KU LEUVEN

FACULTY OF ECONOMICS
AND BUSINESS

The Returns to Entrepreneurship in the Labor Market

Dissertation presented to
obtain the degree of
Doctor in Business Economics

by

Jeroen Mahieu

Number 706 2020

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Examination Committee

Supervisor:

Prof. Dr. Francesca Melillo	SKEMA Business School

Co-supervisor:

Prof. Dr. Bart Van Looy	KU Leuven

External Members:

Prof. Dr. Toke Reichstein	Copenhagen Business School
Prof. Dr. Peter Thompson	Georgia Institute of Technology

Internal Members:

Prof. Dr. Dirk Czarnitzki	KU Leuven
Prof. Dr. Stijn Kelchtermans	KU Leuven

Chair:

Prof. Dr. Vincent Molly	KU Leuven

Acknowledgements

"It ain't what you don't know that gets you into trouble. It's what you know for sure that just ain't so."

– Mark Twain

"[...] In the awareness that we can always be wrong, and therefore ready at any moment to change direction if a new track appears; but knowing also that if we are good enough we will get it right and will find what we are seeking. This is the nature of science."

– Carlo Rovelli, *Seven Brief Lessons on Physics*

At the beginning of my PhD I read a short essay by Martin Schwartz titled "The importance of stupidity in scientific research".[1] Although the essay is only one page long, it had a profound impact on my work and my development as a researcher during the last five years. Its thesis is quite simple: in order to do good research - never mind *important* research - you need to understand how to be 'productively stupid'. Productive stupidity means being ignorant by choice. Unlike taking an exam with right and wrong answers, doing research is an immersion in the unknown where nobody knows the answer. Worse, often we don't even know whether we are asking the right *question*. Science makes you feel stupid. All the time. However, if nobody knows the solution, the best course of action is to muddle through as best as we can.

As the opening quote by Carlo Rovelli already highlights, this means that one of the beautiful things about science is that it allows us to bumble along,

[1] Schwartz, M. A. (2008). The importance of stupidity in scientific research. *Journal of Cell Science*, 121: 1771.

getting it wrong time after time, and feel perfectly fine as long as we learn something each time. However, it can be discouraging at times as well, with individual research projects taking several years to complete, disappointing results after months of hard work, or discovering you got scooped by another paper. Luckily, I have been surrounded by many amazing and bright people who share the joy of doing research, and who helped me through the moments of discouragement. I owe great debts for their guidance, support, and contributions to the work presented in this dissertation.

First and foremost, I will always be indebted to my supervisor, Francesca Melillo. Francesca, I think I will never understand what made you believe that I, a student with average grades and a mediocre master's thesis, would become a good researcher. But I'm sure that without your guidance and help I would not have been nearly halfway than where I'm now. I remember vividly our hours-long brainstorm sessions where we discussed new results, addressed reviewer comments, or came up with crazy new theories until our brains almost literally started melting, or one of us discovered that (s)he needed to urgently go to a meeting that had started one hour earlier. I'm pretty sure many other graduate student can only dream of the amount of time you invested in training me how to become a better researcher, even though you were on the tenure-track yourself and very busy with many other things. Even after you left to SKEMA, and our meetings became more sparse, you constantly kept on checking how I was progressing, and were a great support when I was applying for jobs. Besides your infectious passion for research, you are also a very warm personality who always managed to brighten up the sometimes quite empty professors' hallway at MSI. I really hope we can enjoy many Spritz in the future in Amsterdam or Brussels.

Next, I would like to thank my co-supervisor Bart Van Looy. I got to know Bart as a dedicated department chair, a terrific goalkeeper at the annual FEB soccer tournament, and acclaimed DJ at the MSI event. Over time, I learned he is also a very driven academic, who constantly questions the state of the art, regardless of the fame of the author or the journal where it got published. One example of this is when we co-taught several classes of The Foundations of Entrepreneurship. Typically, I would teach the first part discussing a well-known paper in the field after which Bart would take over. When I was done, Bart would come to the front of the classroom and ask the students in all seriousness: "But do you believe any of this?" Bart, I'm very grateful you agreed to join my committee, and for all the critical questions that pushed me to think deeper, which resulted in significantly improved versions of the chapters of this dissertation.

