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The original sin: firms' dynamics and the life-cycle consequences of economic conditions at birth

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Maurizio Franzini

The original sin: firms' dynamics and the life-cycle consequences of economic conditions at birth

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The original sin: firms' dynamics and the life-cycle consequences of economic conditions at birth*

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October 27, 2021

Abstract

This paper presents new evidence suggesting that the aggregate conditions faced by businesses in the year of birth affect their performance over the entire life cycle. Using a unique employer-employee dataset that covers the universe of Italian businesses over the period 1975-2017, we document that businesses born during recessions start on a larger scale and remain larger compared to businesses born during expansions. These effects persist when we account for: fixed effects at sectoral, provincial and time level; exit attrition; regional and sectoral economic conditions; firm common characteristics; firms' quality; and business formation. We then exploit the reform of the dismissal procedure implemented in Italy in 1990 for gauging the impact of labour market regulation on business creation. We find that a tightening of employment protection widens the employment gap in favour of recessionary startups. The evidence in the paper supports a countercyclical mechanism of selection at entry.

JEL classification: C23, C55, D22, E32, E65

Keywords: firm-level data; firm dynamics; startups; business cycle; panel regression; APC model; Difference-in-difference methods

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Il peccato originale: la dinamica delle imprese e le conseguenze lungo il ciclo vitale delle condizioni economiche alla nascita [1]

Lilia Cavallari
Paolo Naticchioni
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Abstract

Questo lavoro presenta nuova evidenza empirica in supporto della nozione che le condizioni aggregate alla nascita influenzano la performance delle imprese lungo il loro intero arco vitale. Grazie a dati che coprono l'universo delle imprese italiane con almeno un dipendente nel periodo 1975-2017, si documenta che le imprese nate in periodi di recessione sono più grandi e rimangono più grandi rispetto a imprese nate in espansioni cicliche. Tali effetti persistono considerando effetti fissi a livello settoriale, provinciale e temporale; attrito all'uscita; condizioni economiche a livello regionale e settoriale; caratteristiche comuni delle imprese; qualità delle imprese; e creazione di nuove imprese. Il lavoro inoltre sfrutta la riforma della procedura di licenziamento del 1990 per valutare l'impatto della regolazione del mercato del lavoro sulla creazione di nuove imprese. L'inasprimento della protezione del lavoratore allarga il gap occupazionale in favore delle imprese nate in recessione. L'evidenza nel lavoro suggerisce un meccanismo anti-ciclico di selezione all'entrata.

Classificazione JEL: C23, C55, D22, E32, E65

Parole chiave: dati a livello di impresa; dinamica di impresa; startups; ciclo economico; regressioni panel; modelli Age-Period-Cohort (APC); Metodi Difference-in-difference.

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1 Introduction

One important development in the macroeconomic literature in recent years is the emphasis on firms dynamics and its consequences for the propagation of shocks.[2] The process of firms' creation and destruction varies systematically over the business cycle, implying a decline in net business formation and net job creation in recessions.[3] It is by now well-understood that firm entry amplifies business cycle fluctuations, acting like investment at the intensive margin.[4] Because firm dynamics are typically slow, the effects on aggregate variables tend to be persistent. A sharp decline in business formation, as observed in the Great Recession and the COVID pandemic, can slow down the recovery from economic recessions (inter alia, Gourio et al., 2016, Choi, 2018, and Sedlàček, 2020). A prolonged slowdown in the pace of firms' growth and job creation, on the other hand, may lead the economy on a path of secular stagnation.[5] While there is ample consensus on the notion that the timing of business formation is important for the macroeconomy, evidence on the impact of aggregate conditions on the firms' growth trajectory is scant. Exploring these dynamics is the focus of our paper.

In this study we investigate the role of the aggregate conditions faced by businesses in the year of birth for the performance over their entire life cycle. Specifically, we use an age-period-cohort (APC) model to measure the impact of economic conditions at birth on the composition of cohorts, distinguishing the effect of belonging to a certain cohort from the effects of aging (old firms are more resilient to shocks) and current cyclical conditions. Following the intuition of Heckman and Robb (1985) for the identification of APC models, cohort effects are proxied by an indicator of business cycle conditions at birth.

The analysis exploits a unique employer-employee dataset from INPS (Social Security Office of Italy) covering the universe of Italian businesses which have at least one paid employee. The main sample contains all the firms active in Italy between 1975 and 2017 and their outcomes over 10 years of activity: the first cohort is born in 1975 and will be tracked from 1975 to 1984, and the last cohort is born in 2008 and will be tracked from 2008 to 2017. We consider alternate samples that include only businesses that survive for at least 5 or 10 years or over the entire period. For the subsample of businesses born between 2004 and 2017, we complement this information with balance sheet data from CERVED.

We find that firms born in recessionary periods are larger and more productive than firms born in good times and, more importantly, they remain larger and more productive over their entire life cycle. The impact is economically relevant: a 1 percent decline in output below the trend is associated with an increase in the average size of businesses born in that year equal to 1.6 percent while the increase is 2.6 percent after 10 years of activity. We document substantial heterogeneity depending on geographical location, industrial sector, and dimension class. The effect ranges between 0.9 percent for businesses with more than 10 employees operating in the service sector and 2.3 percent for businesses with less than 5 employees located in northern regions. These patterns are robust to adopting different measures of the business cycle, including aggregate, regional or industry-specific measures, and to non-random attrition.

The notion that fewer but better businesses enter the market in recessions is grounded on traditional models of firm dynamics à la Hopenhayn (1992) and Melitz (2003), in which firm heterogeneity derives from differences in productivity. In these models, a larger barrier to entry - as is faced in recessions - implies lower entry rates and higher firm productivity. Evidence that entry costs are countercyclical while entry rates are procyclical is abundant.[6] For the United States, Lee and Mukoyama (2015) show that entry rates in the manufacturing sector are strongly procyclical and that firms entering in recessions are more productive than firms entering in booms.

[2]Early models of firm dynamics, including the seminal contribution of Hopenhayn (1992), focus on firm-specific shocks. Studies that explicitly analyze the aggregate implications of firm dynamics comprise, inter alia, Campbell and Fisher (2004), Lee and Mukoyama (2008, 2015), Clementi and Palazzo (2016), Jaimovich and Floetotto (2008), Cavallari (2013), and Bilbiie et al. (2012).

[3]Ample evidence documents that business formation is pro-cyclical and highly volatile (e.g., Chatterjee and Cooper, 1993, Dunne et al., 1988, Campbell, 1998, Lewis, 2009, and more recently Tian, 2018). The creation of new firms accounts for a significant portion of total job creation and productivity growth in the U.S. economy (see, inter alia, Fort et al., 2013, and Decker et al., 2014).

[4]The role of entry for aggregate shocks has been studied in general equilibrium models with monopolistic competition. One strand has focused on endogenous variation in the diversity of the product space (inter alia., Bilbiie et al., 2012, Bergin and Corsetti, 2008, Lewis and Poilly, 2012, and Cavallari, 2015). A surge in entry leads to the production of more varieties, which in combination with increasing returns encourages agents to work harder and accumulate more capital. Other studies have stressed the pro-competitive effects of entry (Jaimovich and Floetotto, 2008, and Etro and Colciago, 2010 among others). In open economies, see inter alia, Ghironi and Melitz (2005), Bergin and Corsetti (2020) and Cavallari (2013). Limited attention has been devoted to the role of post-entry dynamics (Clementi and Palazzo, 2016 provide a notable exception).

[5]Gourio et al. (2015) show that a "missing generation" of firms affects productivity persistently.

[6]In a large sample of developing and advanced economies, Barseghyan and DiCecio (2011) find that higher entry costs are associated with lower output per capita and lower total factor productivity. Moreover, the price of investment goods - which constitute an important determinant of the overall cost of starting up a new business - is typically low in booms (Fisher, 2006).

A similar pattern holds for firms born in specific crisis periods, like, for example the credit shortage following the Russian default in 1998 in Chile (Ateş and Saffie, 2021), and the East-Asian crisis in Indonesia (Hallward-Driemer and Rijkers, 2013). Unlike these studies, we exploit the INPS database to follow cohorts of firms as they age, and investigate how their later outcomes are affected by aggregate conditions at the time of their birth.

Recent studies have emphasized the importance of demand factors in accounting for firm dynamics. In a context where firm heterogeneity stems from differences in the demand for their products, Sedlàček and Sterk (2017) show that firms born in cyclical downturns may not only be less plentiful, but also weaker in their potential to grow large and create jobs. The reason is that demand shocks affect disproportionately firms which have a high growth potential, like those producing mass goods and devoting a large fraction of expenditures to relaxing their demand constraints. Firms producing niche goods, on the contrary, are both less sensitive to demand shocks and have less potential for expansion. They find evidence that employment created by startups is highly procyclical in the United States and that firms born in recessions tend to remain smaller even when the economy recovers. Moreira (2017) finds a similar pattern for non-farm businesses in the U.S. private sector. She stresses that new businesses enter small because the demand for their products is low compared to the demand faced by incumbents, and this is especially true for businesses born in recessions. Interestingly, she finds that recessionary startups are smaller despite a larger share of high-quality firms enters the market in these periods, pointing to a trade-off between quantity and quality. How the size and productivity of startups changes (endogenously) over the cycle is ultimately an empirical matter.

Our evidence suggests that selection of high-quality firms at the time of entry is important for Italy, and offsets the damages done by low demand in recessions. Compared to the United States, for which the evidence is more nuanced, potential entrants face larger barriers to entry.[7] High costs of setting up a new business induce a selective mechanism whereby only "high-quality" businesses are able to cover the sunk cost and enter the market in the first place. Moreover, institutional and policy factors also contribute to increase entry barriers. A high degree of labour market rigidity, for example, by reducing the extent to which firms adjust labour costs over the cycle, generates uncertainty about the expected profits of a new venture. Only potential entrants which have high prospective returns will find it convenient to enter the market and create new jobs. These effects may become particularly strong under adverse cyclical conditions.

To gauge how a stricter labour market regulation affects business creation, we exploit an important reform of the employment protection legislation implemented in Italy in 1990. The reform provides a natural quasi-experimental setting since it increases dismissal costs for firms which employ less than 15 employees while leaving these costs unchanged for bigger firms. Difference-in-difference regressions show that stricter dismissal rules indeed generate stronger cohort effects, corroborating our narrative that business creation is selective. Businesses born in recessions are larger compared to expansionary startups and the effect is significantly higher for businesses which experience a tightening in employment protection. For these firms, initial conditions at birth have an *additional* effect equal to 3 percent compared to businesses in the control group, which are not affected by the reform.

The paper contributes to different strands of literature. First, it provides rich firm-level evidence on business formation. Extant studies have mainly focused on the cyclical properties of business formation and the patterns of firm demographics, while only few contributions have explored the connection between these two.[8] We are not the first to study business formation in Italy (e.g., Audretsch and Vivarelli, 1996), though we are not aware of studies that explore the relation between aggregate conditions at birth and the performance of firms over their entire life cycle. One contribution of our study is to show that initial aggregate conditions, by shaping the composition of cohorts, can have long-lasting effects on firm performance. Explaining the post-entry dynamics observed in the data and exploring the consequences for aggregate variables constitutes an intriguing challenge for theoretical research in this area.

Second, the paper speaks to studies on business microstructure and aggregate employment. These studies have mainly focused on firm age and size, showing that heterogeneity along these dimensions helps understand the dynamics of aggregate employment. The evidence, though, paints an intricate picture.[9] The importance of

[7] The United States rank in the 5th position for ease of doing business against the 58th position for Italy.

[8] The majority of studies has considered the United States, using the Business Dynamics Statistics (BDS) (e.g., Sedlàček and Sterk, 2017, Lee and Mukoyama, 2015, and Cavallari, 2015) and the National Establishment Time Series (NETS) (e.g., Haltiwanger et al., 2013, and Neumark et al., 2011). In Europe, Moscarini and Postel-Vinay (2012) look at the "cleansing effects" of recessions in Denmark and France. Amici et al. (2016) consider the effects of entry costs in Italy. See also Gschwandtner and Lambson (2002) for a panel of 36 countries.

