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From employee to entrepreneur: The role of unemployment risk ☆

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ABSTRACT

We use Swedish administrative data to study the role of unemployment risk in salaried employees' decisions to become entrepreneurs. Using the 2001 relaxation of Sweden's last-in-first-out (LIFO) dismissal rule as an exogenous shock to unemployment risk, we find that employees facing increased unemployment risk are more likely to become entrepreneurs. The effect is more pronounced for employees with longer tenure, as they were newly exposed to greater unemployment risk. When we track entrepreneurs' income dynamics and the performance of their ventures, we find that entrepreneurs who used to face greater unemployment risk do not underperform compared to other entrepreneurs. Our results provide some of the first empirical evidence of how employees respond to increased unemployment risk.

1. Introduction

Although entrepreneurs can be highly successful, entrepreneurship remains one of the riskiest activities and can result in significant loss of wealth (e.g., Hall and Woodward, 2010). A large body of the literature has focused on *who* becomes an entrepreneur and has presented various explanations. The canonical approach considers an individual's decision between entrepreneurship and employment to be determined by the expected utility offered by these options (Parker, 1996). Consequently, individuals with greater managerial skills will enter into entrepreneurship because the returns from managing a firm exceed the salaries they can earn as employees (Lucas, 1978). As the return to entrepreneurship is inherently riskier than the return to employment, risk-loving individuals will become entrepreneurs, while those who are more risk averse will remain as employees (Hvide and Panos, 2014; Kihlstrom and Laffont, 1979). However, the typical profile of an entrepreneur is a middle-aged individual who has substantial working experience as an employee (Azoulay et al., 2020). A question that arises naturally is what prompts a salaried employee to become an entrepreneur. In this paper, we attempt to answer this question by investigating how nonentrepreneurial employment risk affects the transition to entrepreneurship.

Although unemployment is arguably the biggest risk workers face in their lifetime (Eeckhout and Sepanhsalari, 2024), unemployment risk has largely been overlooked in the literature on the determinants of entrepreneurship. Most models of the decision to become an entrepreneur simply assume salaried employment to be risk free (Parker, 1997; Kihlstrom and Laffont, 1979). However, unemployment risk can lower employees' expected utility from employment and push them toward self-employment. Empirical identification of the impact of unemployment risk is challenging due to measurement issues. Most studies use the ex post unemployment outcome as a proxy, yet unemployment risk corresponds to employees' ex ante likelihood of dismissal. In this paper, we overcome this challenge by exploiting a policy shock that increased

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the ex ante unemployment risk for employees, but only for certain Swedish firms.

The default rule for dismissal, referred to as the "last-in-first-out" (LIFO) principle, is a cornerstone of Sweden's labor protection laws. Simply put, this principle stipulates that the last employee(s) hired by a firm must be the first to be dismissed in case of redundancy. A major reform that relaxed this rigid principle was implemented on January 1, 2001. After this reform, firms with ten or fewer employees were allowed to exempt two workers from being laid off according to the original LIFO principle. This reform took the country by surprise and represented a quasinatural experiment: It was proposed by an unprecedented political coalition and strongly opposed by the ruling party; in late 2000, the reform proposal passed Parliament by a very slim margin. We use this policy shock to examine how unemployment risk affects individuals' decisions to become entrepreneurs. This approach enables us to identify the impact of unemployment risk without requiring explicit measures of individual employees' exposure to unemployment risk.

Our identification strategy is to exploit this exogenous change in labor protection resulting from the LIFO reform to estimate the effects of unemployment risk that are free from endogeneity concerns. Using administrative data, we combine individual-level information with firm-level data to track the dynamics of private businesses and individuals since 1993. The main outcome variable is the decision to transition from salaried employment to incorporated-business (co-)ownership. Our baseline sample includes all non-agricultural private businesses employing 6–14 employees in 1999. The treatment firms have 6 to 10 employees and so were able to exempt two workers following the LIFO reform. Firms with 11 to 14 employees make up the control group. We estimate the likelihood of treatment and control employees transitioning to entrepreneurship annually from 1995 to 2010, observing a distinct shift in this relationship from the pre- to post-LIFO period. The difference becomes notably evident in 2002 and again in 2004, which correspond to the second and fourth years following the LIFO reform's implementation. This evidence strongly supports our empirical design, highlighting a distinct 'kink' in entrepreneurial tendencies across firm sizes when comparing the periods before and after the LIFO reform. Using a cross-sectional setting, we find that a sudden increase in unemployment risk results in a 2.25 percentage point increase in entrepreneurship entry within the first five years following the LIFO reform. This represents an increase of approximately 33% compared to the control group.

An important concern of our identification strategy is that unobserved characteristics among employees from treatment firms (6–10 employees) versus control firms (11–14 employees) may still be correlated with the decision to become entrepreneurs, even after controlling for such individual characteristics as age, gender, income, wealth, marital status, and education. To sharpen the identification, we restrict the treatment firms to those with 10 employees and the control firms to those with 11 employees. The goal is to minimize the differences between the treatment sample and the control sample and to isolate the effects of unemployment risk. Our results confirm that the observed effects on entrepreneurial entry persist even under this more stringent definition.

Moreover, we examine whether the effects are more pronounced for employees with longer tenure. The longer an employee's tenure in a given firm, the greater the protection that employee obtains from the LIFO principle. The 2001 reform shifted unemployment risk from those who were hired late to longer-tenured employees. We find that employees who joined the treatment firms earlier were more likely to become entrepreneurs after the LIFO reform. Overall, our results indicate that a small jolt to employees' job security can greatly stimulate their entry into entrepreneurship.

One might worry that employees of treatment firms rush into starting their businesses without a well-thought-out plan in reaction to the policy shock. Exploiting the longitudinal nature of our administrative data, we examine the long-term dynamics of entrepreneurs' personal income. We find that entrepreneurs who worked for the treatment firms pre-reform and started their own businesses post-reform exhibit similar income and income-growth paths to those of their counterparts from the control firms. Furthermore, we analyze the business performance of the new ventures started by those entrepreneurs who were employees pre-reform. Using various measures including survival rate, value added, sales per employee, and employment size, we find that the firms initiated by entrepreneurs exposed to greater unemployment risk do not underperform compared to their counterparts who were less affected by the LIFO reform. These results are consistent with the hypothesis that the entry decisions made by entrepreneurs exposed to greater unemployment risk are not irrational decisions prompted by a policy shock.

Overall, our study reveals that a rigid employment protection policy is an impediment to entrepreneurship. According to the World Bank in 2015, over 80 other countries had priority rules in case of redundancy similar to Sweden's. Our findings have important implications for designing policies to spur entrepreneurship. For example, on October 1, 2022, the Swedish government further relaxed the LIFO rule by allowing *all* firms, regardless of size, to exempt *three* employees from the LIFO principle.

This paper contributes to three strands of the literature. First, prior studies have documented various outcomes resulting from unemployment risk. Individuals who have been laid off often endure substantial reductions in consumption (Gruber, 1997), long delays before reemployment (Katz and Meyer, 1990), and significant wage cuts after returning to work (Farber, 2005; Gibbons and Katz, 1991). Employees' concerns about becoming unemployed tend to reduce their labor supply (Brown and Matsa, 2016). Some papers study how unemployment rates affect the entry and performance of new entrepreneurs (Kwon and Ruef, 2017). Analyzing a dataset of recent college graduates, Hacamo and Kleiner (2022) find that labor-market declines can lead to not only more firm entries but also better-performing firms. von Greiff (2009) finds that the probability of self-employment is higher after job displacement. However, these papers mostly use some ex post unemployment outcomes as a proxy for unemployment risk; to the best of our knowledge, no prior studies investigate how the *ex ante* risk of being dismissed affects employees' decisions.

Second, economists have long been puzzled by the fact that there is too little entrepreneurship. A large body of literature tries to identify factors impeding entrepreneurship, such as access to capital (Schmalz et al., 2017; Adelino et al., 2015; Bertrand et al., 2007) and entry regulation (Bruhn, 2011; Klapper et al., 2006; Desai et al., 2003; Branstetter et al., 2014). Our findings reveal that a small reduction in job security can bring about long-lasting positive changes in entrepreneurship.

Third, our paper adds to the literature on the effect of tail risk on entrepreneurship.[1] Evidence suggests that policies designed to mitigate downside risk can foster entrepreneurship. Recent studies have focused on some specific forms of downside protection and their impact on entrepreneurial activities. For instance, Gottlieb et al. (2021) examine the extension of job-protected maternity leave for female employees who give birth, while Hombert et al. (2020) investigate extending unemployment insurance to unemployed individuals starting a business. Catherine (2022) studies the value of the fallback option to return to paid employment from self-employment and finds that this option significantly influences the transition to entrepreneurship.

While these studies show that unemployment-protection reforms promote entrepreneurship by providing a safety net for employees,

[1] The literature on tail income risk finds that left-tail income risk is cyclical across the earnings distribution. Guvenen et al. (2014) show that long unemployment spells contribute to the rise in tail risk during economic downturns. Cyclical skewness in the tail risk of labor income has been documented in the United States (Guvenen et al., 2014), Germany, Sweden, and France (Busch et al., 2022).

we show that the LIFO reform exposed employees to job insecurity, nudging them toward entrepreneurship. Further, we study not only the individual decision to become an entrepreneur, but also entrepreneurial firms' performance and entrepreneurs' income over time, thus painting a complete picture of entrepreneurial activity.