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I warmly thank my colleagues at MSI and AFI, of whom many turned into good friends. When I joined MSI, I found a group of dedicated, hard-working people who also knew when it was time to close the laptop and have a good time. The Friday afterwork drinks at STUK, the legendary MSI events, the MSI weekend getaways, the Tour de France, the chicken Fridays, the endless discussions behind a pint of Stella in Stapleton's, they are only a small fraction of the joyous occasions that have made the last five years some of the best of my life. I hope future PhD students at MSI can experience the same friendly and welcoming atmosphere as I did. Special thanks go to Adrián, Dennis, Nuria, and Thomas. Whenever I didn't know the solution to a problem I had been struggling with for days, or got frustrated with academia in general, I knew I could knock on your door (also because, like me, you often spent a considerable amount of the evening in the office, long after everyone else had already left). Besides research, we could also just talk for hours about the most diverse topics in de Blauwe Schuit, at a BBQ at the penthouse, or just with a coffee in the sun on the fourth floor of HOG. You guys are truly some of the smartest and at the same time warmest people I know, and I'm glad to call you my friends.

To Alain, Dries, Kieran, Matthias, Zoë: many have claimed the title of "best office", but we know there is only one, and that is HOG 03.99. I really enjoyed the discussions on entrepreneurship far away from the innovation folks, but also "het fout uur" on Fridays, the questionable business idea pitches, or the waffles from the market. To José: thanks for awesome times at the penthouse, and for reminding me of my baldness on a regular basis. Also thanks to the great people of the SI department at CBS for making my time in Copenhagen an absolutely unforgettable experience.

Outside work, many friends made my life a very pleasant one, mainly by making me stop thinking about research for a while. In particular, I really enjoyed the various trips to the Ardennes, the holidays in France, Tanzania, the unforgettable canoe trip in Sweden, or the winter retreat in Finland with my good friends from Mechelen. Also many thanks to Niel and Luna for managing to live together with a workaholic academic for two years, and for cooking delicious and healthy meals on the many evenings I came home late from work.

To my brothers: sorry for talking way too much about research and work at family dinners during the last couple of years. Your feets-on-the-ground attitude towards all this highbrow chatter has been a refreshing intermezzo. Finally, I wish to express my immense gratitude to my parents for their unconditional love and support. You probably didn't image your eldest son to grow up as an economist with a keen interest in statistics and econometrics. Even so, you have always provided me with all the opportunities to do what I wanted to do. It is impossible to thank you enough for all the support you have given me throughout my life. Thank you for everything.

Jeroen Mahieu
Muizen, July 2020

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General Introduction

Motivation

Economists since Cantillon (1755) have stressed the central role entrepreneurs play in the economy. Although the nature of their activities is multifaceted, ranging from the destabilizing innovators of Schumpeter (1934) to the alert arbitrageurs of Kirzner (1973), empirical evidence from various countries and time periods has shown that entrepreneurs and new ventures are important contributors to the introduction of new products and services (Acemoglu et al., 2018; Akcigit and Kerr, 2018)[2], facilitate knowledge spillovers by commercializing ideas that evolved from an incumbent organization (Audretsch et al., 2006, 2008), and disproportionally contribute to both total and net job creation (Haltiwanger et al., 2013; Criscuolo et al., 2014).

This empirical evidence of substantial social returns to entrepreneurial activity has motivated policy makers to encourage more individuals to start new businesses. For example, the Entrepeneurship 2020 Action Plan of the European Commission states that “To bring Europe back to growth and higher levels of employment, Europe needs more entrepreneurs [...] The principle of “think small first” must become the touchstone of European and national policies.” (European Commission, 2013, p.3).