[9] Moscarini and Postel-Vinay (2012) document that large firms contribute to explain the negative correlation between net job

the firm life cycle for aggregate employment has been emphasized since the influential work of Haltiwanger et al. (2013).[10] Ouimet and Zarutskie (2014) and Davis and Haltiwanger (2014) suggest that the decline in the share of young businesses disproportionately affects the younger and less-educated individuals who are more likely to be hired by these firms. Card et al. (2013) finds that plant-level heterogeneity and rising assortativeness in the assignment of workers to establishments explain a large share of the rise in wage inequality. An important contribution of our work is to document that the cyclical conditions faced by businesses at birth have substantial and persistent effects on firms' employment.[11] This introduces a novel dimension of heterogeneity - recessionary and expansionary startups - which can play a relevant role for labour market dynamics.

The paper is organized as follows. Section 2 discusses the empirical strategy, and Section 3 describes the data. The main evidence is presented in Section 4, while Section 5 provides the results of the DiD experiment, and Section 6 provides a thorough robustness analysis. Section 7 contains the conclusions. All tables are in the Appendix.

2 Empirical approach

Our empirical strategy for analyzing the long-term effects of aggregate conditions at birth exploits variation in employment at the firm level in Italy over 42 years. It draws on a combination of estimation techniques to identify cohort effects and difference-in-difference methods. We start using an age-period-cohort (APC) model to isolate the effect of belonging to a cohort from the distinct influence of age (old firms may be more resilient), and period effects (conditions affecting all firms at a given date). Then, we verify the appropriateness of our empirical specification. Finally, we exploit the reform of the individual dismissal procedure implemented in Italy in 1990 to document stronger cohort effects for firms that face a tightening of employment protection.

Cohort effects capture the impact of the aggregate conditions faced by businesses at the time of their birth and the extent they are linked to firms' outcome late in life. Several confounders might explain the outcomes we observe during a firm's life, making it hard to disentangle the impact of early events. In order to see why, consider a business which is born in a cyclical downturn. One hypothesized effect of the downturn is a negative shift in the (unobserved) quality of businesses that enter the market (*scarring effect*). Depressed demand, tight credit conditions, and high uncertainty might in fact deteriorate prospective profits and constrain firms' expansion plans. Therefore, businesses born in recessionary periods may be smaller and less productive compared to businesses which are born in more favorable conditions. On the other side, recessions imply high mortality rates and low chances of surviving for the weakest incumbents. With fewer businesses of "marginal" quality, we might observe better outcomes later on (*culling effect*).

The tension between selection and changes in the underlying distribution of firms' quality can be analyzed more formally in a stylized latent variable model. Let q_i be the (unobserved) quality of firm i, which is fixed at birth. A higher q_i indicates better quality. If quality falls below a certain threshold, q_0, then incumbent firms will exit the market and potential entrants will not enter in the first place. An active firm is of poor quality if $q_0 < q_i \leq q_1$, where q_1 is a low level of quality.

Given these thresholds, the firm mortality rate (MR) and entry rate (ER) can be defined using the cumulative distribution function $F(q_i)$ as $MR \equiv F(q_0)$ and $ER \equiv 1 - F(q_0)$ respectively. The net business formation rate (NBF) is the difference between entry and mortality rates:

$$NBF \equiv 1 - 2F(q_0)$$

The share of businesses of poor quality (BPQ) is given by the share of businesses that operate in the market and have initial quality below q_1:

creation and the unemployment rate. By contrast, Fort et al. (2013) show that younger and smaller businesses are more sensitive to business cycle shocks. Haltiwanger et al. (2013) find that age is more important than size in explaining employment creation by firms.

[10] Sedlàček (2020) documents that young firms, whose share in total employment is 16 percent, account for about 40 percent of aggregate employment fluctuations. This suggests that a sharp drop in firm entry - as observed in the Great Recession and after the COVID pandemic - may slow down the recovery and affect long run growth by changing the firm age distribution over time. Sedlàček and Sterk (2017) show that by shaping the composition of the cohorts, macroeconomic conditions in the year of birth have long-lasting effects on the aggregate employment fluctuations.

[11] Increasing evidence suggests that initial labour market conditions matter for job performance. Oreopoulos et al. (2012), for example, find that graduates in recessions suffer persistent earnings declines compared to graduates in expansions.

$$BPQ \equiv \frac{F(q_1) - F(q_0)}{1 - F(q_0)}$$

We usually observe a fall in net business formation during recessions. A fall in NBF may be due to a deterioration in the probability distribution of firm quality, $F(q_i)$, and/or to a shift in the threshold q_0. The problem is that we cannot discriminate between these two cases, and they have very different consequences for the outcomes of surviving firms. A deterioration in the probability distribution of quality will increase the share of low-quality firms. Therefore there will be a deterioration in both net business formation and average firms' quality:

$$\frac{\delta NBF}{\delta F(q_i)} < 0 \; and \; \frac{\delta BPQ}{\delta F(q_i)} > 0 \tag{1}$$

When the decline in net business formation - the first term in the expression above - is associated with an increase in the share of firms of poor quality - the second term - the recession has a scarring effect.

A rightward shift in the threshold, on the contrary, will reduce the share of low-quality firms, moving business formation and firm quality in opposite directions:

$$\frac{\delta NBF}{\delta q_0} < 0 \; and \; \frac{\delta BPQ}{\delta q_0} < 0 \tag{2}$$

When the decline in NBF is associated with a reduction in the share of low-quality firms, the average quality of the firms which survive improves as a result of culling of the weakest.

In our analysis, we estimate the impact of recessions on observable firms' outcomes, like size or productivity, without knowing the extent to which the probability distribution of firm quality has changed. We are therefore unable to identify the scarring and the culling effect separately, while we estimate the *net effect*. So, observing a deterioration of firms' outcomes for cohorts born in a recession suggests that the scarring effect is relatively strong, and the damage done by the recession could be even higher than the estimates. On the other hand, observing better outcomes for recessionary startups suggests that selection of high-quality firms is important, and outweighs the eventual damages done by recessions.

2.1 The APC model

The age-period-cohort (APC) model distinguishes the effect of belonging to a certain cohort from the effects of aging and current economic conditions.[12] At the core of APC models is a linear predictor of the form:

$$\mu = \alpha_{age} + \beta_{per} + \gamma_{coh} + \delta \tag{3}$$

which is additively separable in the three time scales, age (α_{age}), period (β_{per}) and cohort (γ_{coh}), each of them is a function of its respective time index. The model (3) has a well-known identification problem in that different values of the time effects on the right hand side result in the same predictor on the left hand side. A range of strategies has been proposed to address the identification problem, with solutions falling in two broad categories. One approach is to identify the three time effects by introducing additional constraints either in the form of non-testable restrictions on the linear parts of the time effects (Hanoch and Honig, 1985) or in terms of invariant, non-linear parts of the time effects (Kuang et al., 2008). A second approach is to reconceptualize the APC model and replace one or all of the time effects with other variables that are "proxies" for the true "latent" variables of interest (Heckman and Robb, 1985). We follow the latter strategy. Specifically, we retain age and period time effects, but replace the cohort fixed effect with an indicator reflecting macroeconomic conditions at the time of birth.[13]

The baseline regression is:

$$\ln Y_{j,ct} = \alpha + \gamma_a + \gamma_t + \beta \ln Z_c + X_{j,t} + u_{j,ct} \tag{4}$$

where $Y_{j,ct}$ is the outcome (employment) of a firm j of age a, which belongs to cohort c, and is observed at time t;

[12] These models are widely used in the contexts of consumption, savings, and labour market dynamics. Moreira (2017) applies APC methods in a study of firm dynamics in the United States.

[13] This gives a sub-model of the APC model which has AP time effects. As such it is a testable restriction on the APC model. Examples of this approach include, among others, Krueger and Pischke (1992); Deaton and Paxson (1994); Browning et al. (2016); and Attanasio (1998).

γ_a and γ_t denote age and period time fixed effects, respectively, Z_c is a measure of business cycle conditions at the time of inception, and $X_{j,t}$ is a matrix of controls. The main coefficient of interest, β, reflects the percent change in the average employment of firms belonging to the same cohort that results from a one percent variation in the indicator Z_c. A positive (negative) value indicates that businesses born during expansions are larger (smaller) than startups born in recessions. All controls are at the firm level and vary over time. In the baseline specification they comprise 3-digit sector of activity and province of location.

In the baseline specification cohort effects are not allowed to vary over the firms' life cycle. Yet initial conditions might subside as a business becomes older. To gauge the importance of variation over the life cycle, the following specification interacts Z_c with age:

$$\ln Y_{j,ct} = \alpha + \gamma_a + \gamma_t + \beta \ln Z_c + \kappa_a \ln Z_c \times Age + X_{j,t} + u_{j,ct} \tag{5}$$

Now the parameter β captures the elasticity relative to business cycle condition in the year of entry (first year of activity) while the parameter κ_a reflects how this elasticity varies with each extra year of age. We also consider a non-parametric specification, in which cohort effects vary non-monotonically with age:

$$\ln Y_{j,ct} = \alpha + \gamma_a + \gamma_t + \sum_{a=1}^{A} \kappa_a \gamma_a \times \ln Z_c + X_{j,t} + u_{j,ct} \tag{6}$$

and the indicator variable γ_a takes the value of one if the business is a years of age. The coefficients κ_a measure the impact of cohort effects for the average firm of a years of age.

2.2 Selection and attrition

Our analysis of aggregate conditions at birth is informed by existing theories of firm dynamics à la Hopenhayn (1992), Hopenhayn and Rogerson (1993) and Melitz (2003). In these models, the decision to create a new business strikes a balance between the cost that must be paid to enter an industry, a portion of which is sunk, and the scrap value which is recouped upon exit. A potential entrepreneur will enter the market whenever the real value of creating a startup, namely the present discounted value of the expected stream of profits, covers the sunk costs. Analogously, an incumbent will exit the market when the real value of the firm falls below the realization value from selling the activity. When production efficiency is heterogeneous across firms, these conditions imply that potential entrepreneurs will actually enter the market if their productivity is above a threshold which depends, among other factors, on entry costs. Existing research has suggested a variety of candidate explanations for differences in firm performance and growth potential, including access to external finance (f.i., Cooley and Quadrini, 2001), firm-specific entry costs (Bergin and Glick, 2007), quality and tastes (Sutton, 1991).[14]

Current business cycle conditions can affect the expected value of a new business, the outside options of entrepreneurs, as well as the realization capital. Hence both the number and characteristics of firms which are at the margin of entry and exit vary systematically over the cycle. While the theory suggests a clear pattern of procyclical entry rates (more firms entering in booms) and countercyclical exit rates (more firms exiting in recessions), the effect on the size and productivity of entering and exiting firms is not always evident *a priori*. Less productive firms are likely to exit in recessions. The decline in realization values in these periods, in fact, reduces the threshold of productivity for which the value of staying falls below the value of exiting, implying that more firms of low quality will exit. Selection at entry, on the contrary, could go either way depending on entry costs and outside options. On the one side, only startups of high quality are able to cover higher entry costs during recessions. On the other side, a recession may make entry attractive even for "necessity entrepreneurs", who start businesses as a means of escaping unemployment (Shoar, 2010). In addition, size and productivity may vary in opposite directions, with smaller but more productive firms entering the market in recessions (Moreira, 2017). How the characteristics of entering firms vary over the business cycle is ultimately an empirical matter.[15]

[14] These include, for example, differences in demand for product quality, markups, fixed costs, and the ability to supply multiple products. See Hottman et al. (2016) for a decomposition of the firm-size distribution into the contributions of costs, quality, markups, and product scope.

[15] For the United States, the evidence is mixed. Lee and Mukuyama, (2015, 2018) show that entering plants in recessions are larger and more productive than entering plants in booms, while there are no significant differences for plants exiting in booms and recessions. Moreira (2017) finds that cohorts born during recessions are smaller but more productive than cohorts born in booms. Sedlàček and Sterk (2017) document that firms born in recessions are smaller and remain smaller even when the economy recovers.

Attrition, selection and cohort effects are deeply intertwined. As we have discussed above, we estimate the net effect of being born in a recession. However, endogenous changes in the number and quality of entering and exiting firms may alter the composition of the sample and bias the estimates. For example, a positive β in our baseline regression would underestimate the damage of being born in a recession as long as recessionary cohorts include fewer firms of "marginal quality" (a positive β would overstate the damage for cohorts born in recessions if more marginal firms enter the market). By the same token, a negative β would understate the benefit of starting up in good periods when weaker and more plentiful firms are able to enter the market and survive during expansions.