The rest of this paper proceeds as follows. We describe the reform of the LIFO principle and its institutional background in Section 2. Then, we discuss the data and variables in Section 3. We report the results on the entry into entrepreneurship in Section 4. We analyze entrepreneurs' personal income outcomes in Section 5 and their ventures' business performance in Section 6. Section 7 concludes.

2. Institutional background and the LIFO reform

Sweden provides a great setting to study the relationship between unemployment risk and entrepreneurship. A nation with some of the most stringent employment-protected laws (Botero et al., 2004), Sweden ranks in the bottom half among OECD countries for entrepreneurial activity (OECD, 2017).

Seniority rules exist in various forms in many countries. In Sweden, the seniority rule, formulated as the LIFO principle, is the cornerstone of the Swedish Employment Protection Act, which regulates all firms in Sweden (Skedinger, 2008). The LIFO principle stipulates that in case of redundancies, the employer must follow a priority list based on employees' tenure within the company. According to this principle, the worker(s) with the shortest tenure must be the first to go. A major reform to the LIFO principle came into effect on January 1, 2001, after which firms with 10 or fewer employees could exempt two workers from the priority list by assigning them "key-worker" status. The main intention of the reform was to increase flexibility for small firms to decide who to retain and who to dismiss. The 2001 LIFO reform was initiated by an unusual alliance between the Green Party and right-wing opposition, despite strong opposition from the ruling Social Democratic Party. After a brief inquiry and referral process, the LIFO reform was ultimately passed by a slim margin in October 2000. See Appendix A for a more detailed description of the reform process.

The reform shifted unemployment risk from shorter- to longer-tenured employees. Fig. 1 illustrates the risk of being laid off before and after LIFO reform, as well as how this risk was redistributed. This is demonstrated using an example of a firm with 10 employees that must lay off two workers, a scenario that can be easily adapted to firms of different sizes and numbers of layoffs. Employees are ranked based on their tenure, with the most junior workers on the right. The top section represents the pre-reform scenario, where layoffs followed a strict last-in, first-out policy. As shown, the two most junior workers (Positions 9 and 10, highlighted in red) would be certain to be laid off. The bottom section illustrates the post-reform scenario. After the reform, the risk of being laid off is distributed among the four least-senior workers (Positions 7, 8, 9, and 10), indicated in yellow. Assuming an equal distribution of risk, each of these four employees faces a 50% probability of being laid off. Throughout both periods, several senior employees remain unaffected by the layoffs, and these employees are shown in the green safe zone. Comparing the distribution of unemployment risk before and after the reform, we see a clear decrease in layoff risk for the most junior employees and a corresponding increase for those with slightly longer tenure. This redistribution of unemployment risk from employees with shorter tenure to those with longer tenure persists regardless of variations in the number of layoffs.

The reform indeed affected many workers and firms. The Confederation of Swedish Enterprise (Svenskt Näringsliv) conducted a survey in 2009 on the use of the LIFO rule, with 29% of the 600 firms that had laid off at least one employee the previous year responding. Among these 174 respondents, 32% stated that they had used the key-worker exemption provision (Svenskt Näringsliv [The Confederation of Swedish Enterprise], 2009). Investigating the effects of the reform on firm behavior, von Below and Thoursie (2010) find that both hires and separations increased among the treatment firms (2–10 employees) relative to larger firms (11–15 employees). Bjuggren and Skedinger (2018) find less screening of new hires after the reform, and Butschek and Sauermann (2022) conclude that the reform lowered the hiring quality at the affected firms. The reform also affected the actions of employees. It for example reduced the use of temporary parental leave among treated fathers (Olsson, 2017).

3. Data and variables

To examine the effects of unemployment risk on entrepreneurship, we assemble an employer–employee-matched data set from LISA (Longitudinal Integrated Database for Health Insurance and Labor Market Studies), maintained by Statistics Sweden. LISA contains detailed individual-level data including demographics, employment, and income for the entire adult population in Sweden. The data set also provides comprehensive information about the individuals' workplace including firm-level information such as sales, number of employees, value added, average salary, location, and industry. We merge the LISA data set with data from the Swedish Wealth Register (Förmögenhetsregistret) to obtain information on wealth. Combining these datasets, we construct panel data for all employees aged 25 to 50 for every year between 1993 and 2010 and track their career paths over time.[2] The age range is 25 through 50 to exclude individuals in training or close to retirement from the sample (cf. Hvide and Panos, 2014).

The Swedish tax authorities record citizens' largest sources of income. We classify individuals as entrepreneurs if the majority of their taxable income comes from a nonagricultural, incorporated business that they fully or partially own.[3] An incorporated business refers to a privately owned limited liability stock company. Åstebro and Tåg (2017) and Lindquist et al. (2015) also use Swedish register data and define entrepreneurs in the same way.[4] We focus on the decision to *become* an entrepreneur as opposed to the status of *being* an entrepreneur. Therefore, our analysis concentrates on individuals whose largest source of income, in a given year, is from wage employment, categorizing them as employees for that year. Specifically, our outcome variable, *Entry*, is a dummy variable that equals one if the individual transitions to entrepreneurship in the subsequent year or within a defined future period.

The granularity of the data allows us to control for several individual characteristics shown to affect the decision to become an entrepreneur: age (Azoulay et al., 2020), gender (GEM, 2022; Ardagna and Lusardi, 2010), and marital status (Taniguchi, 2002). *Male* is a dummy variable equal to one if the individual is a man. *Marital status* is a dummy equal to one if the individual is married or in a registered partnership. We use a granular definition of education to ensure that any observed effects on entrepreneurship are not confounded by variations between fields and in level of education. Specifically, we group individuals into *Educational-level-by-major* groups. Educational level is classified as High school or lower level, Undergraduate, or Postgraduate. For individuals holding at least a bachelor's degree, majors are classified into the following four groups: (1) Science, Technology, Engineering, or Mathematics (STEM); (2) Business administration; (3) Law; and (4) Other majors. Because we do not observe the same

[2] The LISA dataset includes data starting from 1990. However, the coverage of the variables varies across the years and incorporated entrepreneurship is first recorded in 1993.

[3] We use an indicator variable in the LISA dataset equal to one if the individual's largest source of taxable income is from a close company that the individual fully or partially owns. In Sweden, a close company is a limited liability company with few owners, often no more than four. The data do not disclose individuals' exact ownership stakes.

[4] Gottlieb et al. (2021) use a similar definition, categorizing individuals as entrepreneurs if they derive at least 50% of total income from self-employment.

Fig. 1. Unemployment risk before and after the LIFO reform.
Notes: This figure illustrates the impact of the LIFO reform on layoff probabilities in a firm with 10 employees. The top section represents the pre-reform scenario, where the two most junior employees (highlighted in red) are certain to be laid off. The bottom section shows the post-reform scenario, where the risk is redistributed among the four least senior employees (highlighted in yellow), reflecting the reform's impact on layoff-risk distribution. The green areas in both sections represent the more senior employees who remain unaffected by layoffs.

major classification for High school or lower levels of education, a total of nine educational-level-by-major groups are formed. Income and personal wealth provide financial and physical capital, facilitating entry into entrepreneurship. *Income* is an individual's total pretax annual income, including labor income as well as capital income (e.g., dividends and return on financial assets). *Wealth* is the market value of an individual's financial and real assets. Finally, it is possible that the reform pushed individuals into entrepreneurship because they had been laid off. To control for this possibility, we include the number of *Unemployment days* equal to the number of days an individual was registered as unemployed in a given year.

4. Entry into entrepreneurship

4.1. Identification strategy

To estimate the effects of unemployment risk on employees, we take advantage of the fact that the 2001 LIFO reform caused a discrete decrease in job security for employees in firms with fewer than 11 employees. As a result, our treatment group includes employees who worked for firms with fewer than 11 employees pre-reform. Based on the discussion in Section 2 and Appendix A, it is reasonable to assume that few expected the proposal from the unusual coalition of political parties to pass until the autumn of 2000.

Our identification strategy hinges on the condition that the treatment status is uncorrelated with other individual and firm characteristics that could drive entrepreneurship. In Panel A of Table 1, we compare individual profiles between the treatment and control employees. All data on individual characteristics are measured in 1999, except for the main outcome variable; Entry during 2001–2005. Year 1999 is the closest year that is arguably free from potential policy effects related to the 2001 reform. The comparison suggests that the employees in the treatment group (firms with 6–10 employees) are somewhat different from their counterparts in the control group (firms with 11–14 employees). The treatment group has fewer males and is slightly less educated. Also, they have lower income and less wealth and experience more unemployment days.[5] The last row indicates that these treatment employees were more likely to become entrepreneurs within the first five years following the LIFO reform, which corresponds to a nonnegligible 26% increase relative to the control group.

In Table B.1, we reduce the range for the treatment and control groups to the tightest possible. Specifically, we assign individuals working in firms with 10 employees as the treatment group and those in firms with 11 employees as the control group. The employees in the treatment group earn marginally less than employees in the control group. Otherwise, the differences in education, age, gender, marital status, wealth, and number of unemployment days are statistically insignificant. From this comparison, we can conclude that the employees in these narrowly defined treatment and control groups are very similar to each other. However, the treated employees are still more likely to become entrepreneurs in the 2001 to 2005 period.

In addition to comparing the employee-level characteristics, we also compare the employer-level characteristics and present the results in Panel B of Tables 1 and B.1. Average salary is similar between treatment and control firms. Not surprisingly, we find that the average total sales and value added are higher in larger firms (i.e., the control group), primarily due to the size effect. However, the difference in labor productivity, as measured by sales per employee, is statistically insignificant.