The simple but critical assumption underlying these policies is that economic growth is monotonically increasing in the rate of new firm formation.

[2]The relative importance of entrepreneurship and new ventures vis-à-vis established businesses to innovation and growth is a topic of ongoing debate in the innovation literature, and conclusions drawn from data are highly sensitive to model assumptions (cf. Garcia-Macia et al., 2019). However, most studies agree that both entrants and incumbent firms play a non-negligible role.

Is this necessarily true? Recent advances in the literature paint a more complex picture. For example, a growing body of work documents that there are at least two fundamentally different types of entrepreneurs: First, “opportunity entrepreneurs” who aim to create large, vibrant businesses, that provide jobs and income for others. Second, “necessity entrepreneurs” who become self-employed mostly as a means of providing subsistence income for a job in paid employment (e.g. Schoar, 2010; Levine and Rubinstein, 2017; Fairlie and Fossen, 2019).[3] These findings suggest that growth not only correlates with the rate but also with the type of entrepreneurship that prevails in the economy. Relatedly, various studies have documented that, on average, entrepreneurs earn less than workers in wage work, although they face significantly higher income risk (cf. Åstebro and Chen, 2014, for an overview). Again, this seems to suggest that a substantial fraction of all entrepreneurs contribute little to the economy.

These findings have successfully shifted the interest of researchers and policy makers to the drivers of the quality of entrepreneurship, rather than quantity per se (Guzman and Stern, 2016). By now, scholars of this subject have agreed that a better understanding of the mechanisms underlying entrepreneurship and entrepreneurial performance (including the number of jobs created by new firms) require an integration of entrepreneurship into to existing theoretical models of occupational choice and firm dynamics. In particular, recent studies have highlighted that the decision to become an entrepreneur is typically a specific instance of a more general career mobility process in which individuals’ decision can be understood as a trade-off between entrepreneurship and alternative employment options (e.g. Astebro et al., 2011; Burton et al., 2016; Elfenbein et al., 2010; Campbell et al., 2012; Sørensen and Sharkey, 2014; Choi et al., 2019). Models of firm dynamics also indicate that firm entry growth is influenced by aggregate or firm-specific productivity shocks, the costs involved in hiring, retaining and managing workers, as well as by regulations imposed by the institutional context (Hopenhayn, 1992; Clementi and Palazzo, 2016).

[3] In the literature, these two types have received multiple labels. For example, Schoar (2010) labels them “transformative” and “subsistence”entrepreneurs, Levine and Rubinstein (2017) distinguish between “entrepreneurs” and “other business owners”. These different labels represent roughly similar concepts.

The aim of this dissertation is to extend prior work within these streams of research by asking how and why:

1. A spell of entrepreneurship relates to an individual's future earnings trajectory in the labor market

2. Local demand shocks affect job creation through new firm formation

Before turning to an overview of the specific literatures on the private and social labor market returns to entrepreneurship that motivate the chapters to follow, I will first provide the definition of entrepreneurship used in this dissertation and the theoretical lens that will guide the empirical analyses.

In line with most of the literature in this area, I define entrepreneurship as self-employment (Chapters 1 and 2) and new venture creation (Chapter 3) (Parker, 2009). This implies that I make no distinction between individuals based on the role they occupy within an organization, nor do I solely focus on new ventures involved in innovative activities, in the high-tech sector, or that display above average financial performance. However, this does not imply that I assume that the self-employed are a homogeneous group, consistent with the evidence of the existence of different types of entrepreneurs (cf. above). In fact, I exploit heterogeneity among entrepreneurs in terms of their background, reasons to enter entrepreneurship, and experiences during entrepreneurship to test several theoretical predictions in the different chapters.