We address these concerns by considering samples that include only firms which survive for at least 5 or 10 years so as to limit the scope for exit attrition. In addition, we incorporate factors that could conceivably capture endogenous firm selection in the regressions (4), (5) and (6).

In one experiment, we exploit the idea of Heckman and Robb (1985) of using proxies for one or more of the APC effects, and replace not only the cohort fixed effects (with the business cycle indicator at birth) but also the time period fixed effect in equations (4), (5) and (6) with the number of entries and exits in each year. In this way, period effects - which affect all the firms in the sample in a given period of time - are time-varying and can control for unobserved cyclical variation which might affect the outcome of interest. The logic of the experiment follows the common practice of using fixed effects to reduce selection bias by limiting variation thought to contain confounding factors (Wooldridge 2010). In our case, entry and exits are used to eliminate large portions of between-firm variation over time and produce an estimate of the average effect within firms. Given that entry is procyclical and exit is countercyclical in the data, we expect to observe better firms' outcomes on average in periods, like booms, characterized by more entries or less exits (i.e., we expect a positive coefficient on entry and a negative coefficient on exit). Moreover, we expect to observe a smaller impact of aggregate conditions at birth once period effects eliminate portions of cyclical variation between firms. Observing a (still) negative β would ensure that our results are not an artifact of variation in sample composition. In contrast, a positive β would indicate that the culling of the weakest in recessions is more than offset by the scarring once we net out the effects of business formation.

A further experiment includes additional covariates in the baseline regression (4), (5) and (6) by considering time-varying firm-level indicators of profitability, total factor productivity, and capital endowment. All these measures are positively related to the unobserved quality of the businesses in the sample, thereby alleviating the potential bias from omitted variables. In addition, they vary systematically over the cycle (the return on assets, for example, is procyclical), which helps to account for cyclical variation across firms. We consider firm-level indicators in the year of birth to capture variation in the quality of startups. Countercyclical (procyclical) selection at entry implies that we observe more (less) firms of high quality in recessions. Alternatively, we consider firm-level indicators that vary on a period by period basis to reflect changes in the quality of incumbent firms. Once again, counteryclical (procyclical) selection implies that we observe more (less) firms of high quality in recessions. We are agnostic about the impact of these variables on firms' outcomes (size or productivity) while we focus on their capacity to capture business conditions at the firm level. Consider, for example, profitability. If only few firms experience high profits in a given period of time we might observe a negative impact on firms' outcomes on average (i.e., the coefficient on profitability is negative). In periods in which a plentiful of firms are highly profitable, on the contrary, the average effect is likely to be positive. Useful information for our purposes can be obtained by comparing our coefficient of interest in regressions with and without these additional covariates. The observation of a smaller β in absolute value combined with a positive coefficient on the proxy for quality would indicate that the impact of initial conditions is reduced when the sample contains a plentiful of high-quality firms. This in turn implies less culling and/or more scarring effects (depending on the sign of β). A larger β and a negative coefficient on the proxy would instead indicate a stronger impact of initial conditions after netting out the selection of low-quality firms in the sample. Because of data availability, the experiment is conducted on a sub-sample of firms over the period 2004-2017.

2.3 Labour market regulation

The final piece of evidence investigates the role of labour market institutions for business selection, and is motivated by the desire to dig deeper into the selection mechanism behind the contrasting evidence for Italy and the United States. We claim that a more rigid labour market in Italy contributes to raise entry barriers for potential entrepreneurs and helps explain the relatively large culling of the weakest in recessions. Specifically, we will consider a tightening of the employment protection legislation (EPL) and evaluate its impact across cohorts using Difference-in-Difference (DiD) methods. The idea is that a strict EPL, by increasing the costs that potential entrants face for setting up a new venture, may deter entry of low-quality businesses, and determine a high incidence of good firms entering in bad times. We should therefore observe a more negative β for businesses that experience a tightening of EPL.

The experiment that we propose considers the reform of dismissal rules implemented in Italy in 1990. The reform has extended to businesses with less than 15 employees the stricter rules deployed for large firms. Since labour costs constitute an important component of the overall expenses required to start up a small business, we would expect a stronger culling for businesses which are born after 1990 and have less than 15 employees (the "treatment group"). The "control group" includes businesses with more than 15 employees, for which EPL has remained unchanged. Section 5 discusses this experiment.

3 Data description

We use firm-level data from the administrative archives of INPS available in the VisitINPS program. The program allows selected researchers to access richer data on the universe of Italian firms and workers with respect to previous INPS data releases.[16] Data are annual and cover the period from 1974 to 2018. For each firm, there is information on the age of activity, the total number of employees, the Ateco-Nace classification for industries at six digit level,[17] and the geographical location at the province level. For each worker, information concerns age, gender, yearly wage, type of occupation (blue collar versus white collar), type of contract (part-time versus full-time), number of weeks worked (full-time equivalent), and employer.

The study considers all businesses active in Italy between 1975 and 2017 that have at least one paid employee. The unit of analysis is the employer business, which is a business organization consisting of one or more establishments that are under common ownership and control. In the data around 5 percent of businesses have multiple establishments, and these often span multiple geographic areas and industries. In such cases, the geographic location and industry refer to the establishment with the largest number of employees over the period.

Each business is identified as entrant, exiter or continuer. Entrants are defined as businesses that make their first appearance in the universe of employer businesses in a given year.[18] They comprise the creation of brand new businesses as well as the reorganization under a new identity (fiscal code) of businesses which had ceased their activity in the past. Changes in business ownership, like mergers and acquisitions, resulting in the creation of a new business identity are treated as the creation of new firms. Exiters are defined as businesses which cease their activity in a given year. Continuers are all the remaining active businesses.

Balance sheet data come from the CERVED database. This is a longitudinal dataset that covers almost every limited liability company incorporated in Italy and operating in the private sector. Data are annual and span the period from 2004 to 2017. We employ balance sheet data to obtain measures of productivity and performance at the firm level.[19]

3.1 Firm-level outcomes

The study uses the level of employment as a measure of size. INPS defines employment as the number of full and part-time employees on the payroll of each business in each calendar year. We exploit detailed information about wages paid to each employee to construct a measure of employment weighted per effective working time. Therefore, an employee who has received wages for, say, 6 months in a year has a weight of 0.5. For the subset of company businesses we complement this information with measures of productivity and performance.

The baseline sample (Sample 1) tracks the outcomes of all businesses between 1975 and 2017 over a 10-year horizon since birth. Therefore, the first cohort is born in 1975, and the outcomes of these businesses are followed from 1975 to 1984, while the last cohort is born in 2008 and its outcomes are followed until 2017. The sample includes the outcomes of businesses which did not survive for the entire period, so outcomes are actually

[16] For further information on the program see the INPS website http://www.inps.it/nuovoportaleinps/.

[17] Ateco classification (ATtività ECOnomiche) is the industrial classification adopted by ISTAT and represents the Italian version of the NACE industrial classification employed by Eurostat. The current version is Ateco 2007, and corresponds to NACE Rev.2.

[18] The year of entry may not coincide with the official date of creation, since businesses may be inactive for some periods over their life cycle. In all samples, we exclude businesses that have zero employees.

[19] In order to eliminate outliers, we drop the lowest and the highest 0,25% for the following variables: productivity, revenue, value added, tangible assets, intangible assets.

conditional on survival. For robustness purposes, we also consider other sample selection criteria. To assuage concerns about exit attrition, we consider samples that include only businesses which survive for at least 5 or 10 years, and follow their outcomes for up to 5 or 10 years (Samples 3 and 4 respectively). We also consider an unbalanced sample that includes all the businesses born between 1975 and 2017 and their outcomes over the entire period (Sample 5), so that the most long-lived business is of 43 years of age. Finally, we merge administrative and balance sheet data for the subset of company businesses in the period 2004-2017, and follow their outcomes for up to 5 years (Sample 2) since birth.

Table 1 reports summary statistics for all these samples. The baseline sample has around 20.9 millions observations (businesses-years) that span 4,178,650 businesses. The size reduces to 17 and 14 millions observations in the samples that contain only firms surviving for, respectively, 5 and 10 years, reflecting a large number of exits. Notice that the scale of survivors for at least 5 and 10 years is larger compared to businesses in the samples that do not exclude early exits, suggesting that it is mostly small businesses which cease their activity. The sample containing balance sheet information has 860,000 observations for 276,794 company businesses. The average scale of these businesses is 10.3 employees against 6.1 in the baseline sample.

3.2 Business cycle indicators

In our analysis, cohort effects are proxied by the business cycle conditions in the year of entry. The primary business cycle indicator is annual real gross domestic product (GDP). As is standard practice in business cycle studies, (the log of) GDP is detrended using the Hodrick-Prescott (HP) filter with smoothing parameter 6.25. We consider alternate detrending methods, including the band-pass filters of Christiano and Fitzgerald (CF) (2003) and Baxter and King (BK) (1999), and the demeaned log difference. We also consider a non-parametric approach and construct an indicator variable that takes the value of 1 when GDP is above a linear trend and zero otherwise.

In addition, we consider local and sector-specific indicators, regional GDP and value added, respectively. The series of regional GDP come from the regional statistics database of ISTAT (Italian National Institute of Statistics). They cover all Italian regions over the period 1995-2017. The series of value added come from the historical database of the Bank of Italy. They cover agriculture, service, constructions and manufacture over the entire period 1975-2017. All variables are measured in constant prices as chain index with base year 2010. Local and sectoral indicators are HP-filtered for consistency with the aggregate indicator.[20] Table 2 reports summary statistics for all these indicators.

4 Aggregate conditions at birth: results

We estimate the baseline specification of equation (4) using firm-level employment as the dependent variable and the Hodrick and Prescott filtered aggregate real GDP as the proxy for cohort effects. The level of employment is measured by the number of employees weighted per months of work in each year.[21] All OLS regressions include time fixed effects for age and period, together with fixed effect for 3-digit sector of activity and province of location. Errors are corrected for two-way clusters at the business and at the year level.

Table 3 reports the results of this analysis in the baseline sample. The sample contains all the businesses born between 1975 and 2008 and their outcomes up to 10 years, including the outcomes of businesses that did not survive for the entire period. The coefficient on the cohort proxy - β in equation (4) - represents the effect of aggregate conditions at birth for average firm-level employment over a 10-year horizon. The coefficient is negative and statistically significant, implying that businesses born in recessions are larger than businesses born during expansions. The effect is economically relevant: a one percent decline in output below the trend increases the employment level of the firms born in that year by almost 1.6 percent on average.

Taking stock of the fact that initial conditions matter, we investigate how the effect changes with firm age. The role of age for the performance of cohorts is *a priori* ambiguous. On the one side, aggregate conditions at inception may become less important as firms grow older and become more resilient. We may then expect the

[20] Using alternate detrending methods is inconsequential for the analysis.

[21] We have also considered the number of employees on the payroll of each business, independently of the amount of work they effectively provide. Thus, a business whose employees have worked over the whole year results having the same size of a business with the same number of employees each working for, say, 2 months. The results of this analysis are in Table (A3 bis). The measure used for the level of employment is largely inconsequential.

cohort effect to subside with age. On the other side, however, choices made in the startup phase may entrench choices at successive stages, and skills acquired early in a firm life cycle may beget skills later on. This clearly works in the direction of strengthening the impact of initial conditions as firms age. To gauge the relative importance of these effects, we interact the business cycle indicator with the age of the business as in equation (5). In this case, the coefficient on the cohort proxy - β in equation (5) - measures the impact of the cycle in the first year of activity while the coefficient on the interacted term - κ_a in equation (5) - measures the way in which the effect varies with each extra year of age. The combination of a negative β and a positive κ_a suggests that recessionary cohorts start larger but the effect diminishes and eventually disappears with firm age. The combination with a negative κ_a, on the contrary, suggests that initial conditions leave a persisting footprint on firms' outcomes.