Table 1
Mean comparison between treatment and control groups.

Panel A: Individual-level characteristics				
Firm size	6–10	11–14	Difference	*t*-stat
Observations	102 190	59 638		
Age	35.932	35.899	0.033	0.86
Male	0.649	0.657	−0.008***	3.25
Married	0.389	0.389	−0.000	0.02
Undergrad	0.213	0.222	−0.009***	4.38
Log Income	12.131	12.176	−0.045***	19.62
Log Wealth	13.197	13.371	−0.173***	4.79
Log Unemployment days	0.802	0.742	0.060***	6.83
Entry during 2001–2005	0.087	0.069	0.018***	13.07

Panel B: Firm-level characteristics				
Firm size	6–10	11–14	Difference	*t*-stat
Observations	16 385	5901		
Avg employee age	37.947	37.549	0.398***	3.99
% of male employees	0.671	0.678	−0.007*	1.66
Log Average salary	12.274	12.270	0.004	0.78
Log Sales	15.843	16.359	−0.516***	39.76
Log Value added	14.818	15.325	−0.506***	57.28
Log Sales per employee	13.832	13.851	0.019	1.49

Note: This table reports the means and mean differences of key individual- and firm-level variables as measured in 1999, two years before the policy reform. The main outcome variable is entry during 2001–2005. Panel A tests the individual differences between treatment and control groups, specifically comparing employees from firms with an employment size of 6–10 to those from firms with 11–14 employees as of 1999. Panel B examines the differences across firms using the same criteria for defining treatment and control groups. *, **, and *** indicate significance of the *t*-test at the 10%, 5%, and 1% levels, respectively.

[5] In this table, education is a dummy variable equal to one if the individual has at least an undergraduate degree and zero otherwise.

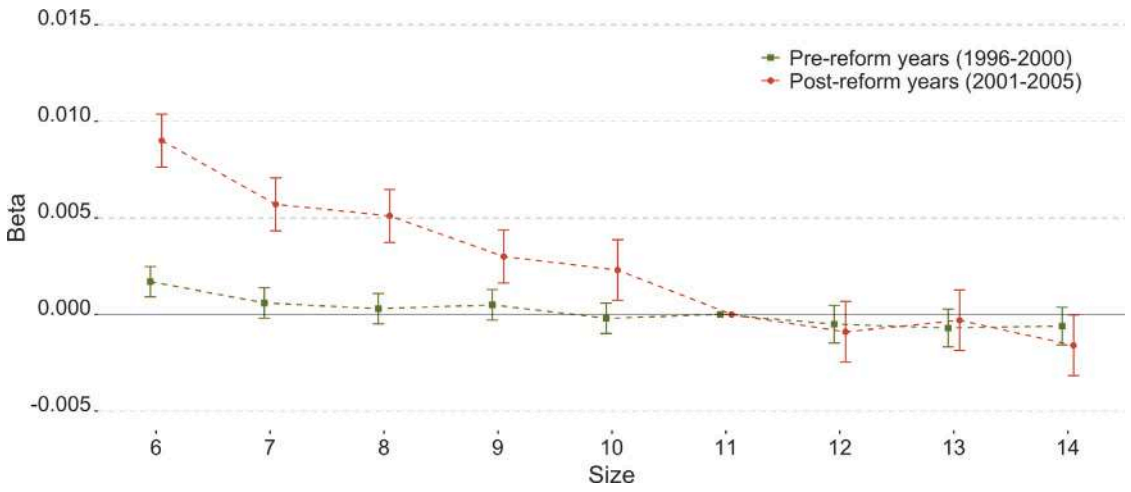

Fig. 2. Entrepreneurship entry and firm employment size.
Notes: This figure presents the estimated coefficients of employer firm size on entry into entrepreneurship. Each point corresponds to a coefficient estimate for a given firm size, with vertical lines indicating the 95% confidence intervals. The coefficients for the pre-reform (pos-treform) years are in green (red).

To further explore how firm size influences an employee's transition from wage employment to entrepreneurship, we initially examine whether this shift is directly correlated with the size of the employer firm. To determine the firm size, we assume that one person in each of the firms holds a managerial position, reducing the firm's effective size by one (cf. von Below and Thoursie, 2010; Bjuggren, 2018).[6] To investigate the impact of firm size on entry effects, specifically among firms with 6 to 14 employees, we estimate the following equation.

$$Entry_{i,s,t} = \alpha + \lambda_{t-2} + \beta_s \sum_{s=6}^{10} Size_{i,s,t-2} + \beta_s \sum_{s=12}^{14} Size_{i,s,t-2} + \gamma' X_{i,t-2} + \epsilon_{i,s,t} \quad (1)$$

where $Entry_{i,s,t}$ is equal to one if employee i, employed at a firm of size s in year $t-2$ chooses to become an entrepreneur in year t. Individuals who are already entrepreneurs in year $t-1$ are not included in the sample. $Size_{i,s,t-2}$ is assigned the value of one if employee i works for a firm with employment size s in year $t-2$. Size is measured as of year $t-2$ instead of year t for two reasons. First, this approach helps mitigate concerns of endogeneity, as firm size could change in response to employees transitioning into entrepreneurship. Second, the two-year gap is the minimum to ensure employees were unaware of the reform. We maintain this two-year gap throughout the remainder of the paper. The coefficient for 11-employee firms, β_{11}, is the benchmark. $X_{i,t-2}$ is a vector of individual characteristics including age, male, married, educational-level-by-major indicators, and the logarithm of total pretax annual income in year $t-2$. It also includes industry-by-county fixed effects to control for industry-specific and county-level regional characteristics. λ_{t-2} denotes year fixed effects that control for broader macroeconomic changes. Standard errors are clustered at the firm level.

We estimate Eq. (1) using a pooled regression spanning a 10-year period. This period includes five pre-reform years (1996–2000) and five post-reform years (2001–2005). For each year t, the treatment status is determined by the employment size in year $t-2$. We perform separate estimations for the periods before and after the reform, and present the coefficient estimates, β_s, for firm employment sizes ranging from 6 to 14 in Fig. 2. For the placebo years (the green line), we document that employment size alone has limited impact on employees' entry into entrepreneurship. For the post-reform years (the red line), we observe a kink around employment size 11, which is precisely the size threshold according to the LIFO-reform mandate. For employees working in firms employing between 11 and 14, the size coefficients are not significantly different from zero, while the coefficients are indeed positive and significant for firm sizes below 11. The figure also illustrates that the probability of entering entrepreneurship increases within the treated group as firm size decreases. This finding is in line with the intuition that employees in smaller firms are more exposed to the reform: The number of employees that can be exempted from the LIFO rule is fixed at two. The results confirm that the differences in transitioning to entrepreneurship are not merely artifacts of firm size.

Another potential concern is that the LIFO reform may have created perverse incentives for firms to evade or embrace the new rule by adjusting the number of employees. We plot the firm size distribution around the year of the reform (from 1999 to 2002) in Fig. C.1. Naturally, the number of firms gradually drops as the firm size increases. Hence, a vast number of employees were exposed to increased unemployment risk as a result of the LIFO reform. When we focus on the number of firms that employ 10 and 11 employees from 1999 to 2002, we do not find any jumps from year to year.

4.2. Event-study results

We proceed to estimate the effects of the LIFO reform on employees in a regression framework. Our primary analysis is carried out using an event-study approach in which we estimate the following linear probability model.

$$Entry_{i,t} = \alpha + \beta Treat_{i,t-2} + \gamma X_{i,t-2} + \epsilon_{i,t} \quad (2)$$

where i denotes the individual employee, t denotes the year of entry, and $t-2$ corresponds to the year of employment-size determination, which also decides treatment status. Therefore, $Entry_{i,t}$ is a dummy variable equal to one if employee i becomes an entrepreneur in year t. Hence, the employee must be an employee in year $t-1$. $Treat_{i,t-2}$ is a dummy variable equal to one if employee i works in a firm that employs fewer than 11 employees in year $t-2$ and zero otherwise. $X_{i,t-2}$ is a vector of individual characteristics including age, male, married, educational-level-by-major indicators, and the logarithm of total pretax annual income. We also control for industry-by-county fixed effects. β is the coefficient of interest, which estimates the treatment effect. Standard errors are clustered at the firm level.

[6] To determine the firm size, the reform proposal stipulates that one should exclude members of the employer's family, workers in management positions, individuals hired to work in the employer's household, and workers in employment-subsidy programs. The data do not allow identification of family links, employees' positions within the firms, or whether the employees have fixed-term or open-ended contracts. The type of contract does not matter: workers with permanent and temporary employment are treated equally.

