. Observations	306,685	300,999	374,336	376,765

Panel C. Excluding Observations within 2 Months of the Cutoffs

	Pre-UI Wage (1)	Male (2)	Contribution Months (3)	Years in Last 20 (4)
Discontinuity	13.734	-0.002	-0.993	-0.016
	(20.594)	(0.004)	(0.664)	(0.045)
Robust p-value	0.471	0.667	0.183	0.527
Bandwidth	1.738	1.876	2.009	1.745
Eff. Observations	353,905	384,156	413,998	355,791

This table reports sharp RD tests for discontinuities in pre-determined covariates. The sample pools observations from a 5-year window around each of the age cutoffs that determine jumps in benefit duration (Table 1), and the running variable is the age distance to the cutoffs at the time of dismissal. The covariates are pre-unemployment wages (column 1), an indicator for male recipients (column 2), months of contributions since the last UI spell (column 3) and years of contributions in the previous 20 (column 4). Panel A presents tests for the full sample, while Panels B and C drop observations within 1 and 2 months of the cutoffs, respectively. Each column also reports the robust bias-corrected p-value for the RD estimate, MSE-optimal bandwidth, and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.4: Effect of UI Funding on Entry – Additional Robustness

	With Controls (1)	Two Bandwidths (2)	Quadratic (3)	Cubic (4)	RD Honest (5)
$\tau^{Inc.,MU}$	0.010***	0.009***	0.009***	0.010**	0.010***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)
$\varepsilon^{Inc.,MU}$	0.654	0.630	0.622	0.717	0.681
First Stage	0.273	0.282	0.281	0.288	0.282
First-stage z	85.9	53.3	55.8	42.0	60.5
Bandwidth	2.036	1.593	2.481	2.248	1.095
Eff. Observations	457,578	290,229	556,587	504,550	205,733

This table reports RD estimates of the effect of log total UI benefits on incorporated entry through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Column 1 controls for pre-unemployment wages, gender, months of contributions since the last UI spell and years of contributions in the last 20 years. Column 2 employs separate MSE-optimal bandwidths on either side of the cutoffs. Columns 3 and 4 use quadratic and cubic local polynomials, instead of linear. Column 5 implements the RD Honest estimation method of Armstrong and Kolesár (2018). Estimates of $\varepsilon^{Inc.,MU}$, the elasticity of entry with respect to funding, are obtained from equation (2) by dividing $\tau^{Inc.,MU}$ by the entry rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.5: Heterogeneous Effects on Unincorporated Entry

	Pre-Unemployment Wage					Financial Stress	
	Bottom Tercile	Middle Tercile	Top Tercile	Men	Women	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\tau^{Uninc.,MU}$	0.006	0.006	0.007	0.008	0.006	0.008	0.006
	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)	(0.005)	(0.003)
$\varepsilon^{Uninc.,MU}$	0.238	0.198	0.138	0.215	0.171	0.207	0.185
First Stage	0.268	0.268	0.286	0.279	0.283	0.285	0.277
First-stage z	40.6	37.8	29.9	38.6	42.7	36.6	43.1
Bandwidth	1.512	1.223	1.209	1.422	1.278	1.922	1.100
Eff. Observations	107,654	90,872	96,663	150,699	152,605	118,433	179,793

This table reports RD estimates of the effect of log total UI benefits on unincorporated entry through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Columns 1-3 split the sample by pre-unemployment wage tercile, columns 4 and 5 by gender, and columns 6 and 7 by date of dismissal into a low financial stress period (before August 2007) and a high financial stress period (from August 2007 onwards), using the ICSF index of financial stress from Banco de Portugal (Braga et al., 2014). Estimates of $\varepsilon^{Uninc.,MU}$, the elasticity of entry with respect to funding, are obtained from equation (2) by dividing $\tau^{Uninc.,MU}$ by the entry rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.6: Long-Run Unincorporated Entrepreneurship

	First Post-UI Spell				MU Decomposition	
	MU (1)	Suspend (2)	Exhaust (3)	Non Entrep. (4)	Remain Uninc. (5)	Inc. to Uninc. (6)
$\tau^{Inc.,MU}$	0.003	0.000	0.002	0.005	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.006)	(0.001)	(0.001)
$\varepsilon^{Inc.,MU}$	0.046	0.003	0.030	0.086	0.026	0.020
First Stage	0.282	0.279	0.280	0.277	0.280	0.280
First-stage z	52.4	59.6	58.0	64.6	55.8	58.0
Bandwidth	1.158	1.473	1.401	1.735	1.315	1.429
Eff. Observations	260,823	331,827	315,680	390,858	295,924	321,894