Results in column (2) confirm that the business cycle conditions at entry have a negative impact on the level of employment in the first year of activity, with an estimated elasticity of almost 0.9. Interestingly, the effect does not subside with age. The coefficient on the interaction term is in fact significantly negative, implying that the average elasticity increases (in absolute value) with the age of the business. The elasticity increases by 0.2 percent on average for each extra year of age, so that after, say, 10 years a business that was born in a recessionary period will be larger by an extra 2 percent compared to an expansionary startup. These estimates hinge on the implicit assumption that cohort effects change monotonically as businesses get older.

To avoid imposing restrictions on the parameters, we consider a specification where the business cycle indicator is interacted with a set of indicator variables that take the value of 1 when the business is of i years of age as in equation (6).[22] The coefficients in column (3) are all negative and statistically significant, and they show no sign of reversion to zero. A one percent fall of GDP below trend in the year of birth is always associated with an increase in employment, and the effect grows non-monotonically with firms' age. Businesses with 1 year of age are on average 1.4 percent larger when they are born in recessions, while the impact is up to 2.6 percent for businesses of 10 years of age. Overall, this evidence provides support to the notion that the impact of initial conditions cumulates over time, affecting firms' outcomes in a dynamic way.

One possible concern about the persistence of cohort effects is that 10 years is a relatively short horizon for adjusting the employment level of a firm. Businesses born under different cyclical conditions may take longer to dissipate the initial imprinting. To avoid imposing restrictions on the length of the convergence process we have estimated equations (4), (5), and (6) using an unbalanced sample that includes all the businesses born between 1975 and 2017 and their outcomes since the first year of activity (Sample 5). The average age of businesses in this sample is 13 years, from a minimum of 1 year (2.4 percent of businesses) to a maximum of 43 years for the most long-lived businesses (0.03 percent). The findings in Table 4 suggest that recessionary startups are indeed larger than expansionary startups, and the effect is persistent over their entire life cycle. The magnitude of the effect is comparable to what we have found before. In regression (4), for example, the coefficient on the business cycle indicator is 1.8 against 1.6 in the main sample.

Another concern is that businesses surviving for relatively short periods may have a disproportionate effect on the results. In the main sample, almost 44 percent of businesses exit in the first 3 years of activity, and 60 percent do not survive for more than 5 years. The average size of businesses that survive up to 3 years is of 4.2 employees, which is below the average size of businesses surviving for at least 5 or 10 years, 4.5 and 5.1 respectively. Frequent exits may generate non-random attrition and a downward bias in the estimates because of the small size of exiting businesses. To address this potential bias we have estimated equations (4), (5), and (6) using samples that include all firms born between 1975 and 2008 and surviving for at least 5 or 10 years (results in Tables 5 and 6 respectively). The results are similar to what we have found in the main sample as concerns the sign and persistence of the cohort effect. Notice that the exclusion of short-lived businesses, which should in principle overstate the damage of recessions, appears to have the opposite effect. In fact, the longer the survival horizon of the businesses included in the sample the larger the size of recessionary businesses. The elasticity of employment increases marginally for businesses which survive for at least 5 years compared to the main sample: in regression (4), for instance, this is 1.8 against 1.6. The difference becomes economically relevant when we consider businesses surviving for at least 10 years, for which the elasticity is 2.2. Analogous considerations hold for the coefficients in equations (5) and (6), which are larger (in absolute value) compared to the main sample. Excluding frequent exits appears to generate not only stronger but also more persistent effects.

[22] The dummy is constructed starting from the second year of activity, i.e. $i \in (1; 9)$. The coefficient on each interacted coefficient κ_a in (6) measures the impact of initial conditions for businesses of age 2 to 10.

Finally, one important issue concerns the cyclical patterns of businesses formation, since both the number and quality of businesses which enter or exit the market may vary systematically over the cycle. A relatively high proportion of high-quality firms entering in recessions (as suggested by models of firm dynamics) would imply a downward bias in the estimates of cohort effects. The use of a detrended measure of the business cycle as a proxy for aggregate conditions at birth helps alleviate the problem of confounding cohort effects with the cyclical patterns of entry and exit. In fact, entries are weakly correlated with the business cycle indicator (the correlation is 0.135), while exits are almost independent (the correlation with the business cycle indicator is 0.033).[23] Moreover, businesses born in recessions start with 33 percent more workers compared to firms entering in booms, the average number of employees at inception is 2.7 for the former against 2.0 for the latter. These differences are persistent over the firms' life cycle: businesses that exit during recessions are larger than businesses that exit during expansions, and the relative size of these firms at the time of exit is almost identical to their relative size at inception (precisely, 2.7 employees for businesses exiting in recessions against 2.1 in booms).

We address the problem of non-random attrition in business formation by using proxies for the unobservable characteristics of businesses which are on the margin of entry and exit. In the baseline sample, we replace the time period fixed effect in equations (4), (5) and (6) with the number of businesses entering or exiting the market in each year. In this way, the period effect, which is common to all firms in a given year, eliminates large portions of between-firm variation thought to contain potential confounders. Movements in business formation, in fact, capture changes in the composition (and presumably the quality) of the sample which might affect the outcome of interest. The results of this analysis are in Table 7. The introduction of entries and exits does not alter the qualitative patterns that we have found above, confirming the culling of the weakest in recessions. Business formation is indeed important for the size of the average business in the sample. In periods in which exits increase or entries reduce, like recessions, we observe a smaller average employment at the firm level, as is expected. Remarkably, the cohort effect remains negative, persistent and economically relevant, though it is smaller (in absolute value) than before. In the baseline regression, for example, the elasticity to initial conditions is 1.2 against the estimate of 1.6 obtained with time period fixed effects. Later on, we will consider proxies for firm quality, exploiting information on balance sheet data for company businesses. Section 6 provides a thorough robustness analysis along this and other dimensions.

5 The role of institutions: evidence from a quasi-experiment

The finding of a negative impact of aggregate conditions at birth is in contrast with evidence for the United States, showing that businesses start smaller in recessions and remain smaller over their life cycle (Moreira, 2017). Many factors contribute to explain the importance of scarring *versus* culling effects. A high share of large businesses in the United States plays in favour of a key role of demand for explaining the potential of firms to create jobs and grow large. Large firms, like those producing mass goods, in fact, are more sensitive to demand conditions compared to small firms, and may be especially damaged in recessions (Sedlàček and Sterk, 2017). Moreover, the creation of fast-growing "gazelles", which bring novel technology, products or business ideas to market, is also adversely affected in recessions (Sterk et al., 2021).

On the other hand, the interplay of labour market rigidity and high entry costs in Italy contributes to explain a relatively high incidence of good firms entering the market in bad times. High labour market rigidity reduces the ability of firms to adjust labour costs over the cycle, generating uncertainty about the prospective of a new venture.[24] This in turn affects the hiring decisions and the incentives to create a new business in the first place: potential entrepreneurs will find it convenient to invest in startups whose expected profits are high enough to cover entry costs. Legal and economic barriers to entry also work in the direction of favouring the selection of high-quality businesses in recession.[25]

In this section, we explore the role of labour market regulation for business selection, focusing on the employment protection legislation (EPL).[26] We use a quasi-experimental setting, exploiting a key reform of the

[23] Of course, there is significant correlation with the GDP level: entries are procyclical and exits are countercyclical.

[24] Strict EPL, strong bargaining power of unions and the ample coverage of collective bargaining all contribute to increase labour market rigidity.

[25] The overall costs to set up a new business, including licences and paper work, are far higher in Italy compared to the United States (their relative position in the Doing Business ranking for 2019 is, respectively, 58 against 6).

[26] The degree of employment protection - as measured by the general EPL index from the OECD - is higher in Italy than in the United States. The indicator measuring the strictness of regulation for individual dismissals in regular and indefinite contracts has an average value of 2.76 in the nineties for Italy against a value of 0.26 for the United States. The distance between the two countries

dismissal procedure implemented in Italy in 1990. The rules for individual and collective dismissals date back to the "Workers' Statute" of 1970 ("Statuto dei Lavoratori"). Article 18 stipulates that individual dismissals be justified - the so-called "just-cause rule" - and that workers have the right to appeal against employer-initiated dismissals.[27] The article was initially intended for firms with more than 15 employees. It was reformed in 1990, by extending the "just-cause rule" to firms with less than 15 employees.[28] The reform constitutes a tightening of EPL for firms below the threshold of 15 employees. [29]

We exploit variation in the strictness of EPL between small and large firms to test whether a tightening in EPL affects the impact of aggregate conditions at birth. The experiment is meant to substantiate our narrative that a negative impact reflects the culling of the weakest in recession. The logic is that a stricter EPL, by implying higher entry barriers, should lead to stronger selection effects. Therefore, we should observe a larger impact (in absolute value) of aggregate conditions at birth for firms experiencing stricter EPL compared to firms for which EPL has not changed. To identify these effects we make use of an enriched version of a standard difference-in-difference model, in which the main covariate of interest, the continuous variable $\ln Z_C$, is interacted with dummies that split the firms in the sample into the treatment and control groups and the time dimension into periods before and after the treatment.

As a first step, we need to compute the threshold of 15 employees as it is intended for in Article 18 of the labour code. For this purpose, the sample includes employees with indefinite contracts for the entire duration of their employment, while employees with temporary contracts are considered only for the period in which they have effectively worked. In addition, the sample excludes the categories of workers for whom Article 18 does not apply, like interim workers, full-time and part-time consultants. Unfortunately, we are not able to compute the threshold for each establishment as required by Art. 18 because INPS data do not include relevant information at the establishment level. The eventual bias caused by considering the threshold at the firm level would nevertheless be negligible. Only a small portion of businesses in our sample are multi-establishment (almost 4% in the baseline sample) and only a small share of these have establishments close to the threshold of 15 employees.

Having defined the threshold per each firm in each year, the next step is to define a criterion for distinguishing businesses facing an increase in EPL (the treatment group after the reform) and businesses for which EPL is unchanged (the control group). The treatment group includes all the firms that have up to 15 employees in the year of entry, while the control group includes firms that have more than 15 employees at entry. The reform takes place in 1990, around the middle of the time period considered, 1975-2017. To qualify for the treatment, a business should have less than 15 employees on its payroll each year and being born after 1990, and this effect is given by the interaction of the treatment group (i.e., firms with more than 15 employees at entry) and the time dummy for the period 1991-2017, as in a standard DiD model.

Of course, employment is an endogenous choice and firms may choose their level of employment in a strategic way.[30] For example, a business born before 1990 might have chosen to have no more than 14 employees on its payroll so as to benefit from the looser EPL granted to small firms. Strategic sorting violates the assumption that the treatment is randomly assigned, failing to ensure the comparability of businesses assigned to the treatment and control conditions. The presence of a non-negligible share of strategic employers would then affect the size of businesses born before and after 1990 and confound the effects of business conditions at birth. To assuage concerns about non-random sorting and mismeasurement of the threshold, we will consider a regression discontinuity and omit from the analysis the observations closest to the threshold (the "donut hole").

A related concern is that the impact of initial conditions for large and small firms may vary over time for reasons unrelated to the reform. The observation of, say, a stronger impact for small firms after 1990 might simply reflect the trend that small firms would have followed in the absence of treatment. We provide a test for

is similar for other EPL indicators, and remains substantial also in more recent times.

[27] Unjustified dismissals entitle workers to a compensation that, among other criteria, depends on the employer's size. Employers with more than 15 employees must compensate the worker for the foregone wages from the date of the dismissal. Workers that are unjustifiably dismissed may opt for being rehired or for a severance payment of 15 months.

[28] Before 1990, these firms were not subject to the "just-cause" rule, and their employees had no right to appeal against the dismissal. After the reform, they are required to justify the dismissals in accordance to Art.18, and to compensate dismissed workers with a severance payment in the range between 2.5 and 6 monthly wages, depending on the worker's seniority.

[29] In 1991 another change in EPL has further reduced the distance between large and small firms. Specifically, firms with more than 15 employees at risk of bankruptcy are allowed to negotiate with unions conditions for dismissing at least 5 employees in contrast with Article 18. This reduces employment protection for firms with more than 15 employees, going in the same direction of the 1990 reform. Compared to the pre-1990 situation, the distance in terms of employment protection between small and large firms reduces: for small firms EPL increases after 1990 while for large firms EPL falls after 1991. Since the change for small firms is more substantial we focus on this reform, but interpretation would be the same considering any of the two changes in regulation.