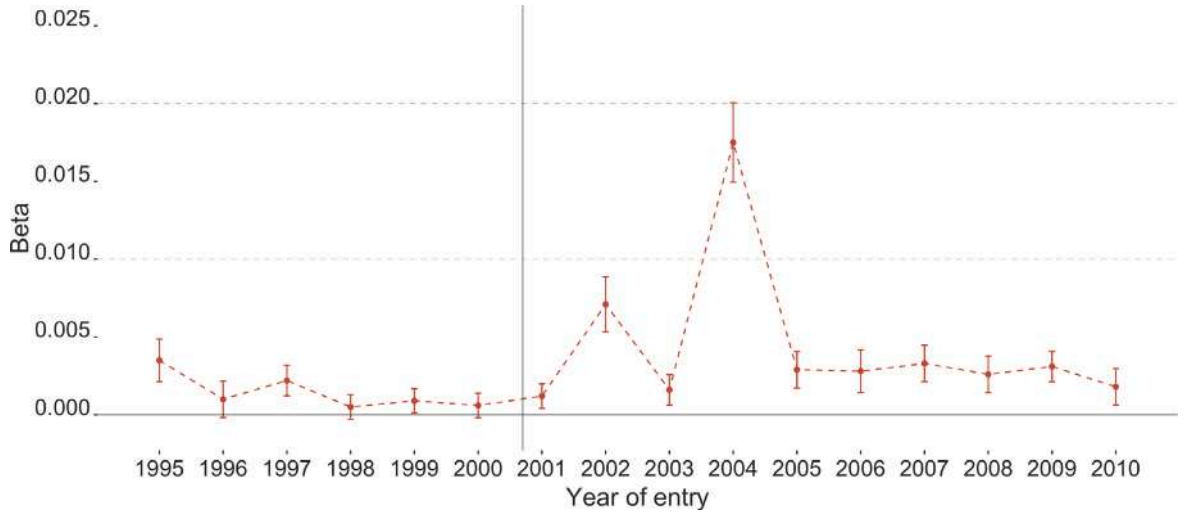

Fig. 3. Differential likelihood that employees in firms with fewer than 11 employees enter entrepreneurship.
Notes: This figure shows the estimated coefficients of treat on entry into entrepreneurship. The year of entry ranges from 1995 to 2010. Each point corresponds to a coefficient estimate for a given year of employment-size determination, assessed two years before entry. This employment size determines treatment status. Vertical red lines indicate the 95% confidence intervals. The gray vertical line indicates the time point when the reform was announced to the public.

Because the reform in 2001 was largely unexpected, we should not observe any treatment effect before 2000, given that the reform was first announced to the public in the later part of 2000 (indicated by the vertical line in Fig. 3). We estimate Eq. (2) for every year between 1995 and 2008 and present the point estimates of β in Fig. 3. The estimates are also reported in Table B.2. The figure supports the notion that the LIFO reform, which increased unemployment risk for many, significantly affected the probability of employees in treated firms entering entrepreneurship. The flat line, with most beta coefficients not statistically different from zero observed for the pre-reform years, indicates no treatment effect before the LIFO reform took place. This lack of difference is important because it suggests that any changes post-reform can be attributed to the reform rather than other underlying trends. In contrast, there are discrete increases in the beta coefficients after 2001, reflecting the impact of the increased unemployment risk on employees entering entrepreneurship. We document a significant impact on entry into entrepreneurship in 2002: Employees at treated firms showed a 0.71 percentage point higher probability of transitioning into entrepreneurship, representing a 46% increase compared to the control group's transition rate, which we estimate to be 1.54%. Furthermore, this effect appears to be sustained rather than transitory; by 2004, the increase in entry probability reached 1.75 percentage points, equivalent to a 49% rise relative to the estimated control benchmark of 3.59% for that year.[7]

The data structure allows us to construct a panel consisting of all employees between 1995 and 2008. We estimate a dynamic difference-in-differences (DiD) model of the specification below.

$$Entry_{i,t} = \alpha + \lambda_{t-2} + \theta Treat_{i,t-2} + \sum_{t=1995}^{2010} \beta_{t-2}(Year_{t-2} * Treat_{i,t-2}) + X_{i,t-2}\gamma + \varepsilon_{i,t} \quad (3)$$

In the above equation, we interact year dummies, $Year_{t-2}$, with the treatment dummy, $Treat_{i,t-2}$, to generate a DiD estimate for every year with 1993 as the benchmark. The coefficient estimates of β_{t-2} are plotted in Fig. C.3. The panel setting allows us to control for various fixed effects: industry by county, firm, and individual. Standard errors are clustered at the firm level. The results are robust and comparable across these fixed-effect specifications. We confirm that employees in the treatment group are more likely to become entrepreneurs. For example, we find that employees who worked for treated firms in 1999 were, depending on the specification, approximately 0.4 percentage point (26%) more likely to become entrepreneurs during year 2002 compared to their counterparts.

4.3. Cross-sectional results

One might worry that employees and firms had already adjusted to the LIFO reform in 2000. For instance, risk-averse employees may prefer to switch to large firms. To enhance the identification of the treatment effect, we focus on the 1999 cross-section alone. This test also allows us to control for additional variables (wealth and number of unemployment days) due to increased data availability from 1999. The evidence from the event study indicates that it takes time for employees to respond to the reform and prepare for the transition into entrepreneurship. In recognition of this delay, we analyze the post-reform entry within a five-year time window. Specifically, we estimate the following model.

$$Entry_i^{01-05} = \alpha + \beta Treat_i^{99} + \gamma X_i^{99} + \varepsilon_i \quad (4)$$

where $Entry_i^{01-05}$ is a dummy variable set to one if employee i transitions to entrepreneurship within the first five years following the LIFO reform, during 2001–2005. $Treat_i^{99}$ is a dummy variable that equals one if employee i works in a firm that employed fewer than 11 employees in 1999 and zero otherwise. X_i^{99} is a vector of individual characteristics equal to those in Eq. (2), but it additionally includes wealth (the market value of an individual's financial and real assets) and number of unemployment days in 1999. As in the event study, we control for industry-by-county fixed effects. β is the coefficient of interest, which estimates the treatment effect of the LIFO reform. Standard errors are clustered at the firm level.

Table 2 reports the results. We document a significant positive coefficient of $Treat_i^{99}$. Employees working in treated firms are 2.25 percentage points more likely to transition into entrepreneurship during the 2001–2005 period. This represents a substantial 33% increase

[7] The results remain when we assign treatment status based on employment size three years before entry year.

Table 2
The effect of the LIFO reform on entrepreneurship.

	Entry during 2001–2005
Treat	0.0225***
	(0.0019)
Log Age	−0.0051
	(0.0036)
Male	0.0345***
	(0.0016)
Married	0.0293***
	(0.0016)
Log Income	0.0610***
	(0.0022)
Log Wealth	0.0029***
	(0.0001)
Log Unemployment days	−0.0079***
	(0.0003)
Observations	161,828
Educational-level ×Major FEs	Yes
Industry ×County FEs	Yes
Adj R^2	0.044

Note: This table reports the regression estimates of entry into entrepreneurship. Individuals are defined as entrepreneurs if the majority of their taxable income is derived from an incorporated business they fully or partially own. The dependent variable equals one if an individual becomes an entrepreneur during the 2001–2005 period. Treat is a dummy variable equal to one if the individual worked in a firm with 10 or fewer employees in 1999. The model includes six control variables measured as of 1999: age, gender, married, income, wealth, and number of unemployment days. Income is an individual's total pretax annual income. Wealth is the market value of an individual's financial and real assets. We control for educational-level-by-major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

compared to their counterparts in firms with 11–14 employees. This finding aligns with those observed in the event-study setup. Calculating the cumulative effect, based on the estimates reported in Table B.2, from the event study for the 2001–2005 period shows a total increase of 3.03 percentage points for treated employees.[8] Furthermore, the cross-sectional estimate implies that our results are stable upon including additional the control variables — wealth and the number of unemployment days — as these variables do not alter the established effect of unemployment risk on entry, suggesting that our main findings are not confounded by these factors.

To further refine our identification, we reestimate Eq. (4) using a subsample that includes only employees from firm of sizes 10 and 11. The results, presented in Table B.3, show a significant treatment effect of 0.64 percentage point, equivalent to a 9% increase relative to the control group. This analysis, resembling a regression-discontinuity design, confirms that increased unemployment risk significantly boosts entrepreneurship.

Turning to the control variables, the results indicate that male employees, married employees, and employees with higher income and wealth are more likely to become entrepreneurs. These findings are consistent with previous studies (e.g., Ardagna and Lusardi, 2010; Borjas, 1986; Glenn, 2004; Lerner and Schoar, 2010; Lucas, 1978; Hvide and Panos, 2014). We also find that the length of the unemployment spell is negatively associated with an individual's decision to pursue entrepreneurship.

[8] The cumulative effect differs conceptually from the impact on the probability of entry over the five-year period. For example, a repeat entrepreneur who transitions twice, once in 2002 and again in 2004, will contribute to the effect in both years. However, when evaluating entry during the 2001–2005 period, these two transitions will be treated as a single event.

For brevity, we do not report the coefficients of education-level-by-major fixed effects in Table 2. We do however observe that individuals with a STEM undergraduate degree are more likely to become entrepreneurs. It can indeed be argued that STEM graduates are particularly well-suited for entrepreneurship due to their training in problem-solving and technical skills, competencies that are central to successful entrepreneurship (cf. Hacamo and Kleiner, 2022). In Table B.4, we interact a STEM undergraduate degree dummy with $Treat_i^{99}$ and find a positive and significant coefficient for the interaction term in the baseline sample. The coefficient remains positive, although insignificant, for the subsample consisting of firms with the employment size of 10–11. Our results reveal that treated employees who pursue STEM majors in their undergraduate studies are especially likely to become entrepreneurs compared to treated employees who pursue other majors.