This table reports RD estimates of the effect of log total UI benefits on unincorporated entrepreneurship 7 years after the date of dismissal obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Columns 1-3 present estimates for UI recipients who originally selected into entrepreneurship through the MU program (column 1), benefit suspension (column 2), and benefit exhaustion (column 3), while column 4 reports results for UI recipients who did not select into entrepreneurship in their first post-UI spell. Column 5 presents estimates for MU unincorporated entrepreneurs who remain unincorporated and column 6 for MU incorporated entrepreneurs who transitioned into unincorporated entrepreneurship. Estimates of $\varepsilon^{Uninc.,k}$, the elasticity of entrepreneurship with respect to funding, are obtained from equation (2) by dividing $\tau^{Uninc.,k}$ by the 7-year entrepreneurship rate just below the cutoffs. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.7: Effect of UI Funding on Business Outcomes – 10% Trim

Panel A

	Sales (1)	Value Added (VA) (2)	Employment (L) (3)	Fixed Assets (K) (4)	EBITDA Margin (5)	Survival (6)
$\tau^{Inc.,MU}$	2.098***	1.691***	1.307***	1.565**	0.533**	0.453**
	(0.644)	(0.559)	(0.456)	(0.643)	(0.172)	(0.173)
First Stage	0.299	0.274	0.300	0.304	0.288	0.309
First-stage z	3.9	4.0	3.6	4.0	4.3	4.0
Bandwidth	1.610	2.158	1.257	1.599	2.082	1.610
Eff. Observations	1,976	2,430	1,552	1,949	2,552	1,986

Panel B

	VA/L (1)	VA/K (2)	TFPR (3)	TFPQ (4)	Initial Equity (5)	Initial Debt (6)
$\tau^{Inc.,MU}$	0.773*	0.415	0.731*	1.271**	1.046**	0.766
	(0.366)	(0.608)	(0.388)	(0.555)	(0.429)	(0.475)
First Stage	0.285	0.286	0.290	0.290	0.305	0.306
First-stage z	3.7	3.7	3.7	3.8	4.2	4.0
Bandwidth	1.617	1.631	1.566	1.644	1.774	1.635
Eff. Observations	1,839	1,841	1,756	1,855	2,159	1,980

This table reports RD estimates of the effect of log total UI benefits on business outcomes for firms started through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. We restrict the sample to inframarginal entrants using the selection correction procedure of Chodorow-Reich et al. (2024), and we further drop the bottom 10% of firms on either side of the cutoffs by size, with size measured by cumulative assets up to age 8. In columns 1-4 of both Panels the dependent variables are log cumulative outcomes up to age 8. In column 5 of Panel A, EBITDA margin is the ratio of cumulative EBITDA to cumulative sales up to age 8, winsorized at the 5th and 95th percentiles. In column 6 of Panel A, the outcome is survival up to age 8. In columns 5 and 6 of Panel B the outcomes are log equity and log debt at age 1. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.8: Effect of UI Funding on Business Outcomes – Size Rank at Age 1 + 10% Trim

Panel A

	Sales (1)	Value Added (VA) (2)	Employment (L) (3)	Fixed Assets (K) (4)	EBITDA Margin (5)	Survival (6)
$\tau^{Inc.,MU}$	1.399**	1.364**	0.829**	1.458**	0.288	0.198
	(0.559)	(0.611)	(0.338)	(0.594)	(0.180)	(0.161)
First Stage	0.267	0.246	0.272	0.268	0.267	0.272
First-stage z	3.7	3.3	3.9	3.8	3.4	3.8
Bandwidth	2.024	2.102	2.101	2.118	1.808	1.950
Eff. Observations	2,378	2,272	2,473	2,469	2,125	2,304