[30] We thank an anonymous referee for raising the point.

the assumption of "parallel trends", namely that in the absence of treatment differences between the treatment and control groups are constant over time. The test is based on the interaction of the treatment group and the pre-treatment subperiod. Specifically, the entire period is split into three subperiods, the two subperiods 1975-1982 and 1983-1990 before the treatment and the post-treatment subperiod 1991-2017. Observing a different behaviour for the treatment and control groups in the two subperiods before 1990 would violate the assumption of parallel trends. Notice that the usual approach, considering interactions year by year, is not feasible since our variable of interest varies on a yearly basis and the model would not be identified.

We estimate the following specification:

$$\begin{aligned}\ln Y_{j,ct} &= \alpha + \gamma_a + \gamma_t + \beta_1 \ln Z_c + \beta_2 * T + \beta_3 \ln Z_c * T + \beta_4 \ln Z_c * T * Pre2 + \beta_5 \ln Z_c * T * Post + \\ \sum_{k=1}^{3} \alpha_k I(t &= k) \times T + \sum_{k=1}^{3} \delta_k I(t = k) \times \ln Z_c + X_{j,t} + u_{j,ct} \end{aligned} \tag{7}$$

where T , $Pre2$ and $Post$ are indicator variables taking the value of 1 for, respectively, businesses with less than 15 employees in the year of entry, businesses born in the pre-treatment period 1983-1990, and businesses born after 1990; the omitted time dummy is the first pre-treatment subperiod 1975-1982; and I is the identity matrix. The treatment group (T) is defined as the group of firms that employ up to 15 employees at entry, and the treatment they receive concerns the businesses born after 1990 ($T * Post$).

In the specification above, β_1 measures the impact of aggregate conditions at birth, after controlling for all base effects. Hence our variable of interest, $\ln Z_c$, is interacted with T, $Pre2$ and $Post$ in a fully saturated way, i.e. all interactions between T, $\ln Z_c$ and the time dummies are included (the terms $\sum_{k=1}^{3} \alpha_k I(t = k) \times T$ and $\sum_{k=1}^{3} \delta_k I(t = k) \times \ln Z_c$). This is standard practice for DiD models with a continuous variable, and in our context it allows to capture potential confounders for within-group variation across cohorts. Furthermore, the coefficient β_3 - on the interaction term $\ln Z_c * T$ - measures the impact of initial conditions for firms that belong to the treatment group. It is identified in the omitted period 1975-1982, and reflects differences between the treatment and control groups in this time period (before the treatment). The coefficient β_4 - on $\ln Z_c * T * Pre2$ - captures how these differences vary in the second period before treatment ($Pre2$). Under the hypothesis of parallel trends, we should observe $\beta_4 = 0$, namely that differences in the impact of initial conditions between the treatment and control groups are constant before the treatment.

The coefficient β_5 is the treatment effect. It represents the additional impact of aggregate conditions at birth for businesses facing a tightening of EPL (the treatment group T in the $Post$ treatment period). The relevant comparison is between β_1 and β_5. Our prediction is that a stricter EPL, by increasing entry barriers, entails a stronger selection at birth. We should therefore observe that businesses born in recessions are larger ($\beta_1 < 0$) and the more so after treatment ($\beta_5 < 0$).

The base regression (7) is estimated for the sample of businesses born between 1975 and 2017 that have an employment level between 5 and 25 units in the year of entry, i.e. in a symmetric interval with respect to the threshold of 15 employees. In this way, businesses very distant from the threshold and whose behaviour is likely to be less informative (or even misleading) are excluded from the analysis. The results are in Table 8. The effects of the reform go in the expected direction. In the baseline regression, the coefficient on the treatment effect (β_5) is negative and statistically significant, implying that the culling of the weakest is stronger for businesses that face an increase in EPL. The effect is economically relevant: aggregate conditions at birth have an additional impact of around -3% after the treatment, compared to an average effect of -2.7%.

The base regression is compared to alternative specifications that investigate how sensitive are the results to the outcomes of businesses that are close to the threshold of 15 employees in the year of entry or that grow very fast. We start by excluding from the analysis businesses that are more likely to manipulate selection into the treatment and control groups, and omit businesses with an employment level in the range 14-16, 13-17 or 12-18 (columns 2, 3 and 4, respectively). The exclusion of businesses very close to the threshold is largely inconsequential: the coefficient on the treatment effect (β_5) is almost identical to the one in the base regression (columns (1) and (2)). Interestingly, we find that the pattern is robust to the exclusion of firms more distant from the threshold, namely between 13 and 17 employees, or between 12 and 18 employees. The coefficient on the treatment effect remains significantly negative, and its value slightly smaller in absolute value the farther from the threshold (from 3.2 to 2.9). These findings suggest that eventual errors in the measurement of the threshold or strategic sorting in the treatment and control groups do not play a relevant role. We then exclude businesses that grow fast and may have a disproportionate effect on the results (column 5). Fast-growing gazelles are identified according to the OECD definition as businesses that have a minimum of 10 employees in the base year and display an average employment growth greater than 20% per annum, over a five year period (around 2 percent in our sample). Excluding the gazelles reduces the impact of aggregate conditions at birth, though these remain significant and stronger after the treatment.

Finally, we stress that the effect of a stricter EPL is not an artifact of variation in the impact of initial conditions for reasons unrelated to the reform. We have shown above that differences in the trends for businesses in the treatment and control groups are constant in the pre-treatment period, ensuring that the previous pattern in the data does not play a role for the impact of the reform. Moreover, no other major policy measures, like the reform of Art. 18, were introduced around the middle of the sample period affecting the threshold of 15 employees. The closest change of EPL took place in 1997 with the so-called "Pacchetto Treu ", and this reform was not about firing costs and dismissals but about the introduction of new labour contracts for flexible workers. These contracts apply to all firms, independently of their employment level.

6 Robustness

In this section we conduct a thorough robustness analysis to ensure that the empirical pattern we document is not an artifact of the specific assumptions that we make in our analysis.

6.1 Business cycle measures

One important check concerns the method used for detrending aggregate output. In alternative to the Hodrick-Prescott filter used in the main analysis, we consider two approximations of the ideal band-pass filter that are largely entertained in macroeconomic studies, namely the filters of Baxter-King (BK), and Christiano-Fitzgerald (CF), together with the log-deviation of real aggregate output.[31] We also consider a non-parametric approach using an indicator variable that takes a value of 1 when GDP is above a linear trend and 0 otherwise. The results reported in Table (9) confirm that our baseline findings hold using these indicators. Businesses that start their operations in recessions are larger by a factor that ranges between 0.8 and 1.6 percent depending on the detrending method. The impact is even larger, equal to 2.5 percent, when we use the indicator variable as a proxy for the cohort effect. All these findings are persistent over the firms' life cycle (not shown in Table).

Another caveat is that the pattern that we document may not be specific to the business cycle conditions that prevail at the time of entry. Given that the current state of the economy affects also old businesses, the aggregate conditions faced by businesses of any age may confound the cohort effect. It is therefore worth investigating whether business cycle conditions at inception have a larger effect than business cycle conditions at any other point in time. We have estimated equation (4) using the business cycle indicator from 1 up to 6 years since entry in place of the indicator at inception (see Table 10). We find that conditions faced late in life are less important than the conditions at inception: the coefficients on the business cycle indicator in any year since entry are either not significantly different from zero or they are smaller (in absolute value) than the coefficient in the year of entry. These differences are relevant, especially the farthest is entry. Notice that the conditions in the first year after entry is zero, and it eventually becomes positive later on.

6.2 Differences by class of firm

One caveat in interpreting our findings is that they refer to the average business in the sample, which may conflate ample heterogeneity. In the baseline specification we have exploited detailed information on businesses characteristics, conditioning the analysis on the sector of activity and geographical location. Yet, these and other characteristics, such as size, may have an impact on the imprinting of startups that goes beyond fixed effects. Recent evidence shows that small and young businesses may be more vulnerable to a deterioration in aggregate conditions compared to large and older businesses because they are more dependant on external resources - like

[31] Band-pass filters typically extract smoother cycles compared to a smoothing linear filter as the HP, though they all behave similarly. The log-deviation, on the contrary, gives a fairly different picture, and it is in fact more used for stationarity than for extracting the cyclical component of a series. It is widely recognized that different detrending methods provide different types of information about the data. (Canova, 1998).

bank lending - that move in a procyclical way (Fort et al., 2013). Given the large share of small businesses in the data, it is important to verify whether the cohort effect fades away when business size increases. For this purpose, we have estimated equations (4), (5), and (6) in the main sample for businesses that have more than 5, 10, or 250 employees on average over a 10-year horizon (results in Table 11). In our data, businesses that have up to 5 employees represent two thirds of the total. On the opposite extreme, large businesses with more than 250 employees represent only 0.14 percent of the total. The impact of aggregate conditions at birth is negative for all size classes, but it is statistically significant only for businesses with more than 5 employees.[32] In addition, for these firms the effect persists for two years after entry and then vanishes completely. Compared to the sample containing all the businesses, a clear pattern emerges: the larger the size of the firm the lower the impact of aggregate conditions at inception and over the life cycle. A one percent fall of output below the trend is associated with a larger size of businesses born in that year equal to 1.5 percent on average for businesses with more than 5 employees, against 1.6 for businesses of all sizes and zero for businesses with more than 10 employees. Moreover, the effect fades away far more quickly in samples that contain large businesses. These findings suggest that larger businesses are more resilient to business cycle conditions at birth.

Resilience may also vary systematically with the sector of activity. In the baseline specification we control for heterogeneity across sectors using fixed effects at the 3-digit sector level. These effects, however, are not informative about the implications that operating in a given sector may have for the "cyclical imprinting" of firms. In particular, businesses operating in sectors that are open to international trade, like manufacture, may be relatively more sensitive to initial aggregate conditions compared to businesses that operate in protected sectors, such as constructions and services. In addition, they face higher entry costs for accessing foreign markets (Bilbiie et al., 2012). We would therefore expect a larger elasticity to initial conditions (in absolute value) for manufacturing businesses. To investigate this possibility we construct an indicator variable that takes the value of 1 for businesses that operate in the manufacture sector and zero otherwise.[33] For multi-unit businesses operating in more than one sector, the relevant sector of activity is the one that employs the largest number of employees. We estimate equation (4) including the indicator variable and its interaction with the cohort proxy. The findings in Table 12 show that operating in the manufacturing sector increases the elasticity to economic conditions at inception (in absolute value) by almost 50 percent (the average β is -1.635, while the interacted coefficient is equal to -0.777). The effect appears to be driven by small businesses. For businesses with more than 5 employees, in fact, the interacted coefficient is not significantly different from zero. Once again, the evidence supports the view that cohort effects are particularly large for small and vulnerable businesses.

Lastly, a similar experiment is done for geographical location. In the baseline specification, fixed effects at the provincial level capture heterogeneity, which might be related with several factors, like economic structure (industrial districts, for example, are typically located within one province), the quality of infrastructures, or access to external resources. The fixed effects, however, are silent about the implications that being located in a specific geographic area may have for the impact of the business cycle at entry. We follow a long tradition of regional studies in Italy and focus on the North-South divide.[34] The divide reflects systematic differences across regions in terms of characteristics, like economic structure, internationalization and innovation capacity, that are key for setting up a new business. The level of industrialization in the South has always been relatively low compared with Northern and Central Italy. In the data, employees in the manufacturing sector in the Southern regions are 24 percent of the labour force against 30 percent in the Centre-North. Relative to the total working population the percentage is 3 and 14 respectively (Cannari and Franco, 2011). Southern regions account for a small percentage of the export performance of Italy: all southern regions together export less than 40% of the total exports of Lombardy. Moreover, only less than 3% of the Italian foreign-owned firms - in terms of employees - are located in Southern Italy (Svimez, 2013). This suggests that businesses located in northern regions are relatively more exposed to international competition compared to businesses located in the South. With regard to innovation, southern firms have a lower level of R&D expenditure (0.3% in Southern firms against 0.8% in

[32] The relatively small number of observations combined with two-way clustered errors contributes to explain the lack of statistical significance. In regressions with standard errors clustered at the business level, all coefficients of interest are significant except for the sample of very large businesses.