We furthermore investigate the possibility that the treatment effect is driven by two salient subsamples. Sweden witnessed a thriving information and communication technology (ICT) sector before the dot-com bubble burst in 2001, which coincided with the introduction of the LIFO reform. It is possible that the increasing number of layoffs in small ICT companies coupled with a lack of similar employment opportunities compelled individuals in this sector to become self-employed.[9] A second subsample that might have driven our results consists of employees in the Stockholm area. There are more business opportunities in Stockholm than in the rest of the country, making the transition from employment to entrepreneurship relatively more compelling. In addition, many of the ICT companies that went bankrupt around 2001 were located in Stockholm. Table B.5 reports the results from estimating Eq. (4) in the four subsamples using entry during 2001–2005 as the dependent variable. As shown in the first row, all treatment effects remain statistically significant and positive, except in the ICT-only subsample, which is positive but not statistically significant. Overall, these results suggest that our findings are not influenced by any specific industry or geographic location.

4.4. The effects of tenure

The LIFO reform shifted the unemployment risk from the most junior employees to employees with longer tenures within their firms, as illustrated in Fig. 1. If the increase in entrepreneurship for the treated employees was due to increased unemployment risk, we should expect the effects to be stronger for employees with longer tenures. In this section, we test this hypothesis.

Based on the employment records dating back from 1993, we construct a variable, $Juniority_i$, which equals the fraction of employees who joined the firm before employee i. Hence, a higher value of $Juniority$ corresponds to a shorter tenure within a firm. The data only identify the year, not the exact date when an individual joined a particular firm. In the case of multiple individuals joining a firm in the same year, we assume that the individual with the highest income joined the firm earlier that year as a tie-breaking rule.

Using the tenure measures, we augment Eq. (4) by interacting $Juniority$ in 1999 with $Treat_i^{99}$. The results from the baseline sample are reported in Table 3. The main effect of unemployment risk, as indicated by the coefficients for $Treat$, remains positive and significant. The coefficient of $Juniority$ is negative and significant, suggesting that junior employees are less likely to start their own businesses. This finding remains robust within the most stringent subsample, only firms of sizes 10 and 11, as documented in Table B.6.

To conclude, these results suggest that employees in the treated firms were more likely to become entrepreneurs and these effects were significantly higher for employees who had been with their firm

[9] It is worth noting that in Sweden, at the time of the LIFO reform, approximately 90% of the total labor force had unemployment insurance.

Table 3
The effect of employee tenure on entrepreneurship.

	Entry during 2001–2005
Treat	0.0427***
	(0.0041)
Juniority	−0.1029***
	(0.0049)
Treat ×Juniority	−0.0514***
	(0.0062)
Observations	161,828
Controls	Yes
Educational-level ×Major FEs	Yes
Industry ×County FEs	Yes
Adj R^2	0.062

Note: This table reports the regression estimates of entry into entrepreneurship. Individuals are defined as entrepreneurs if the majority of their taxable income is derived from an incorporated business they fully or partially own. The dependent variable equals one if an individual becomes an entrepreneur during the 2001–2005 perdiod. Treat is a dummy variable that equals one if the individual worked in a firm with 10 or fewer employees in 1999. Juniority is the fraction of employees who joined the firm before the employee. The model includes six control variables measured as of 1999: age, gender, married, income, wealth, and number of unemployment days. We control for educational-level-by-major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 4
Entrepreneurs' income dynamics.

	(1) Inc. growth	(2) Total disc. inc.	(3) Income vol.
Treat	−0.0104*	−0.0144***	−0.0002
	(0.0056)	(0.0022)	(0.0044)
Entry	0.1481***	0.1510***	0.6512***
	(0.0137)	(0.0080)	(0.0177)
Treat ×Entry	−0.0044	−0.0114	−0.0834***
	(0.0173)	(0.0096)	(0.0208)
Observations	157,631	159,273	159,326
Controls	Yes	Yes	Yes
Educational-level ×Major FEs	Yes	Yes	Yes
Industry ×County FEs	Yes	Yes	Yes
Adj R^2	0.056	0.429	0.221

Note: This table presents the relationship between individual income and entry into entrepreneurship. The dependent variables in Columns (1)–(3) are (1) income growth rate, defined as the log growth rate of income between 1999 and 2010; (2) total discounted income during the 2000–2010 period with an annual discount rate of 2%; and (3) income volatility, defined as the standard deviation of annual log total income during 2000–2010. Treat is a dummy variable equal to one if the individual worked in a firm with 10 or fewer employees in 1999. Entry is a dummy variable equal to one if the individual became an entrepreneur during the 2001–2005 period. The models include five control variables based on values in 1999: age, gender, married, income, and number of unemployment days. All models also control for educational-level-by-major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are presented in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

for a longer time period. Taken together, the findings indicate that the treatment effect is weaker for more junior employees, which is consistent with our hypothesis that employees who are more affected by the LIFO reform are more likely to pursue entrepreneurship.

5. Entrepreneurs' income dynamics

The previous results reveal that a sudden increase in unemployment risk stimulated more employees to become entrepreneurs within a short period of time. However, their decisions were not necessarily optimal if these individuals rushed into them without the necessary skills or preparation. Such impulsive decisions might lead to detrimental outcomes for the entrepreneurs. To investigate this possibility, we track individuals' long-term income dynamics after the LIFO reform. To maintain consistency with our previous investigation, we use $Entry_i^{01-05}$ to indicate whether individual i, who was an employee in 1999, entered entrepreneurship within the first five years after the LIFO reform, specifically during the 2001–2005 period. We apply the same framework as in Section 4.3, using a cross-section from 1999. We proceed to estimate the following regression.

$$Income_i = \alpha + \beta Treat_i^{99} + \delta Entry_i^{01-05} + \gamma Treat_i^{99} \times Entry_i^{01-05} + \zeta X_i^{99} + \epsilon_i \quad (5)$$

where $Income_i$ denotes one of three income variables: *Income growth rate* is the growth rate of income between 1999 (the base year) and 2010. *Total discounted income*, in log terms, is the total income during the 2000–2010 period discounted at an annual rate of 2% (cf. Hamilton, 2000). *Income volatility* is the standard deviation of annual income during 2000–2010. $Treat_i^{99}$ is a dummy variable equal to one if the individual worked in a treated firm in 1999. X_i^{99} includes the same individual-level controls as in Eq. (4). The interaction term, $Treat_i^{99} \times Entry_i^{01-05}$, is the main variable of interest. A significant coefficient, γ, implies that entering entrepreneurship generates different income dynamics for individuals previously employed at treated firms relative to entrepreneurs previously employed at control firms.

Column (1) of Table 4 reports that, on average, the employees in treated firms do not experience inferior income growth over the examined years. The employees in treated firms experience a 1.04-percentage-point lower growth rate, which is economically negligible over a 10-year period. The positive and significant coefficients for $Entry^{01-05}$ suggest that entrepreneurs enjoy nearly 15 percentage points higher income growth than individuals who remain in wage employment. The coefficient for the interaction term is insignificant, suggesting that entrepreneurs previously employed at treated firms do not, on average, experience a different income trajectory over the years. To further examine this, we evaluate the annual income growth rate for each year during the 2000–2010 period using an event-study approach where the dependent variable is the income growth rate from 1999 to year t. Fig. 4 plots the treatment-year coefficients for the selected firm-size groups. The results show that there is no significant difference in income growth between entrepreneurs who were employed at treated firms in 1999 and those at control firms. Column (2) of Table 4 shows limited differences in the 11-year total discounted income, which corroborates the findings of similar income patterns when comparing treated and untreated entrepreneurs. Column (3) suggests that, while entrepreneurs generally experienced more pronounced income fluctuations — a result consistent with the inherent volatility of entrepreneurship — this effect is notably less pronounced among those who were previously employed at treated firms. We conduct the same analysis for the subsample consisting of firms with 10–11 employees, presenting similar results in Table B.7 and Fig. C.2, respectively. Taken together, these results suggest that the treated employees who chose to become entrepreneurs enjoyed similar income outcomes to their counterparts in the control firms.

Using administrative data and carefully chosen treatment and control samples, our results also contribute to the debate on whether entrepreneurs earn more than salaried employees. Åstebro and Chen (2014) conclude that entrepreneurs in general earn more, thus challenging earlier studies that usually find that entrepreneurs earn a lower income than waged employees (e.g., Moskowitz and Vissing-Jørgensen, 2002; Hamilton, 2000). Åstebro and Chen (2014) propose that the income gap can best be explained by entrepreneurs underreporting. We overcome this challenge by extracting individual income from census data. Our findings support the conclusions in Hacamo and Kleiner (2022) and show that *forced entrepreneurs* — those who experience large, negative shocks upon entering the labor market — do not necessarily perform worse. Meanwhile, the greater income volatility for average entrepreneurs is consistent with the notion that entrepreneurship is a risky pursuit (e.g., Catherine, 2022).