The theoretical foundations of the different chapters are grounded in labor economics approaches to understand job creation and the structure of wages and earnings. This means that I assume that individuals' behavior is determined to a large extent by comparing income streams across different employment settings, including self-employment. This also implies that I assume individuals act without taking into account their relationships with others, unlike sociological approaches to labor markets that emphasize the influence of social networks and demographic constraints. I also abstract away from psychological approaches emphasizing stable traits to explain career choices.

The private returns to entrepreneurship

In light of the observed contributions of entrepreneurial activity to the economy, it is not surprising to see that the majority of labor market studies treating entrepreneurship as an occupational choice have focused on the questions why and when individuals become entrepreneurs. Early work on this topic examined how heterogeneity in managerial abilities (Lucas, 1978), risk aversion (Kihlstrom and Laffont, 1979), or borrowing constraints (Evans and Jovanovic, 1989) could explain why some individuals are more likely to select into self-employment. From a human capital perspective, the nature of entrepreneurial activity may also require individuals to invest in a set of skills that maximizes their income in self-employment at the cost of foregoing earnings in other employment settings. A seminal paper within this strand of the literature is Lazear (2005) who argues that entrepreneurs need to be "jacks-of-all-trades" with a balanced set of skills, while traditional wage employment rewards specialization in a small number of skills. The entrepreneurial choice may also be driven by the characteristics of a person's current employment setting. For example, Elfenbein et al. (2010) show that entrepreneurs are more likely to come from small firms, which may partly reflect differences in access to resources and skill development opportunities between small and large firms. However, it may also be due to large firms providing more internal opportunities for advancement (Kacperczyk and Marx, 2016).

These theories of entrepreneurship all rely on a standard framework of expected utility theory where individuals are assumed to choose between entrepreneurship and some outside option (a job in wage employment); and they choose the occupation that offers them the highest expected utility[4]. However, at first sight, this seems to be inconsistent with a substantial body of research documenting that despite working longer hours and bearing higher income risk, the median self-employed earns less than his salaried counterpart (cf. Evans and Leighton, 1989; Carrington et al., 1996; Hamilton, 2000;

[4]This requires another assumption, namely that the occupational choice is discrete, meaning that each activity requires full-time involvement. This is not necessarily true for a large fraction of entrepreneurs (Folta et al., 2010) I discuss simultaneous employment in wage work and self-employment ("hybrid entrepreneurship") in Chapter 2.

Moskowitz and Vissing-Jørgensen, 2002, for early examples).

Over the last two decades, various studies have come up with potential explanations for this so-called “returns-to-entrepreneurship-puzzle”. One interpretation is that entrepreneurs possess different preferences or traits; for example, entrepreneurship may provide more autonomy than regular paid employment, attracting individuals who value more non-pecuniary benefits such as being one’s own boss (Hamilton, 2000; Hurst and Pugsley, 2011). Or, entrepreneurs are simply overconfident (Åstebro et al., 2014). Perhaps, treating the self-employed as a homogeneous group masks important heterogeneity, by making “little distinction between Michael Bloomberg and a hotdog vendor.” (Glaeser, 2007); indeed, when disaggregating the self-employed based on the incorporation status of their venture, the evidence shows that the median incorporated self-employed person earns more than the median employee, while the median unincorporated self-employed earns significantly less (Levine and Rubinstein, 2017). This is consistent with the notion that the self-employed are drawn from the tails of the ability distribution (Elfenbein et al., 2010; Astebro et al., 2011; Levine and Rubinstein, 2018). It is also possible that the measured differences reflect in part underreporting of earnings by the self-employed (Åstebro and Chen, 2014). Recent theoretical and empirical advances point out that cross-sectional earnings differences may also underestimate the returns to entrepreneurship for another reason. Because these studies do not take into account the possibility that entrepreneurship may hold experimentation value if entrepreneurs have the option to return to wage work, the expected lifetime earnings from entry into entrepreneurship may be much more attractive than estimates from cross-sectional earnings data suggest (Vereshchagina and Hopenhayn, 2009; Manso, 2016; Dillon and Stanton, 2017).