[33] Non-manufacture sectors include agriculture, services, and constructions. We have also experimented an indicator variable that excludes businesses operating in agriculture. The exclusion of agriculture is inconsequential.

[34] The gap in GDP per capita between the South and the Centre-North has ranged from 55% to 60% from the 50's until the present day (Musolino, 2018). In terms of labour productivity, Southern Italy is 20% below Central-Northern Italy, while in terms of the employment rate the two macro-regions are even farther from each other, about 30% (Cannari and Franco, 2011). An immense literature has analysed the causes of the divide, stressing the importance of economic, social, institutional, and historical factors.

Central and Northern firms), and a smaller number of Southern firms are able to introduce innovations (Padovani, 2013). Since entry costs are relatively high in high-tech sectors, one can expect that northern businesses are more exposed to selection compared to southern businesses. For all these reasons, northern businesses might be more sensitive to the business cycle at entry. Hence, we construct an indicator variable that takes the value of 1 for businesses that operate in northern and central regions and zero otherwise, and estimate equation (4) including the indicator variable and its interaction with the cohort proxy (see Table 13).[35] The cohort effect is larger (in absolute value) for businesses located in the north by almost 55 percent (the average β is -1.135, while the interacted coefficient is equal to -0.623). Notice that the effect turns not significant for businesses with more than 5 employees. Once again, the impact of initial conditions appears to have culling effects for small businesses and particularly for those that operate in relatively more industrialized areas.

Overall, these findings suggest important differences in the impact of cohort effects depending on size, location and sector of activity. Section 6.5 will investigate further dimensions of heterogeneity for the sub-sample of company businesses.

6.3 Local conditions

So far we have considered the impact of aggregate conditions. Yet, many businesses, and especially small businesses, may be more exposed to the conditions that prevail in the markets in which they sell their products. These conditions in turn may display ample variability across regions and sectors. It is instructive at this point to consider local indicators of the business cycle, using GDP at regional level and value added at the macro-sector level. Table 2 contains summary statistics on regional and sectoral business cycle indicators, while Table 14 displays the results of regression (4) where each of these indicators is used in turn.

Accounting for regional and sector-specific initial conditions does not alter the main pattern found in the data. Businesses born in recessions are on average larger than businesses born in expansions, and the elasticity to initial conditions is of comparable magnitude using aggregate, regional or sectoral indicators. A one percent fall in value added in a given sector leads to an increase in the size of startups active in that sector that is equal to 1.2 percent on average, against a value of 1.6 found using the aggregate indicator. As in the baseline specification, the cohort effect tends to reduce as firm size increases: the estimated elasticity is 0.8 (0.5) in the sample that contains businesses with at least 5 (10) employees, and becomes non significantly different from zero in the sample of businesses with more than 250 employees (not shown in Table). A fall in regional GDP leads to an increase in the size of businesses born in that region equal to 0.5 percent on average, against a value of 0.7 that we obtain in the regression that uses the aggregate indicator. Also in this case, the cohort effect is negligible for large businesses.

6.4 Aggregate demand

An important insight from the evidence above is that more resilient businesses tend to start up during recessions, improving the average quality of cohorts born in recession. Yet, a decline in aggregate demand occurring at the time of birth or later on during the firm life may reduce firm-level employment and constrain firms' expansion plans for entrants as well as for incumbent firms. Systematic variation of demand conditions between firms may lead to a bias in the potential damage caused by recessions within cohorts.

In the baseline regression, period time fime effects are used to control for conditions - like aggregate demand - that affect all firms in a given period of time. Fixed effects, however, are not informative about variation of these conditions over time and more importantly about their impact on variation between firms and within cohorts. For this purpose, we introduce additional covariates capturing the aggregate demand conditions faced by all firms in each period of their life cycle. Specifically, we consider private and public components of aggregate demand, including private consumption expenditure net of durables, total government expenditure, government expenditure net of interest payments, general government net lending/borrowing (total and primary), and the public deficit. All these variables are expressed as percentage of GDP. Private and public expenditures are introduced jointly, together with each measure of the overall fiscal stance considered in turn. The scope of these covariates is to capture between-firm variability in demand conditions which might confound the impact of aggregate conditions at birth. We are agnostic about the effect of each of these regressors on average firm-level

[35] We have experimented an alternate classification that considers northern regions against central and southern regions, with no appreciable effects on the results.

employment, since variation between firms and over time may go in any direction. Our main interest is to verify how sensitive are our results to variation of demand conditions across firms.

Table 15 displays the results of these regressions. The finding that cohort effect are negative is robust to the introduction of demand variables: businesses born in recessions are still significantly larger than businesses born during expansions, and the effect is economically relevant. Quantitatively, the elasticity to initial conditions reduces from 1.5 in the baseline regression to values ranging from 0.8 to 1.2 depending on the control used for aggregate demand.

6.5 Business performance

So far we have focused on employment as the dependent variable. Here we consider measures of performance together with alternate measures of size. The exercise has a twofold objective. First, by documenting the relationship between measurable dimensions of business quality and business cycle conditions at inception, it provides evidence on selection. Second, it considers a broad range of outcomes which can be affected by the original sin, including labour productivity and revenues. Data availability limits the evidence for the subset of company businesses over the period 2004-2017.[36] The outcomes of these businesses are followed for up to 5 years since entry (instead of 10 years in the baseline specification). The sample includes businesses that do not survive for the whole period.[37]

Table 16 displays the results of regressions where the dependent variable is alternatively a measure of labour productivity (turnover per worker) and a measure of size (employment or total revenues). For each regression, we consider the baseline specification (4) (panel A); a specification that includes firm-level proxies for business quality measured on a year by year basis (panel B); and a specification that considers the same proxies but measured at the time of entry (panel C).[38] Proxies for business quality are meant to capture variation between firms which can affect the outcomes of interest. They include tangible and intangible assets (ratio to total revenues), total factor productivity (TFP)[39] and the return on asset (ROA). The former two variables capture investments in technology and knowledge capital, which are positively related to both firm size and labour productivity; total factor productivity is a direct measure of productivity at the firm level; the ROA is a measure of profitability. In all regressions, the business cycle indicator is the HP-filtered GDP at inception.[40]

The key pattern documented for the main sample holds true also for the sample of company businesses. Recessionary startups are larger than expansionary startups, no matter the measure of size that is used, the level of employment or total revenues, and the specification considered. The revenues of businesses that are born in a year in which aggregate output is 1 percent below the trend are larger by 0.8 percent on average. In contrast to what found for the main sample, these effects increase (in absolute value) with firm size. The estimated elasticity is around 1.3 percent for businesses that have more than 5 employees, and around 1.7 for businesses that have more than 10 employees (not shown in Table). In regressions in which the dependent variable is the level of employment, the estimated elasticity ranges between 0.36 and 1.09 percent depending on the specification considered. It becomes not different from zero for large firms (with more than 5 employees). In general, the estimated coefficients on the cohort variable (β) are lower (in absolute value) for the baseline specification (panel A) compared to the specifications that include proxies for firm quality (panels B and C). This suggests that variation in the quality of firms is indeed important, especially variation of startups' quality, and implies a stronger effect of culling of the weakest in recessions.

[36] Preliminary results show that extending the the sample for the period 1998-2018 does not alter the pattern found in the data.

[37] We also consider an unbalaced panel where outcomes are tracked until the end of the sample period. The empirical pattern is very similar to what we document below (results are available upon request).

[38] We have also considered the specifications (5) and (6), obtaining evidence of persistent cohort effects (results not shown in Table and available upon request).

[39] TFP at the firm level is defined as the Solow residual:

$$Residual \equiv g_{VA} - [\alpha g_L + \beta g_K]$$

where g_{VA} is the growth rate of firm-level value added, g_L is the growth rate of firm-level labor costs, g_K is the growth rate of firm-level capital, α is the share of labor and β is the share of capital. Factor shares are estimated using a log-specification of the firm-level production function:

$$\ln(VA_{it}) = \beta \ln(K_{it}) + \alpha \ln(L_{it}) + \epsilon_{it}$$

where production is approximated by value added (VA), while capital (K) and labor (L) inputs are approximated by tangible assets and labor cost, respectively (see Ciani et al., 2019). All variables are deflated using the sector-specific deflator of value added.

[40] We have experimented alternative detrending methods with no remarkable consequences (results are available upon request).

Remarkably, we find negative cohort effects in regressions in which the dependent variable is labour productivity, indicating that businesses born in recessions are not only larger but also more productive than businesses born during expansions. The effect is economically significant: cohorts born in a year in which output is one percent below the trend are more productive by a factor of 0.4 percent on average. The finding is robust across specifications, with an estimated elasticity in the range between 0.14 and 0.56 percent. The presence of covariates varying on a yearly basis generates stronger cohort effects. Instead, the elasticity reduces but remains significant when we allow for variation in the quality of businesses at the time of entry. Similarly to what we found for revenues, labour productivity is more responsive to business cycle conditions at birth when we restrict the sample to firms that have more than 5 employees (estimated coefficients vary in the range between 0.6 and 1.2).

A high labour productivity may stem from a variety of sources, including more skilled employees, a more efficient work organization, a high level of capital per worker, or improved technology. Though we are unable to discriminate among potential factors, we focus on technology improvements (TFP) and capital endowment (tangible and intangible assets). The analysis of how these variables are distributed across firms provides useful insights for our purposes. Productivity growth is highly dispersed across firms: for the median business in the sample TFP increases by only 0.06 percent over the whole period, while it increases by 1.15 percent for businesses in the top 5% and it reduces by 1.4 percent for businesses in the bottom 5%. The pattern is similar if we consider only periods of expansion, periods of recession or the distribution on a yearly basis. As for the distribution of capital, tangible assets are around 0.4 percent of revenues for the average business in the sample; the comparable measure for intangible assets is 0.14. Yet, the median value of tangible (intangible) assets is equal to 0.56 (0.01), spanning from a value as high as 1.4 (0.57) for businesses in the top 5% of the distribution to a value of 0 (0) for the bottom 5%. Similar values hold considering only expansionary or recessionary periods, or the distributions on a yearly basis. These patterns indicate that between-firm variation in technology and capital is large, and a very small portion of firms has very high productivity or very large capital endowments. In addition, these distributions are stable over time. Hence, between-firm variation along both these dimensions is important for explaining firm-level productivity (in fact, the coefficients on related covariates are statistically significant), and largely more important than variation over time. This in turn suggests that changes in the distribution of productivity or capital are likely to play a minor role for cohort effects. We can see why using the example of the latent variable model of Section 2. We argued before that variation in the unobserved quality of businesses over the cycle may depend on systematic changes in the probability distribution, $F(q_i)$, and/or on changes in the threshold of firm quality, q_0, and these have different implications for the scarring and culling effects of recessions. Using TFP and capital as proxies for quality (q_i), the patterns described above imply that variation of quality over the cycle is more likely to derive from a change in q_0 rather than a deterioration in the probability distribution, $F(q_i)$. As argued before, this fits well with our narrative that cohort effects reflect the culling of the weakest.

7 Conclusions

We have estimated the long-term effects of entering the market in recessionary periods for the universe of Italian businesses over the period 1975-2017. We find that businesses born in recessions are larger and more productive compared to businesses born during expansions. These effects are persistent and tend to increase as firms grow older. The impact is economically relevant: a one percent decline in output below the trend in the year of entry is associated with an increase in firm-level employment equal to 1.6 percent on average and as high as 2.6 percent after 10 years of activity. These patterns are robust to fixed effects at sectoral, provincial and time level; exit attrition; regional and sectoral economic conditions; aggregate demand; firm common characteristics; firm quality, and the dynamics of business formation. We document substantial heterogeneity in the magnitude of the effect (but not the sign) depending on firms' dimension class, location and sector of activity.

An important contribution of the paper is the analysis of the mechanism behind these patterns. Negative cohort effects suggest a relatively strong mechanism of "culling of the weakest" compared to the scarring effect of recessions. Selection depends crucially on the costs faced by potential entrants for setting up a new venture. An increase in these costs, by deterring entry of relatively low-productivity businesses, may determine a high incidence of good firms entering in bad times. Scarring, on the other hand, depends on the degree to which firms are sensitive to demand conditions. Large firms, by producing mass goods on a large scale, are more vulnerable to a deterioration in demand conditions compared to smaller firms. High entry barriers together with a large share of very small firms contribute to generate strong culling effects among Italian firms. To corroborate our narrative that selection is important, we have exploited a radical reform of the individual dismissal procedure implemented in Italy in 1990 for a quasi-experiment. The reform extends to small firms the stricter dismissal rules deployed for large firms, implying a tightening of EPL and larger entry barriers for small firms. Based on difference-in-difference methods, we provide evidence that stricter dismissal rules indeed generate stronger selection and increase the employment gap in favour of recessionary startups.