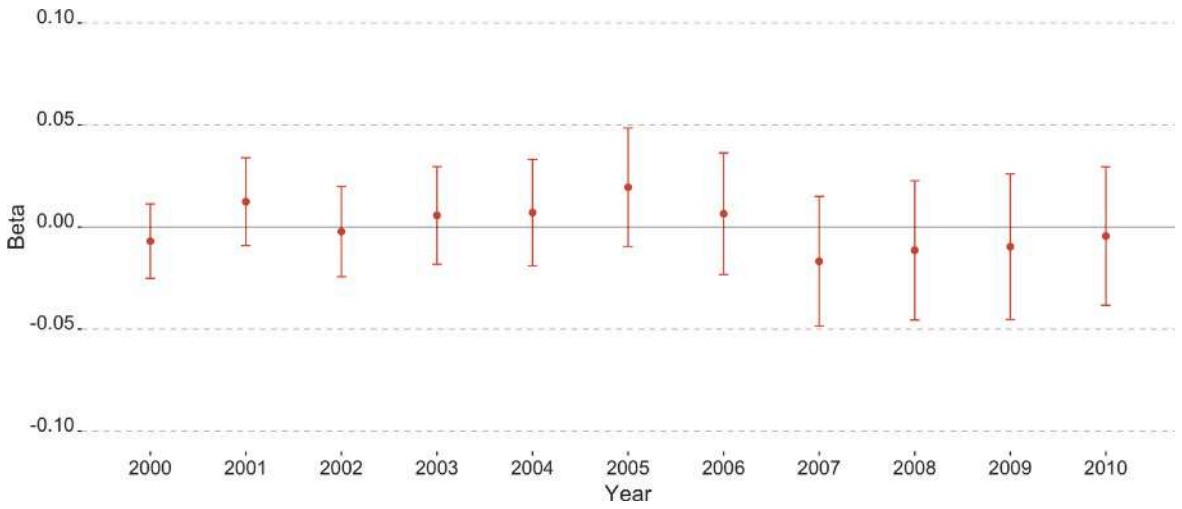

Fig. 4. Dynamics of income growth rate.
Notes: This figure plots the coefficients of treat ×entry on the income growth rate for each year between 2000 and 2010, relative to the level in 1999. Each point corresponds to a coefficient estimate with the vertical lines indicating the 95% confidence intervals.

Table 5
Entrepreneurs' business performance.

	(1) Five-year survival	(2) Log Value added	(3) Log Sales per empl.	(4) Log Num. of empl.
Treat	0.0172	0.0119	−0.0035	0.0152
	(0.0199)	(0.0740)	(0.0528)	(0.0497)
Observations	1,839	1,208	1,208	1,208
Controls	Yes	Yes	Yes	Yes
Educational-level ×Major FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Founding year FEs	Yes	Yes	Yes	Yes
Adj R^2	0.046	0.062	0.106	0.072

Note: This tables reports estimates of entrepreneurs' firm-performance regressions. The samples include all firms founded during the 2002–2005 period that were actively operating in 2005. Moreover, the CEOs of the firms had to be employees in firms with 6–14 employees in 1999. The dependent variables in Columns (1)–(4) are (1) five-year survival, (2) log of value added in 2010, (3) log of sales per employee in 2010, and (4) log of number of employees in 2010. Treat is a dummy variable equal to one if the individual worked in a firm with 10 or fewer employees in 1999. The models include six control variables based on values in 1999: age, gender, married, income, wealth, and number of unemployment days. All specifications also control for educational-level-by-major, industry, county, and founding-year fixed effects. Standard errors are presented in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

6. Performance of entrepreneurial firms

In this section, we turn our attention to the performance of newly established firms founded by waged employees in 1999. Although we show in Section 5 that the entrepreneurs who are directly affected by the LIFO reform enjoy similar income as comparable entrepreneurs, individual incomes do not necessarily translate to business gains. To this end, we analyze firms founded between 2002 and 2005 that were still operating as of 2005, determining whether their CEOs were previously employees in treatment or control firms in 1999.[10] To ensure meaningful comparisons, we restrict the sample to those firms whose founders were employees in our baseline sample in 1999. Specifically, we estimate the following equation.

$$FirmPerf_i = \alpha + \beta Treat_i^{99} + \delta X_i^{99} + \varepsilon_i \quad (6)$$

where $FirmPerf_i$ denotes one of three performance measures: log(*Value added*), log(*Sales per employee*) or log(*Number of employees*), all of which are measured using the 2010 value to capture long-term performance. In addition, we examine the five-year survival rate of those newly established firms. The dummy variable *Five-year survival* equals one if the firm was still active five years after firm registration. $Treat_i^{99}$ is a dummy variable equal to one if the 2005 CEO of firm i worked in a treatment firm in 1999. X_i^{99} is a vector of the same control variables used in Eq. (4). We also control for industry, county, and founding-year fixed effects.

From the results presented in Table 5, we conclude that firms founded by the treatment entrepreneurs, who were subject to increased unemployment risk due to the LIFO reform, perform at least as well as the control entrepreneurs. In general, the survival rate appears to be higher for the firms founded by treated employees, although the coefficients are not statistically significant. Furthermore, the results presented in Table B.8, based on the subsample of firms with 10 and 11 employees that define our treatment and control groups, reveal that firms founded by treated employees achieve a 33% higher total value added. However, the 17% higher sales per employee and the 19% higher number of employees, although positive, are not statistically significant.

[10] We chose 2002 as the starting year instead of 2001 because we lack data on the month of establishment, and it is possible that firms registered early in 2001 were unaffected by the policy reform. Nonetheless, the results are not significantly different if we consider the 2001–2005 time window. One and only one CEO was identified for each firm in 2004. See Andersson and Andersson (2009) for more details on the identification.

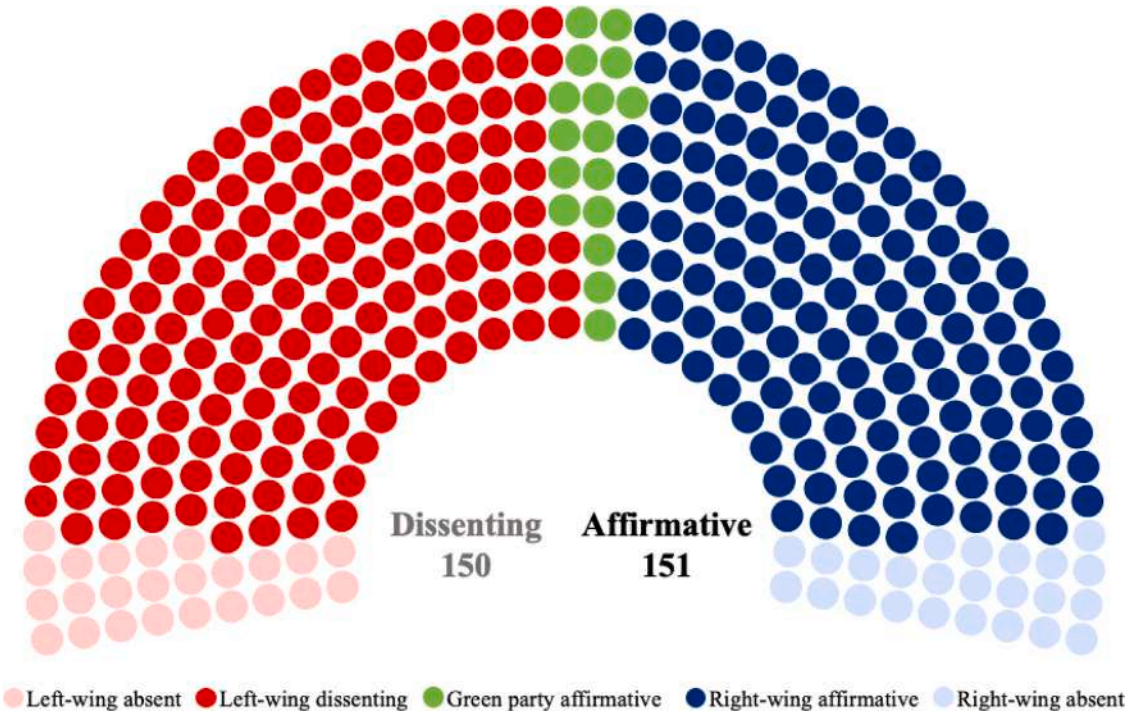

Fig. A.1. Parliament voting results on the LIFO reform.
Notes: This figure shows the distribution of votes on the parliamentary decision regarding LIFO reform on October 11, 2000. The left-wing in the red-color coalition consists of the Swedish Social Democratic Party (131 seats in total, of which 17 were absent and 114 were dissenting votes) and the Left Party (43 seats in total, of which seven were absent and 36 were dissenting votes). The right-wing coalition in blue colors includes the Moderate Party (82 seats in total, of which 11 were absent and 71 were affirmative votes), the Christian Democrats (42 seats in total, of which six were absent and 26 were affirmative votes), the Centre Party (18 seats in total, of which two were absent and 16 were affirmative votes) and the Liberals (17 seats in total, of which five were absent and 12 were affirmative votes). The Green Party in the green color had 16 seats in total and all of their votes were affirmative.

In short, these results are consistent with the findings in Section 5, indicating that the treated entrepreneurs do not underperform compared to their counterparts.

7. Conclusion

In this paper, we offer novel, causal evidence of the effects of unemployment risk on employees' decisions to become entrepreneurs and their entrepreneurial performance. The reform to the LIFO principle in Sweden provides us with quasi-experimental variations in employees' exposure to unemployment risk. We use rich individual- and firm-level administrative data to address the question.

Our results show that greater unemployment risk leads to an increased tendency for employees to become entrepreneurs, and this effect is more pronounced for employees with longer tenures. One might worry that individuals faced with increased unemployment risk were forced into entrepreneurship without sufficient skills and preparation. Our findings however indicate that the entrepreneurs who were exposed to greater unemployment risk did not underperform relative to otherwise similar entrepreneurs. Policymakers have long been searching for ideas to stimulate entrepreneurship. Our findings indicate that in a country with strong labor protection and generous unemployment insurance coverage, even a slight drop in job security can spur significant and productive entry into entrepreneurship.