Entrepreneurs as employers

Empirical evidence (Criscuolo et al., 2014; Haltiwanger et al., 2013, 2017) indicates that startups, and in particular high-growth new ventures, account for a disproportionate share of total and net job creation. For example, Haltiwanger et al. (2013) find that while startups account for only 3 percent of overall employment, they are responsible for almost 20 percent of US gross job creation. High-growth ventures (which are disproportionally young) account for about 50 percent of gross job creation, and, conditional on survival, young ventures have significantly higher growth rates than more mature firms. These findings resonate well with results from other papers that a small number of fast growing businesses account for most of the job creation by newcomer firms (Brüderl and Preisendörfer, 2000; Acs and Mueller, 2008).

At the micro-lecvel, the evidence shows that startups account for most of the employment growth following positive shocks to local demand. (Adelino et al., 2017), for example, exploit regional variation in income shocks in the manufacturing sector to see how changes in local demand affect job creation in the nontradable sector. They find that firm entry accounts for almost all of the net employment creation following local demand shocks. Similarly, (Decker et al., 2017) find that startups account for nearly all job creation in regions that experienced a boom in the exploration and production of shale oil and gas.

Contributions

While the above sections highlight the progress the literature has made in fostering our understanding of the private returns to entrepreneurship and the social returns in terms of the jobs new ventures create, previous work has overlooked several important aspects to these questions. I briefly explain these gaps in the literature, and how the different chapters contribute to the body of work around these topics.

Regarding the returns to entrepreneurship, most studies have focused on the earnings *during* entrepreneurship, ignoring how a spell of entrepreneurship may affect entrepreneurs' wages in a future job in wage work. This is unfortunate, given that estimates indicate that 40 to 50 percent of all workers who enter self-employment returns to wage work within five years (Kaiser and Malchow-Møller, 2011; Dillon and Stanton, 2017). Furthermore, studies measuring the lifetime returns to entrepreneurship implicitly or explicitly assume that entrepreneurial experience does not impact future wages in the paid sector[5]. In fact, empirical evidence shows a wage penalty for former entrepreneurs, at least at the time of re-entry into wage work (Kaiser and Malchow-Møller, 2011; Baptista et al., 2012; Failla et al., 2017), contradicting the assumption of no switching costs between entrepreneurship and wage work. However, explanations for this observed penalty are under-theorized and mostly suggestive.

Chapter 1 contributes to this literature by laying out a novel theory for why entrepreneurs are penalized when they return to wage work. We argue that entrepreneurship is a noisy signal of ability, leading employers to discount former entrepreneurs' wages due to the increased uncertainty about their future productivity. We provide empirical evidence in support of this theory, and against several alternative explanations. To the best of our knowledge this is the first study to offer a theory backed by empirical evidence for why former

[5]For example, Dillon and Stanton (2017) simply assume a zero relationship between a past spell of entrepreneurship and future wages on the basis of difficulties of integrating non-random selection in their model of occupational choice: "[...] we find small and imprecise returns to entrepreneurial experience after various attempts to control for non-random selection between sectors. Because of the difficulty of fully accounting for this selection, in the current specification we impose that entrepreneurial experience has no effect on paid sector earnings." (p. 64)

entrepreneurs are penalized.

Chapter 2 builds upon the empirical findings of Chapter 1, and explores if the observed wage penalty is only temporary, or whether it persists over time. To the best of our knowledge, only Manso (2016) has examined this question for the US, a labor market characterized by high rates of job mobility and wage flexibility. The empirical findings show significant persistent wage losses for former entrepreneurs in Belgium, which are split up by a reduction in hours worked and a reduction in the real wage. We find that former entrepreneurs are more likely to start part-time or temporary jobs, and to change jobs. These factors explain much of the penalty in terms of hours worked per quarter but none of the daily wage loss. We analyse several different potential explanations for the daily wage loss, but none of them can fully explain the wage gap. This is the persistent wage penalty puzzle.