The empirical patterns in our study suggest that aggregate conditions at birth leave an increasingly large footprint on firm-level employment, stressing the role of start-ups for job creation. Further research is needed to study the impact of policies aimed at supporting business activity in the light of the firm life cycle and the aggregate business cycle.

8 Appendix: Tables

Table 1: Summary Statistics

The table reports summary statistics for the samples used in the analysis. Panel A presents the statistics for the baseline sample 1, which includes all businesses born between 1975 and 2017 and their level of employment for up to 10 years. The outcomes of businesses that did not survive for the entire period are included. Panel B presents the statistics for sample 2 which includes all company businesses born between 2004 and 2017 and their outcomes for up to 5 years. The outcomes of businesses that did not survive for the entire period are included. Sample 2 contains outcomes from administrative and balance sheet data. Panel C presents the statistics for the samples used in the robustness exercises. Sample 3 includes all businesses born between 1975 and 2017 and their level of employment including only businesses which survive for at least 5 years (age 5-10). The outcomes of businesses that did not survive over the period are excluded. Sample 4 includes all businesses born between 1975 and 2017 and their level of employment including only businesses which survive for 10 years. The outcomes of businesses that did not survive over the period are excluded. Sample 5 includes all businesses born between 1975 and 2017 and their level of employment over the entire period (unbalanced sample).

Panel A - Main Sample 1						
Cohorts 1975-2017, Ages 1-10						
	Mean	Median	St. Dev.	10th %ile	90th %ile	Obs.
Empl. (headcount)	6.158	2.0	77.601	1.0	12.0	20,883,884
Empl. (weighted)	3.785	1.083	35.584	0.333	7.083	20,883,884
Panel B - Sample 2						
Cohorts 2004-2017, Ages 1-5						
	Mean	Median	St. Dev.	10th %ile	90th %ile	Obs.
Empl. (headcount)	10.33369	4	32.82088	1	21	861,496
Empl. (weighted)	5.601986	2	17.69363	.4166667	11.5	861,496
Productivity (turnover per worker)	160.3375	61.20245	354.1946	11.70393	349.3333	861,496
Productivity (turnover per weightedworker)	315.7353	111.6723	776.6657	28.08945	650.3079	861,496
PANEL C						
Sample 3						
Cohorts 1975-2017, Ages 5-10						
	Mean	Median	St. Dev.	10th %ile	90th %ile	Obs.
Empl. (headcount)	7.174892	3	85.222	1	13	17,961,572
Empl. (weighted)	4.801404	1.667	41.24589	0.5	9	17,961,572
Sample 4						
Cohorts 1975-2017, Ages 10						
	Mean	Median	St. Dev.	10th %ile	90th %ile	Obs.
Empl. (headcount)	7.4646	3	67.95712	1	14	14,081,224
Empl. (weighted)	5.16987	1.916667	38.26773	0.5	9.583334	14,081,224
Sample 5						
Cohorts 1975-2017, Ages 1-43						
	Mean	Median	St. Dev.	10th %ile	90th %ile	Obs.
Empl. (headcount)	6.641962	2	76.26242	1	12	27,086,453
Empl. (weighted)	4.253214	1.25	37.31782	.3333333	8	27,086,453

Table 2: Business cycle indicators

The table reports descriptive statistics for the business cycle indicators used in the analysis. Panel A reports the statistics for the business cycle indicators at the national level. GDP_HP is the Hodrick-Prescott (HP)-filtered natural logarithm of the annual aggregate real gross domestic product. GDP_BK and GDP_CF are the band-passed filtered logarithms of the annual aggregate real gross domestic product obtained with the Backus-Kehoe (BK) and the Christiano-Fitzgerald (CF) filters respectively. Log diff is the demeaned difference in logarithms of the annual aggregate real gross domestic product. Panel B reports the statistics for the corresponding indicators at regional level while Panel C reports the statistics for the indicators at sectoral level.

Panel A							
Aggregate business cycle indicators (1975 - 2017)							
	Mean	St. Dev.	Min	Max	Corr with GDP	Corr. with Log diff	Obs.
GDP_HP	0,000	0,012	-0,031	0,021	0,236	0,526	44
GDP_BK	0,001	0,011	-0,029	0,024	0,390	0,496	38
GDP_CF	0,000	0,010	-0,033	0,021	0,170	0,556	44
Log _Diff	0,013	0,021	-0,056	0,064	0,208	1,000	43

Panel B					
Regional business cycle indicators [GDP_HP] (1995-2017)					
	Mean	St. Dev.	Min	Max	Obs.
Piemonte	0.000	0.015	-0.05	0.028	23
Valle d'Aosta	0.000	0.015	-0.044	0.025	23
Liguria	0.000	0.014	-0.02	0.030	23
Lombardia	0.000	0.013	-0.036	0.025	23
Trentino	0.000	0.011	-0.023	0.020	23
Veneto	0.000	0.014	-0.034	0.027	23
Friuli	0.000	0.019	-0.041	0.036	23
Emilia	0.000	0.015	-0.039	0.030	23
Toscana	0.000	0.010	-0.022	0.022	23
Umbria	0.000	0.015	-0.038	0.029	23
Marche	0.000	0.013	-0.025	0.032	23
Lazio	0.000	0.011	-0.016	0.022	23
Abruzzo	0.000	0.016	-0.035	0.023	23
Molise	0.000	0.015	-0.033	0.035	23
Campania	0.000	0.011	-0.019	0.026	23
Puglia	0.000	0.013	-0.026	0.026	23
Basilicata	0.000	0.019	-0.032	0.040	23
Calabria	0.000	0.010	-0.019	0.016	23
Sicilia	0.000	0.010	-0.018	0.017	23
Sardegna	0.000	0.010	-0.015	0.021	23
Panel C					
Sectoral business cycle indicators [GDP_HP] (1975-2017)					
	Mean	St. Dev.	Min	Max	Obs.
Agricolture	0.000	0.023	-0.062	0.049	44
Industry	0.000	0.026	-0.093	0.054	44
Construction	0.000	0.019	-0.038	0.037	44
Services	0.000	0.008	-0.019	0.014	44

Table 3: Employment and the business cycle at entry

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. Age is the number of years since entry. γ_i is an indicator variable that takes the value of one if the business is of i years of age. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

Ln (Employment)			
Ln Zc	-1.593*** (0.302)	-0.853* (0.456)	
Ln Zc x Age		-0.204** (0.100)	
γ_1 x ln Zc			-1.396* (0.710)
γ_2 x ln Zc			-1.248* (0.639)
γ_3 x ln Zc			-1.367* (0.602)
γ_4 x ln Zc			-1.660** (0793)
γ_5 x ln Zc			-1.970* (1.012)
γ_6 x ln Zc			-2.526** (1.097)
γ_7 x ln Zc			-2.519*** (1.153)
γ_8 x ln Zc			-2.333*** (1.155)
γ_9 x ln Zc			-2.575*** (1.074)
Obs	16,096,662	16,096,662	16,096,662
R squared	0.203	0.203	0.203
Sample	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes

Table 3 bis: Employment and the business cycle at entry

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, non-weighted per effective working time. Age is the number of years since entry. γ_i is an indicator variable that takes the value of one if the business is of i years of age. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

Ln (Employment)			
Ln Zc	-1.580*** (0.283)	-1.132*** (0.474)	
Ln Zc x Age		-0.124 (0.084)	
γ_1 x ln Zc			-0.850 (1.070)
γ_2 x ln Zc			-1.112 (0.667)
γ_3 x ln Zc			-1.369*** (0.505)
γ_4 x ln Zc			-1.551** (0.641)
γ_5 x ln Zc			-1.975** (0.869)
γ_6 x ln Zc			-2.304** (0.984)
γ_7 x ln Zc			-2.399** (1.018)
γ_8 x ln Zc			-2.220** (1.026)
γ_9 x ln Zc			-2.162** (1.026)
Obs	16,114,177	16,114,177	16,114,177
R squared	0.170	0.170	0.170
Sample	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes

Table 4: Employment and the business cycle at entry: life-long consequences

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. Age is the number of years since entry. γ_i is an indicator variable that takes the value of one if the business is of i years of age. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes over the entire period. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	Ln (Employment)		
Ln Zc	-1.782*** (0.244)	-1.393*** (0.373)	
Ln Zc x Age		-0.047***(0.021)	
γ_1 x ln Zc			-1.404* (0.781)
γ_2 x ln Zc			-1.311* (0.722)
γ_3 x ln Zc			-1.420** (0.568)
γ_4 x ln Zc			-1.635** (0.664)
γ_5 x ln Zc			-1.895** (0.801)
γ_6 x ln Zc			-2.446*** (0.874)
γ_7 ln Zc			-2.502*** (0.915)
γ_8 x ln Zc			-2.349*** (0.949)
γ_9 x ln Zc			-2.557*** (0.895)
Obs	21,966,575	21,966,575	21,966,575
R squared	0.201	0.201	0.201
Sample	Sample 5	Sample 5	Sample 5
Age fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes

Table 5: Employment and the business cycle at entry: survivors for more than 5 years

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. Age is the number of years since entry. γ_i is an indicator variable that takes the value of one if the business is of i years of age. The sample used in this table contains all the businesses born between 1975 and 2017 which survive for 5 years or more. The outcomes of businesses that did not survive over the period are not included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	Ln (Employment)		
Ln Zc	-1.779*** (0.257)	-1.257***(0.420)	
Ln Zc x Age		-0.052** (0.020)	
γ_1 x ln Zc			-0.645 (1.548)
γ_2 x ln Zc			-0.760 (1.661)
γ_3 x ln Zc			-1.073 (1.610)
γ_4 x ln Zc			-1.523 (1.470)
γ_5 x ln Zc			-1.919*** (0.697)
γ_6 x ln Zc			-2.389*** (0.784)
γ_7 ln Zc			-2.525*** (0.791)
γ_8 x ln Zc			-2.327*** (0.879)
γ_9 x ln Zc			-2.338*** (0.982)
Obs	17,961,572	17,961,572	17,961,572
R squared	0.180	0.180	0.180
Sample	Sample 3	Sample 3	Sample 3
Age fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes

Table 6: Employment and the business cycle at entry: survivors for 10 years

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. Age is the number of years since entry. γ_i is an indicator variable that takes the value of one if the business is of i years of age. The sample used in this table contains all the businesses born between 1975 and 2017 which survive for 10 years. The outcomes of businesses that did not survive over the period are not included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	Ln (Employment)		
Ln Zc	-2.192*** (0.176)	-2.143***(0.257)	
Ln Zc x Age		-0.004 (0.013)	
γ_1 x ln Zc			-1.986* (1.065)
γ_2 x ln Zc			-1.995* (1.099)
γ_3 x ln Zc			-2.021* (1.138)
γ_4 x ln Zc			-2.576** (0.996)
γ_5 x ln Zc			-2.398* (1.202)
γ_6 x ln Zc			-2.642** (1.171)
γ_7 ln Zc			-2.452** (1.201)
γ_8 x ln Zc			-2.295* (1.209)
γ_9 x ln Zc			-2.129* (1.154)
Obs	13,543,080	13,543,080	13,543,080
R squared	0.185	0.185	0.185
Sample	Sample 4	Sample 4	Sample 4
Age fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes

Table 7: Employment and the business cycle at entry: proxies for selection

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

Ln (Employment)		
Ln Zc	-1.593*** (0.302)	-1.159***(0.254)
Entries		0.179*(0.106)
Exits		-0.075*** (0.020)
Obs	16,096,662	16,064,247
R squared	0.203	0.197
Sample	Sample 1	Sample 1
Age fixed effects	Yes	Yes
Year fixed effects	No	No
Province fixed effects	Yes	Yes
Sector fixed effects	Yes	Yes