CRediT authorship contribution statement

Ai Jun Hou: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Sara Jonsson:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Xiaoyang Li:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Qinglin Ouyang:** Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Software, Validation, Visualization, Writing – original draft, Writing – review and editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. The LIFO reform

The 2001 LIFO reform shocked the country (Lindbeck et al., 2006). First, the reform was proposed by an unusual coalition between the Green Party and the right-wing opposition parties. This proposal was strongly opposed by the ruling Social Democratic Party.[11] Second, the inquiry and referral process for this reform were quite short. In April

[11] The formation of the Swedish government is mostly a struggle between two political blocs. The Social Democratic Party and the Left Party form the left-wing Socialist bloc, while the right-wing bloc of the bourgeois parties comprises the Moderate Party, the Christian Democrats, the Centre Party, and the Liberals. After the 1998 general election, the 349 seats in the Parliament were distributed as follows: Social Democratic Party (131), Left Party (43), Moderate Party (82), Christian Democrats (42), Centre Party (18), Liberals (17), and Green Party (16).

Table B.1
Mean comparison between treatment and control groups, firms size 10–11.

Panel A: Individual-level characteristics				
Firm size	10	11	Difference	*t*-stat
Observations	16 974	16 670		
Age	35.781	35.784	−0.003	0.04
Male	0.655	0.662	−0.007	1.36
Married	0.388	0.382	0.006	1.21
Undergrad	0.217	0.215	0.002	0.45
Log Income	12.151	12.173	−0.022***	4.59
Log Wealth	13.207	13.316	−0.109	1.42
Log Unemployment days	0.764	0.749	0.015	0.83
Entry during 2001–2005	0.079	0.073	0.006**	1.98
Panel B: Firm-level characteristics				
Firm size	10	11	Difference	*t*-stat
Observations	2111	1802		
Average employee age	37.523	37.494	0.029	0.14
Male	0.679	0.682	−0.003	0.38
Log Avg salary	12.265	12.269	−0.003	0.32
Log Sales	16.149	16.218	−0.069***	2.64
Log Value added	15.115	15.203	−0.088***	5.13
Log Sales per employee	13.846	13.820	0.026	1.00

Note: This table reports the means and mean differences of key individual- and firm-level variables as measured in 1999, two years before the policy reform for firms sized 10–11. The main outcome variable is entry during 2001–2005. Panel A tests the individual differences between treatment and control groups, specifically comparing employees from firms with an employment size of 10 to those from firms with 11 employees as of 1999. Panel B examines the differences across firms using the same criteria for defining treatment and control groups. *, **, and *** indicate significance of *t*-test at the 10%, 5%, and 1% levels, respectively.

Table B.2
Differential entry likelihood for employees in firms with fewer than 11 employees.

Year of entry	Coefficient	Std. Error
1995	0.0035***	0.0007
1996	0.0010*	0.0006
1997	0.0022***	0.0005
1998	0.0005	0.0004
1999	0.0009**	0.0004
2000	0.0006	0.0004
2001	0.0012***	0.0004
2002	0.0071***	0.0009
2003	0.0016***	0.0005
2004	0.0175***	0.0013
2005	0.0029***	0.0006
2006	0.0028***	0.0007
2007	0.0033***	0.0006
2008	0.0026***	0.0006
2009	0.0031***	0.0005
2010	0.0018***	0.0006

Note: This table shows the estimated coefficients of treat on entry into entrepreneurship. The year of entry ranges from 1995 to 2010.

1999, the coalition requested that the Social Democratic government propose a law that would allow firms to exempt two workers from the LIFO rule. The proposal to reform the Swedish job-security legislation was however not publicly discussed until February 2000 when the Ministry of Industry presented a response to the coalition's request. The Ministry of Industry provided two alternatives: (1) All firms should be allowed to exempt two employees from the LIFO rule or (2) Only

Table B.3
The effects of LIFO reform on entrepreneurship, firm size 10–11.

	Entry during 2001–2005
Treat	0.0064**
	(0.0029)
Log Age	−0.0181**
	(0.0076)
Male	0.0281***
	(0.0033)
Married	0.0274***
	(0.0032)
Log Income	0.0697***
	(0.0036)
Log Wealth	0.0028***
	(0.0002)
Log Unemployment days	−0.0064***
	(0.0009)
Constant	−0.7737***
	(0.0474)
Observations	33,644
Educational-level ×Major FEs	Yes
Industry ×County FEs	Yes
Adj R^2	0.052

Note: This table reports the regression estimates of entry into entrepreneurship for a subsample of employees from firms of size 10–11. Individuals are defined as entrepreneurs if the majority of their taxable income is derived from an incorporated business they fully or partially own. The dependent variable equals one if an individual becomes an entrepreneur during the 2001–2005 period. Treat is a dummy variable that equals one if the individual worked in a firm with 10 or fewer employees in 1999. The model includes the following control variables measured as of 1999: age, gender, married, income, wealth, and number of unemployment days. Income is an individual's total pretax annual income. Wealth is the market value of an individual's financial and real assets. We control for educational-level×major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table B.4
LIFO reform, STEM education and entrepreneurship.

	Entry during 2001–2005	
	(1)	(2)
Firm size	6–14	10-11
Treat	0.0210***	0.0054*
	(0.0018)	(0.0030)
STEM undergrad	0.0137**	0.0267***
	(0.0056)	(0.0075)
Treat ×STEM undergrad	0.0188**	0.0129
	(0.0074)	(0.0104)
Observations	161,828	33,644
Controls	Yes	Yes
Industry ×County FEs	Yes	Yes
Adj R^2	0.046	0.047

Note: This table reports the estimates of entry into entrepreneurship with treat interacted with the educational-level-by-major category STEM Undergraduate. The regressions are performed on two samples with firm size bandwidths of 6–14 (Column 1) and 10–11 (Column 2), determined in year 1999. The models include six control variables measured as of 1999: age, gender, married, income, wealth, and number of unemployment days. We control for industry-by-county fixed effects. Standard errors are clustered at the firm level and are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table B.5
Subsample analysis: LIFO reform and entrepreneurship.

	Entry during 2001–2005			
	(1)	(2)	(3)	(4)
Subsample	Excl ICT	Only ICT	Excl Sthlm	Only Sthlm
Treat	0.0225***	0.0184	0.0208***	0.0304***
	(0.0019)	(0.0149)	(0.0020)	(0.0052)
Observations	158,604	3,224	137,953	23,875
Controls	Yes	Yes	Yes	Yes
Educational-level ×Major FEs	Yes	Yes	Yes	Yes
Industry ×County FEs	Yes	No	Yes	Yes
Adj R^2	0.046	0.044	0.044	0.053

Note: This table reports regression estimates of entry into entrepreneurship for different subsamples depending on whether the individuals worked in the ICT sector and whether they worked in the Stockholm area. All subsamples include individuals employed in firms with 6–14 employees with additional restrictions: Column (1) reports the results for a subsample that excludes employees in the ICT sector, while Column (2) includes only employees in the ICT sector. Column (3) excludes employees in firms in the Stockholm area, whereas Column (4) includes only employees in firms in the Stockholm area. The models include six control variables measured as of 1999: age, gender, married, income, wealth, and number of unemployment days. We control for educational-level-by-major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table B.6
The effects of employee tenure on entrepreneurship, firm size 10–11.

	Entry during 2001–2005
Treat	0.0164***
	(0.0054)
Juniority	−0.1115***
	(0.0077)
Treat ×Juniority	−0.0249**
	(0.0104)
Observations	33,644
Controls	Yes
Educational-level ×Major FEs	Yes
Industry ×County FEs	Yes
Adj R^2	0.060

Note: This table reports the effect of tenure on entry into entrepreneurship, with a subsample of employees from firms of size 10–11. Individuals are defined as entrepreneurs if the majority of their taxable income is derived from an incorporated business they fully or partially own. The dependent variable equals one if an individual becomes an entrepreneur within the period 2001–2005. Treat is a dummy variable that equals one if the individual worked in a firm with 10 or fewer employees in 1999. Juniority is the fraction of employees who joined the firm before the employee. The model includes six control variables measured as of 1999: age, gender, married, income, wealth, and number of unemployment days. We control for educational-level-by-major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table B.7
Entrepreneurs' income dynamics, firm size 10–11.