Panel B

	VA/L (1)	VA/K (2)	TFPR (3)	TFPQ (4)	Initial Equity (5)	Initial Debt (6)
$\tau^{Inc.,MU}$	0.525	-0.153	0.353	0.776	1.081**	0.814*
	(0.365)	(0.622)	(0.379)	(0.547)	(0.445)	(0.442)
First Stage	0.246	0.247	0.250	0.250	0.281	0.278
First-stage z	3.3	3.3	3.3	3.3	3.9	4.0
Bandwidth	2.041	2.070	2.044	2.052	1.976	2.165
Eff. Observations	2,209	2,222	2,200	2,204	2,319	2,522

This table reports RD estimates of the effect of log total UI benefits on business outcomes for firms started through the MU program obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. We restrict the sample to inframarginal entrants using the selection correction procedure of Chodorow-Reich et al. (2024), and we further drop the bottom 10% of firms on either side of the cutoffs by size, with size measured by total assets at age 1. In columns 1-4 of both Panels the dependent variables are log cumulative outcomes up to age 8. In column 5 of Panel A, EBITDA margin is the ratio of cumulative EBITDA to cumulative sales up to age 8, winsorized at the 5th and 95th percentiles. In column 6 of Panel A, the outcome is survival up to age 8. In columns 5 and 6 of Panel B the outcomes are log equity and log debt at age 1. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.9: Effect of UI Funding on Business Outcomes – No Selection Correction

Panel A

	Sales (1)	Value Added (VA) (2)	Employment (L) (3)	Fixed Assets (K) (4)	EBITDA Margin (5)	Survival (6)
$\tau^{Inc.,MU}$	-0.877	0.086	-0.275	-0.683	-0.295	-0.209
	(0.681)	(0.638)	(0.303)	(0.615)	(0.251)	(0.165)
First Stage	0.268	0.243	0.269	0.271	0.269	0.284
First-stage z	3.8	3.4	4.1	4.0	3.7	4.1
Bandwidth	1.481	1.732	1.856	1.626	1.319	1.487
Eff. Observations	2,362	2,400	2,987	2,549	2,121	2,430

Panel B

	VA/L (1)	VA/K (2)	TFPR (3)	TFPQ (4)	Initial Equity (5)	Initial Debt (6)
$\tau^{Inc.,MU}$	-0.109	0.173	-0.011	-0.064	0.651	-0.599
	(0.440)	(0.711)	(0.446)	(0.642)	(0.487)	(0.485)
First Stage	0.244	0.248	0.252	0.252	0.276	0.279
First-stage z	3.3	3.4	3.4	3.4	3.8	4.0
Bandwidth	1.566	1.502	1.509	1.523	1.424	1.603
Eff. Observations	2,152	2,035	2,032	2,048	2,217	2,480

This table reports RD estimates of the effect of log total UI benefits on business outcomes for firms started through the MU program obtained from equation (1), without applying the selection correction procedure of Chodorow-Reich et al. (2024). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. In columns 1-4 of both Panels the dependent variables are log cumulative outcomes up to age 8. In column 5 of Panel A, EBITDA margin is the ratio of cumulative EBITDA to cumulative sales up to age 8, winsorized at the 5th and 95th percentiles. In column 6 of Panel A, the outcome is survival up to age 8. In columns 5 and 6 of Panel B the outcomes are log equity and log debt at age 1. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.10: Effect of UI Funding on Business Outcomes – Benefit Suspension

Panel A

	Sales (1)	Value Added (VA) (2)	Employment (L) (3)	Fixed Assets (K) (4)	EBITDA Margin (5)	Survival (6)
$\tau^{Inc.,MU}$	-0.424	-0.307	-0.410	-0.231	0.698	-0.108
	(1.170)	(0.955)	(0.562)	(1.015)	(0.539)	(0.223)
First Stage	0.432	0.376	0.433	0.451	0.382	0.429
First-stage z	2.7	2.3	2.9	3.2	2.2	3.2
Bandwidth	1.341	1.615	1.588	1.770	1.064	1.930
Eff. Observations	385	382	460	440	301	560

Panel B

	VA/L (1)	VA/K (2)	TFPR (3)	TFPQ (4)	Initial Equity (5)	Initial Debt (6)
$\tau^{Inc.,MU}$	0.324	-0.692	0.663	0.698	1.364*	0.340
	(0.523)	(0.977)	(0.579)	(0.766)	(0.826)	(0.779)
First Stage	0.349	0.406	0.433	0.432	0.422	0.419
First-stage z	2.5	2.7	2.5	2.5	2.8	3.1
Bandwidth	2.016	2.023	1.440	1.464	1.608	2.123
Eff. Observations	454	415	299	303	435	537