Regarding job creation in startups, the literature has mainly highlighted so far that job creation through new venture formation appears to respond disproportionally to local economic shocks. Much less is known about the underlying mechanisms. On the one hand, characteristics tied to the entrepreneur may play a role. For example, Bernstein et al. (2018) find that mainly young individuals start up a new venture when new local opportunities occur. This may reflect greater willingness to take entrepreneurial risks among the young (Lévesque and Minniti, 2006). On the other hand, characteristics of established firms, such as bureaucratic inflexibility, are thought to contribute. However, convincing empirical evidence for any of these explanations is currently lacking.

Chapter 3 contributes to this literature by examining which firms create jobs in sectors associated with rebuilding and recovery activities after a region is hit by a natural hazard. In particular, I examine whether job creation occurs mainly through new venture formation or through expansion of established businesses. In line with previous studies, I find that startups contribute disproportionally to net employment growth, whereas old firms create less jobs than their share of total employment would predict. I interpret these findings in a rent-sharing framework, and provide additional evidence consistent with the idea that job creation in established businesses following local economic shocks is muted because part of the increased profits are passed through to incumbent workers.

A short summary of each chapter is provided in the next section.

Outline of the Dissertation

Chapter 1 – *Shooting Stars? Uncertainty in Hiring Entrepreneurs*

Recent empirical studies have found that a large share of entrepreneurs return to wage work after several years, but that, on average, these individual appear to receive a lower wage than similar employees without entrepreneurial experience. So far, some scholars have interpreted this as evidence for the notion that a spell of entrepreneurship is a negative signal of ability in the labor market, similar to a stigma of failure. Others have argued that entrepreneurs may acquire less valuable human capital than workers in paid employment. Yet, empirical evidence that favors one of these interpretations is scarce. Chapter 1 starts by putting forward a novel theory for the observed wage penalty, namely that a spell of entrepreneurship is not a negative but a *noisy* signal of ability. The noisiness of the signal increases the uncertainty about an individual's future productivity. Because it is costly for employers to hire new employees, and to fire them if they turn out to be bad (unproductive) hires, they will respond to this increased uncertainty by offering a lower wage to former entrepreneurs compared to similar employees without entrepreneurial experience. We provide several pieces of empirical evidence confirming our theoretical predictions, using a novel matched sample of entrepreneurs and employees from Belgium. Furthermore, we discuss several alternative explanations for our findings, and provide additional evidence inconsistent with these explanations. The key takeaways are that, in contrast to assumptions made by earlier studies, there appear to be significant switching costs involved in moving from self-employment to wage work, and these costs are dependent on a person's experience prior to becoming entrepreneur, and by future employer attributes. Our findings also caution against policies promoting entrepreneurship as a valuable route of experimentation, regardless of a person's background.

Chapter 2 – *The Wage Persistence Puzzle: Earnings Trajectories of Former Entrepreneurs*

While the focus in Chapter 1 is on wage differences between former entrepreneurs and non-entrepreneurs at the time the entrepreneur re-enters wage work, Chapter 2 examines whether these initial differences fade away over time in the labor market or whether they are persistent. Using the same data source as in Chapter 1, we start by estimating the short- and medium-term wage differences between entrepreneurs who return to wage work, and their matched counterparts up to five years after entrepreneurship. We find robust evidence showing that the initial wage differences persist years after the entrepreneur has returned to paid employment, without a sign of a catching up effect. These long-term losses are split between a penalty in terms of hours worked per quarter (60 percent of the penalty) and a penalty in terms of a lower daily wage (40 percent of the penalty). We find that former entrepreneurs are more likely to start part-time or temporary jobs, and to change jobs. These factors explain much of the penalty in terms of hours worked per quarter but none of the daily wage loss. Having documented the persistent wage penalty, we explore several potential explanations which we interpret as resulting from market frictions on the one hand, and from signaling problems on the other. However, none of these candidate explanations can fully account for the observed wage gap.