Table 8: The role of EPL

The table reports the coefficients of the DiD experiment on the EPL reform. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. The sample used in this table contains all the businesses born between 1975 and 2017 which have between 5 and 25 employees, and their outcomes up to 10 years. The outcomes of businesses that did not survive over the entire period are included. Column 1 refers to the baseline regression (7) on the whole sample. Columns 2-4 consider samples excluding firms that have, respectively, between 14 and 16, 13 and 17, or 12 and 18 employees in the year of entry; column 5 excludes fast-growing gazelles. Standard errors (in parentheses) are two-way clustered at the firm and time level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)	(5)
	5-25	5-14&16-25	5-13&17-25	5-12&18-25	no gazelles
		Ln(Employment)			
$\ln Z_c$	-2.700** (1.341)	-2.295* (1.239)	-2.155* (1.231)	-2.778** (1.436)	-1.638* (1.028)
T	-0.761*** (0.011)	-0.807*** (0.002)	-0.848*** (0.012)	-0.888*** (0.011)	-0.754*** (0.008)
$\ln Z_c * T$	2.810** (1.421)	2.900* (1.466)	2.617*** (1.413)	2.328*** (1.338)	1.838* (1.104)
$\ln Z_c * T * Post$	-3.259** (1.520)	-3.291** (1.552)	-3.183** (1.542)	-2.917** (1.469)	-2.416** (1.222)
$\ln Z_c * Pre_2 * T$	-0.900 (1.050)	-1.396 (0.849)	-1.196 (0.764)	-0.289 (1.211)	-0.873 (0.605)
Obs	868,241	831,530	789,809	745,562	651,491
R squared	0.139	0.140	0.140	0.141	0.151
Sample	Sample 1	Sample 1	Sample 1	Sample1	Sample1
Time*T	Yes	Yes	Yes	Yes	Yes
Time*$\ln Z_c$	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes

Table 9: Alternate detrending methods

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level. $\ln Z_c\_BK$ is the log of the BK-filtered annual real gross domestic product, $\ln Z_c\_CF$ is the log of the CF-filtered annual real gross domestic product, and Log diff is the is the demeaned difference in logarithms of the annual real gross domestic product. I_cycle is and indicator variable that takes the value of 1 if annual real gross domestic product is above a linear trend and zero otherwise. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	Ln (Employment)			
$\ln Z_c\_BK$	-1.584*** (0.303)			
$\ln Z_c\_CF$		-0.833*** (0.219)		
Log diff			-0.357* (0.178)	
I_*cycle*				-0.025*** (0.007)
Obs	15,486,382	16,096,662	16,069,995	16,114,177
R squared	0.198	0.203	0.203	0.169
Sample	Sample 1	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes

Table 10: Employment and the business cycle after entry

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level. $\ln Z_c\_i$ is the main business cycle indicator after i years since entry. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	Ln (Employment)					
$\ln Z_c\_1$	-1.593*** (0.302)					
$\ln Z_c\_2$		-0.146 (0.291)				
$\ln Z_c\_3$			0.498** (0.215)			
$\ln Z_c\_4$				0.503 (0.315)		
$\ln Z_c\_5$					-0.217 (0.356)	
$\ln Z_c\_6$						0.068 (0.251)
Obs	16,096,662	16,072,239	16,020,761	15,919,575	15,799,430	15,634,538
R squared	0.203	0.202	0.201	0.200	0.199	0.199
Sample	Sample 1	Sample 1	Sample 1	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 11: Employment and the business cycle at entry: firm size

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. Age is the number of years since entry. γ_i is an indicator variable that takes the value of one if the business is of i years of age. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Panel A reports the results of regression (4). Panel B and C report the results of regressions (5) and (6) respectively. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

Ln (Employment)			
	Size >5	Size >10	Size >250
Panel A			
$\ln Z_c$	-1.466** (0.546)	-0.999 (0.622)	0.715 (2.155)
Obs	4,418,799	1,970,103	11,851
R squared	0.179	0.177	0.265
Panel B			
$\ln Z_c$	-1.276 (1.319)	-1.040 (1.549)	1.987 (3.441)
$\ln Z_c$ x Age	-0.049 (0.230)	0.010 (0.034)	-0.334 (0.681)
Obs	4,418,799	1,970,103	11,851
R squared	0.179	0.177	0.265
Panel C			
γ_1 x $\ln Z_c$	-1.568* (1.003)	-1.243 (1.187)	1.712 (3.600)
γ_2 x $\ln Z_c$	-1.551** (0.695)	-1.391 (2.255)	2.903 (4.801)
γ_3 x $\ln Z_c$	-1.198 (2.037)	-0.913* (0.779)	-0.869 (4.302)
γ_4 x $\ln Z_c$	-1.661 (1.353)	-1.106 (0.943)	-3.302 (3.466)
γ_5 x $\ln Z_c$	-2.024 (1.658)	-1.466 (1.690)	0.404 (4.386)
γ_6 x $\ln Z_c$	-1.982 (2.037)	-1.062 (1.997)	0.841 (4.013)
γ_7 x $\ln Z_c$	-1.776 (2.027)	-1.072 (2.243)	-0.460 (4.985)
γ_8 x $\ln Z_c$	-1.469 (1.967)	-0.572 (2.224)	-1.790 (4.469)
γ_9 x $\ln Z_c$	-1.440 (1.947)	-0.852 (2.096)	-0.512 (5.190)
Obs	4,418,799	1,970,103	11,851
R squared	0.179	0.177	0.265
Sample	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes

Table 12: Employment and the business cycle at entry: manufacture

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. $I\_manufacture$ is an indicator variable that takes the value of 1 for businesses operating in manufacturing and a value of 0 otherwise. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

Ln (Employment)		
		Size >5
$\ln Z_c$	-1.635*** (0.287)	-1.173*** (0.404)
$\ln Z_c$ x $I\_manufacture$	-0.777* (0.403)	-0.033 (0.669)
$I\_manufacture$	0.492*** (0.015)	0.275*** (0.016)
Obs	20,861,880	5,513,489
R squared	0.143	0.136
Sample	Sample 1	Sample 1
Age fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Province fixed effects	Yes	Yes
Sector fixed effects	Yes	Yes

Table 13: Employment and the business cycle at entry: North-South

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. I_North is an indicator variable that takes the value of 1 for businesses located in northern and central regions and a value of 0 otherwise. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

Ln (Employment)		
		Size >5
$\ln Z_c$	-1.135*** (0.347)	-1.803*** (0.558)
$\ln Z_c$ x I_North	-0.623*** (0.271)	0.511 (0.347)
I_North	0.054*** (0.008)	0.068*** (0.009)
Obs	16,096,662	4,418,799
R squared	0.199	0.174
Sample	Sample 1	Sample 1
Age fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Province fixed effects	Yes	Yes
Sector fixed effects	Yes	Yes

Table 14: Employment and the business cycle at entry: local and sectoral indicators

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. $\ln Z_c\_reg$ is the log of the HP-filtered annual real gross domestic product at regional level, $\ln Z_c_$sec is the log of the HP-filtered annual real value added at sector level. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

	Ln (Employment)			
$\ln Z_c$	-0.655* (0.360)		-1.593*** (0.302)	
$\ln Z_c\_reg$		-0.534* (0.273)		
$\ln Z_c\_sec$				-1.149*** (0.178)
Obs	9,190,087	9,190,087	16,096,662	16,096,662
R squared	0.179	0.179	0.203	0.203
Sample	Sample 1	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes

Table 15: Employment and the business cycle at entry: aggregate demand

The table reports the coefficients of OLS regressions. The dependent variable is the natural logarithm of the number of employees at the firm level, weighted per effective working time. The sample used in this table contains all the businesses born between 1975 and 2017 and their outcomes up to 10 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and the year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively. and 10% respectively.

	Ln (Employment)			
ln Z_c	-1.520*** (0.310)	-1.158*** (0.348)	-1.192*** (0.381)	-0.793** (0.308)
ln (Priv. Cons.)$_t$		-0.006 (0.012)	0.019 (0.013)	-0.032** (0.015)
ln (Net lending)$_t$		0.023***(0.004)		
ln (Gov. Exp.)$_t$		-0.020*** (0.005)		
ln (Primary net lending)$_t$			0.014*** (0.003)	
ln (Primary Gov. exp)$_t$			-0.043*** (0.007)	
ln (Public deficit)$_t$				-0.017*** (0.004)
Obs	15,753,911	15,753,911	15,753,911	13,821,038
R squared	0.203	0.198	0.199	0.189
Sample	Sample 1	Sample 1	Sample 1	Sample 1
Age fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes

Table 16: Performance and the business cycle at entry

The table reports the coefficients of OLS regressions. The dependent variable in columns (1)-(3) is given by the natural logarithm of the number of employees at the firm level (weighted per effective working time), the natural logarithm of total revenue (ratio to value added), the natural logarithm of value added per employee respectively. Panel A displays the results of regressions with age and period fixed effects. Panel B displays the results of regressions that include additional covariates measured on a period by period basis, and Panel C displays the results of regressions that include additional covariates measured in the year of entry. The sample used in this table contains all company businesses born between 2004 and 2017 and their outcomes up to 5 years. The outcomes of businesses that did not survive over the period are included. Standard errors (in parentheses) are two-way clustered at the firm and year level. ***, **, and *, denote statistical significance at 1%, 5% and 10% respectively.

				Size >5		
	ln (empl.)	ln (revenue)	ln (productivity)	ln (empl.)	ln (revenue)	ln (productivity)
	Panel A					
Ln Zc	-0.361*	-0.764**	-0.404*	-0.138	-1.301***	-1.163**
	(0.191)	(0.275)	(0.261)	(0.486)	(0.434)	(0.434)
Obs	655,617	655,617	655,617	412,075	292,731	292,731
R squared	0.209	0.215	0.247	0.159	0.272	0.292
	Panel B					
Ln Zc	-0.364	-0.924***	-0.563*	-0.055	-1.235*	-0.624**
	(0.328)	(0.230)	(0.287)	(0.520)	(0.666)	(0.299)
ln (value added)_t	0.063**	-0.146***	-0.209***	0.027***	-0.163**	-0.206**
	(0.013)	(0.039)	(0.055)	(0.009)	(0.061)	(0.083)
ln (tang. assets)_t	-0.060***	-0.064***	-0.004	-0.047***	-0.001	-0.010***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
ln (intang. Assets)_t	-0.178***	-0.248***	-0.070***	-0.135***	-0.007	0.023
	(0.006)	(0.008)	(0.009)	(0.010)	(0.012)	(0.007)
ROA_t	-0.383***	0.315***	0.069***	0.428***	0.177***	-0.003***
	(0.019)	(0.021)	(0.008)	(0.015)	(0.019)	(0.0014)
TFP_t	0.045***	0.645***	0.600***	0.248***	0.496***	0.334***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.020)	(0.023)
Obs	578,576	451,605	578,576	221,734	221,734	221,734
R squared	0.228	0.397	0.381	0.217	0.445	0.431
	Panel C					
Ln Zc	-1.199***	-1.339***	-0.140*	-0.611	-1.235***	-0.624**
	(0.389)	(0.423)	(0.101)	(0.738)	(0.355)	(0.272)
ln (value added)_c	0.057**	-0.108***	-0.165***	0.043**	-0.163***	-0.206***
	(0.019)	(0.033)	(0.052)	(0.022)	(0.060)	(0.079)
ln (tang. assets)_c	-0.027***	-0.024***	-0.003	-0.011***	-0.001	-0.010***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
ln (intang. Assets)_c	-0.052***	0.064***	-0.013*	-0.030***	-0.007	-0.023***
	(0.010)	(0.011)	(0.010)	(0.008)	(0.010)	(0.007)
ROA_c	-0.139***	-0.210***	-0.071***	-0.174***	-0.177***	-0.003
	(0.010)	(0.013)	(0.011)	(0.013)	(0.015)	(0.011)
TFP_c	-0.009**	0.437***	-0.445***	0.161***	-0.496***	0.334***
	(0.009)	(0.013)	(0.013)	(0.007)	(0.011)	(0.009)
Obs	367,658	367,658	367,658	168,929	168,929	168,929
R squared	0.207	0.353	0.359	0.180	0.324	0.355
Sample	Sample 2	Sample 2	Sample 2	Sample 2	Sample 2	Sample 2
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

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