This table reports RD estimates of the effect of log total UI benefits on business outcomes for firms started by UI recipients who suspend their benefits. Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. In columns 1-4 of both Panels the dependent variables are log cumulative outcomes up to age 8. In column 5 of Panel A, EBITDA margin is the ratio of cumulative EBITDA to cumulative sales up to age 8, winsorized at the 5th and 95th percentiles. In column 6 of Panel A, the outcome is survival up to age 8. In columns 5 and 6 of Panel B the outcomes are log equity and log debt at age 1. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.11: Effect of UI Funding on Business Outcomes – Benefit Exhaustion

Panel A

	Sales (1)	Value Added (VA) (2)	Employment (L) (3)	Fixed Assets (K) (4)	EBITDA Margin (5)	Survival (6)
$\tau^{Inc.,MU}$	-0.149	-0.001	0.149	-0.863	-0.063	0.044
	(0.937)	(0.908)	(0.513)	(1.090)	(0.312)	(0.223)
First Stage	0.224	0.295	0.244	0.258	0.229	0.268
First-stage z	3.0	3.1	3.4	2.8	3.1	3.3
Bandwidth	2.070	1.341	2.141	1.393	1.992	1.531
Eff. Observations	1,688	887	1,757	1,042	1,629	1,293

Panel B

	VA/L (1)	VA/K (2)	TFPR (3)	TFPQ (4)	Initial Equity (5)	Initial Debt (6)
$\tau^{Inc.,MU}$	-0.283	0.189	-0.014	-0.124	0.318	0.368
	(0.523)	(0.821)	(0.519)	(0.775)	(0.676)	(0.731)
First Stage	0.301	0.260	0.290	0.254	0.294	0.248
First-stage z	3.3	3.1	3.1	3.1	3.2	3.1
Bandwidth	1.412	1.899	1.558	1.962	1.353	1.867
Eff. Observations	927	1,180	941	1,214	1,017	1,402

This table reports RD estimates of the effect of log total UI benefits on business outcomes for firms started by UI recipients who exhaust their benefits obtained from equation (1). Log UI benefits are instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. In columns 1-4 of both Panels the dependent variables are log cumulative outcomes up to age 8. In column 5 of Panel A, EBITDA margin is the ratio of cumulative EBITDA to cumulative sales up to age 8, winsorized at the 5th and 95th percentiles. In column 6 of Panel A, the outcome is survival up to age 8. In columns 5 and 6 of Panel B the outcomes are log equity and log debt at age 1. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on log total benefits, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.12: Effect of MU Funding on Job Creation and Output, Imputing Unincorporated

	By Age 1			By Age 8		
	Job-years per €10k	Sales per €1	Value Added per €1	Job-years per €10k	Sales per €1	Value Added per €1
	(1)	(2)	(3)	(4)	(5)	(6)
τ_Y^{MU}	1.355***	3.448**	1.044**	4.450**	16.559	4.055
	(0.248)	(1.374)	(0.388)	(1.462)	(10.569)	(2.975)
First Stage	0.012	120.942	120.438	0.012	119.804	118.627
First-stage z	6.3	5.0	5.1	5.7	5.3	5.6
Bandwidth	1.923	1.085	1.135	1.615	1.286	1.506
Eff. Observations	335,668	189,782	198,928	282,765	225,204	263,625

This table reports RD estimates of the effect of MU funding on cumulative job creation and output from businesses launched through the MU program, obtained from equation (4). MU funding is instrumented with the age cutoffs that determine jumps in benefit duration (Table 1). The sample pools observations within a 5-year window around each cutoff, and the running variable is age distance to the relevant cutoff at the time of dismissal. Outcomes for unincorporated entrepreneurs prior to exit are imputed using national averages for unincorporated businesses, as reported by INE (2024). In columns 1 and 4, MU funding is expressed in tens of thousands of euros; in the remaining columns, it is expressed in euros. Each column also reports first-stage estimates of the effect of jumps in benefit duration at the age cutoffs on MU funding, the z-statistic for the first-stage coefficient, the MSE-optimal bandwidth and the corresponding effective number of observations. Standard errors are clustered at the UI recipient level. Significance levels based on robust bias-corrected p-values: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.