	(1)	(2)	(3)
	Inc. growth	Total disc. inc.	Income vol.
Treat	0.0004	−0.0024	0.0088
	(0.0117)	(0.0046)	(0.0092)
Entry	0.1257***	0.1417***	0.6361***
	(0.0278)	(0.0141)	(0.0296)
Treat ×Entry	0.0829**	0.0327	−0.0035
	(0.0384)	(0.0199)	(0.0424)
Observations	32,784	33,118	33,131
Controls	Yes	Yes	Yes
Educational-level ×Major FEs	Yes	Yes	Yes
Industry ×County FEs	Yes	Yes	Yes
Adj R^2	0.055	0.430	0.233

Note: This table presents the relationship between individual income and entry into entrepreneurship with a subsample of employees from firms with size 10–11. The dependent variables in Columns (1)–(3) are (1) income growth rate, defined as the log growth rate of income between 1999 and 2010; (2) total discounted income during the 2000–2010 period with an annual discount rate of 2%; and (3) income volatility, defined as the standard deviation of annual log total income during 2000–2010. Treat is a dummy variable equal to one if the individual worked in a firm with 10 or fewer employees in 1999. Entry is a dummy variable equal to one if the individual became an entrepreneur during the 2001–2005 period. The models include five control variables based on values in 1999: age, gender, married, income, and number of unemployment days. All models also control for educational-level-by-major and industry-by-county fixed effects. Standard errors are clustered at the firm level and are presented in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

firms with fewer than 10 employees can exempt two workers. The Social Democratic government chose to propose alternative (1) to the Parliament with the amendment that exempted individuals should be "key workers" of "specific importance" to the firm, and that such specific importance must be assessed by a court of law. The coalition was against this proposition, arguing that small firms would not be able to afford the legal fees, hence in practice small firms would not be able to benefit from the reform. Furthermore, the Green Party would only accept a reform that targeted small firms. In September 2000, Parliament's Labor Market Committee, therefore, developed a new proposal suggesting that firms with fewer than 11 employees could be allowed to exempt two workers. In addition, the employers were given the right to assign key-worker status. Parliament voted in favor of this proposal on October 11, only one month after the bill was presented. Third, the voting outcome of this reform proposal was expected to be very close. Fig. A.1 shows how narrow the margin was: 151 Members of Parliament (the coalition of the Green Party and the right-wing opposition) voted in favor, 150 (the left-wing coalition) voted against, and the rest abstained (Parliamentary Protocol 2000/01:9). Eventually, the proposal was passed, as decisions in Parliament are made by a simple majority.

Table B.8
Entrepreneurs' business performance, firm size 10-11.

	(1) Five-year survival	(2) Log Value added	(3) Log Sales per empl.	(4) Log Num. of empl.
Treat	0.0165	0.3306*	0.1666	0.1921
	(0.0464)	(0.1768)	(0.1049)	(0.1197)
Observations	390	243	243	243
Controls	Yes	Yes	Yes	Yes
Educational-level ×Major FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Founding year FEs	Yes	Yes	Yes	Yes
Adj R^2	0.015	0.059	0.250	0.064

Note: This tables reports estimates of entrepreneurs' firm-performance regressions for a subsample of employer firm sizes 10–11. The samples include all firms founded during the 2002–2005 period and that were actively operating in 2005. Moreover, the CEOs of the firms had to be employees in firms with 10–11 employees in 1999. The dependent variables in Columns (1)-(4) are (1) five-year survival, (2) log of value added in 2010, (3) log of sales per employee in 2010, and (4) log of number of employees in 2010. Treat is a dummy variable equal to one if the individual worked in a firm with 10 or fewer employees in 1999. The models include six control variables based on values in 1999: age, gender, married, income, wealth, and number of unemployment days. All specifications also control for educational-level-by-major, county, industry, and founding-year fixed effects. Standard errors are presented in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

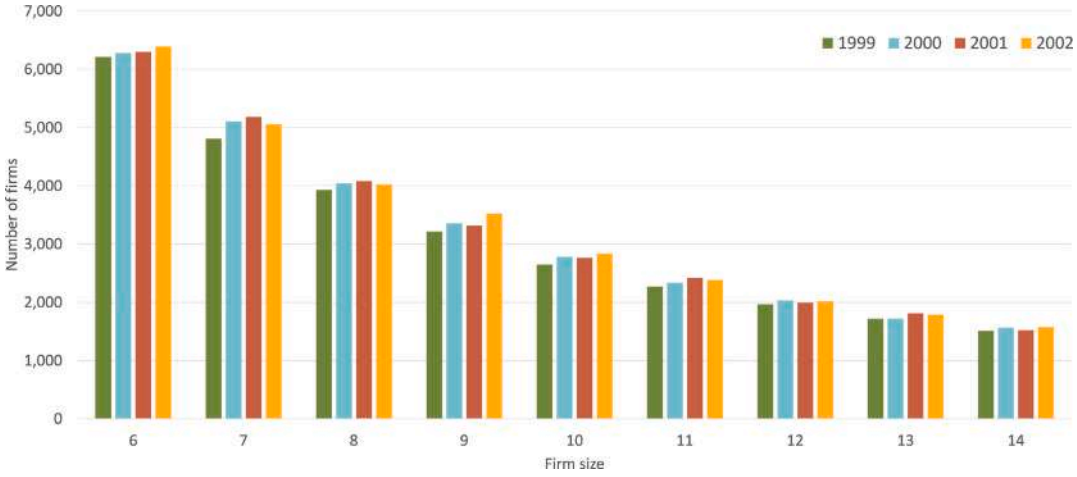

Fig. C.1. Firm distribution before and after the LIFO reform.
Notes: This figure plots the distribution of firm sizes among firms with 6–14 employees by year from 1999 to 2002.

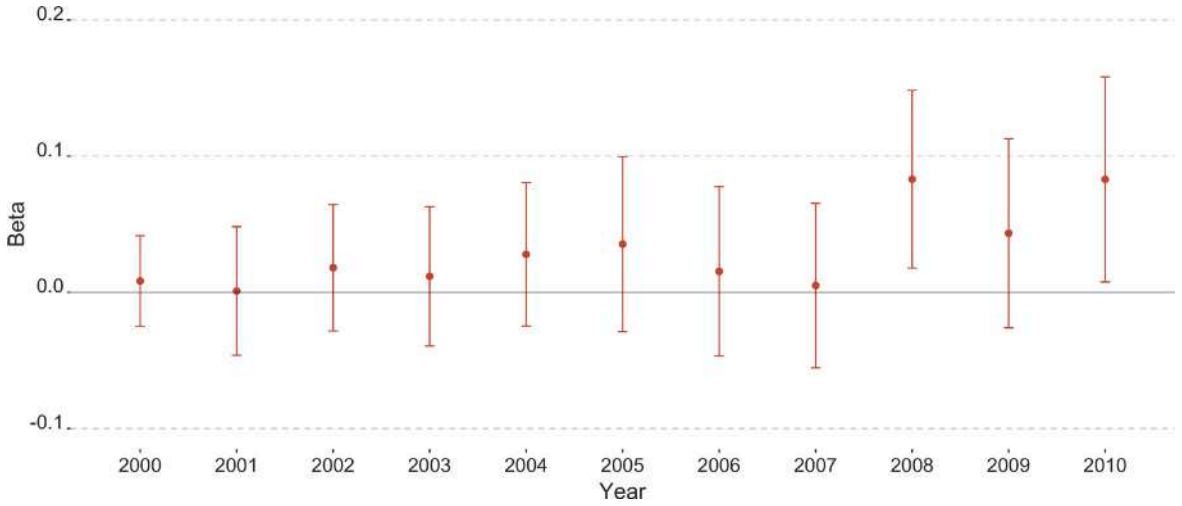

Fig. C.2. Dynamics of income growth rate, firm size 10–11.
Notes: This figure plots the coefficients of treat ×entry on the income growth rate for each year between 2000 and 2010, relative to the level in 1999 for a subsample of employees in firms of size 10–11. Each point corresponds to a coefficient estimate with vertical lines indicating the 95% confidence intervals.

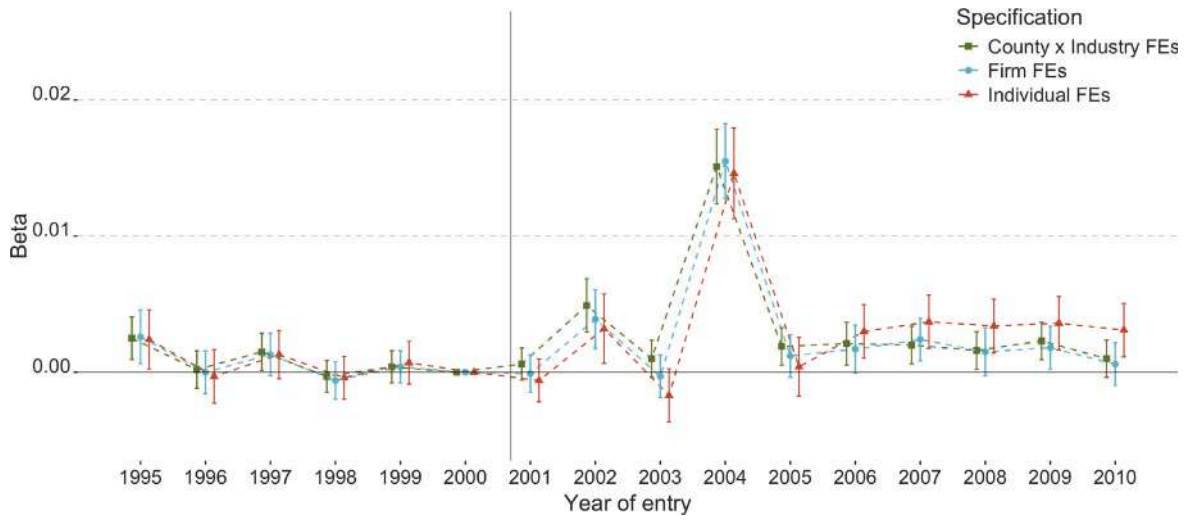

Fig. C.3. Difference-in-Differences analysis: The effect of the LIFO reform on entrepreneurship entry.
Notes: This figure shows the DiD coefficient estimates of year×treat on entry into entrepreneurship from a panel regression for the years 1995–2010. The gray vertical line indicates the time point when the reform was announced to the public. Each point corresponds to a coefficient estimate for a given year of employment-size determination. Year of employment-size determination, which establishes treatment status, is set to two years prior to the year of entry. The vertical green, blue, and red lines indicate the 95% confidence intervals. The colors indicate the use of different fixed effects.

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