Chapter 3 – *Picking Up The Pieces: Natural Disasters, Firm Dynamics, and the Demand for Reconstruction*

In Chapter 3, I examine which types of firms create jobs to address the increased demand for reconstruction and recovery following a natural hazard. In particular, I examine whether new or existing firms are more likely to contribute to net employment growth in sectors associated with rebuilding. To do so, I use county-level data for the North Atlantic Basin area on employment by firm age and sector that span all quarters in the period between 2000-2015 and estimate how employment changes in the years following a hurricane strike. I estimate that employment in startups increases significantly in the first four years after a hurricane, with a peak of almost 24% above expected levels after one to two years. This implies that job creation through new venture formation accounts for nearly 23% of excess total job creation in these sectors following a hurricane. Given that startups on average account for only 4% of total employment, this shows that startups disproportionally respond to local demand shocks. I do observe an increase in firms aged 6 years or older as well, but the estimated increase is much smaller than their share of total employment would predict. In line with theories of rent-sharing in which part of profit rises pass-through to incumbent workers, the results from analyses on the average monthly earnings of workers show that wages in established businesses rise following a hurricane, unlike those of startup employees which remain similar to their pre-hurricane levels. This can explain the observed disproportionate responsiveness of startups.

Chapter 1

Shooting Stars? Uncertainty in Hiring Entrepreneurs*

1.1 Introduction

A fundamental question at the heart of entrepreneurship is why individuals become entrepreneurs. A substantial volume of research recognizes that this choice is the outcome of a trade-off between opportunity costs and the expected returns from entrepreneurship, which include both pecuniary and non-pecuniary benefits (Fairlie, 2002; Hamilton, 2000). More recently, scholars and policy makers have viewed entrepreneurship also as a way to experiment with ideas and career options (e.g., Polkovnichenko, 2003; Kerr et al., 2014; Manso, 2016). The implicit assumption is that in case of failure or mismatch with a person's own abilities and preferences, it is always possible for the entrepreneur to easily quit and find a job back in the wage sector. Indeed, moves between paid employment and entrepreneurship are rather common, and most entrepreneurs return to the wage sector within a few years (e.g., Kaiser and

* This chapter is based on the forthcoming paper: Mahieu, J., Melillo, F., Reichstein, T., & Thompson, P. (2019). Shooting stars? Uncertainty in hiring entrepreneurs. *Strategic Entrepreneurship Journal*. Earlier versions of the chapter have been presented at EPFL Lausanne, DRUID 2018, the 2018 annual meeting of the Academy of Management, the 2018 ZEW Conference on the Dynamics of Entrepreneurship, and the 2019 SMS special conference in Frankfurt.

Malchow-Møller, 2011; Dillon and Stanton, 2017). However, for individuals contemplating founding a company to experiment with ideas, knowing whether their future salary will be jeopardized upon returning to the wage sector is valuable.

Recent research has begun to document the consequences of such entrepreneurial attempts. Apart from a few exceptions (Hyytinen and Rouvinen, 2008; Luzzi and Sasson, 2016; Manso, 2016), existing evidence indicates that entrepreneurs, on average, receive a pay cut when rejoining the labor market immediately after a spell of entrepreneurship (see e.g., Bruce and Schuetze, 2004; Baptista et al., 2012; Failla et al., 2017; Kaiser and Malchow-Møller, 2011). This evidence suggests that labor market frictions could negatively affect the value of experimenting with entrepreneurship (Aksaray and Thompson, 2017; Gottlieb et al., 2016).

However, little is known about the factors driving this adverse treatment. A few scholars suggest that employers discount entrepreneurs because their experience may signal skills or preferences that do not fit with the wage employment context. Furthermore, exiting entrepreneurship may signal firm failure and, in turn, poor individual qualities, given the strong expected association between founder quality and start-up performance (Bruce and Schuetze, 2004; Kaiser and Malchow-Møller, 2011; Koellinger et al., 2015). Overall, the underlying assumption is that entrepreneurial experience holds a signaling value and that this value is negative. Yet, empirical support in favor of these mechanisms remains limited.

This paper advances a theory to explain why entrepreneurs are penalized. In developing this theory, we were motivated by recent intriguing insights from labor economics on the role of uncertainty in hiring (Kuhnen and Oyer, 2016). In contrast to previous work, we posit that a (short) spell of entrepreneurship holds a low rather than a high signaling value. The noisiness of the signal increases the uncertainty about an individual's future productivity. Employers respond to this uncertainty in recruitment by offering lower wages to entrepreneurs relative to equivalent employees without entrepreneurial experience. To offer empirical support for this uncertainty-based mechanism, we consider how the degree and importance of uncertainty in the hiring process affects the size of the wage penalty. We predict that the penalty is stronger for entrepreneurs (i) with previous high-ability signals, (ii) who exited quickly, and (iii) who are hired by firms that bear higher firing and replacement costs.

We test our claims by using a newly developed ad hoc longitudinal linked employer–employee dataset from the Belgian Datawarehouse Labor Market and Social Protection, covering all quarters between 2000 and 2015. We construct a matched sample of employees who transition into entrepreneurship and workers who never leave paid employment between the first quarter of 2004 and the last quarter of 2015. The matched group of workers who remain in wage employment serves as a counterfactual example of what would have happened to the entrepreneurs had they not become self-employed. From this control sample, we select the matched pairs in which the entrepreneurs move from a full-time job to entrepreneurship and back, and compare the wages between the two groups immediately before and after the entrepreneurship spell.

We find strong support for our predictions. The wage penalty is more severe for entrepreneurs who come from the upper end of the wage distribution in the quarter before entering entrepreneurship – here referred to as *stars* – whereas there is no penalty for those coming from the lower tail. Stars earn around 16 percent less after a spell of entrepreneurship than their matched pairs who remained in wage employment. We also find that the penalty diminishes as uncertainty resolves or when it is inherently absent. Correspondingly, only entrepreneurs who return to the wage sector relatively quickly – within five years from firm foundation – and who *do not* return to their previous employer appear to be penalized. Finally, we find that stars' penalty is smaller for entrepreneurs who move to a large employer, presumably because uncertainty is more damaging when the cost of firing and replacing unsuccessful hires is high, as is the case for small firms (Kuhnen and Oyer, 2016; Tate and Yang, 2015).

Our results are robust to several robustness checks, in particular, to different counterfactual groups (stayers, movers, and not yet entrepreneurs) and to relaxations of various sample restrictions. In the supplementary section, we also explore a number of possible alternative explanations based on the extant literature, such as human capital depreciation, expected job mismatches, taste-based discrimination, stigma of failure, sorting on non-pecuniary preferences, and labor market frictions. We discuss how these mechanisms yield predictions that are inconsistent with the empirical evidence presented in this paper. Among a number of potential contributing factors, uncertainty appears to be the sole mechanism that is consistent with all the empirical patterns observed in our data.

This study contributes to our understanding of the returns from entrepreneurship. First, we offer a novel explanation for why entrepreneurs returning to the wage sector are adversely treated. Specifically, we develop and test a theory for which a spell in entrepreneurship increases employers' uncertainty about a job applicant's future productivity. Second, we argue and show that entrepreneurs who were star employees are penalized, whereas those who come from the bottom of the ability distribution are not. With this finding, we extend prior work, which implicitly assumes that the returns from entrepreneurship are independent of one's previous career (e.g., Kaiser and Malchow-Møller, 2011). Moreover, we contribute to research on stars and entrepreneurship (Astebro et al., 2011). While prior work proposes entrepreneurship as an attractive career option for stars (e.g., Groysberg et al., 2007), the stronger penalty we find in case of short spells cautions stars against experimenting with entrepreneurship. Third, the finding that large firms penalize star entrepreneurs less seems at odds with theories about the sorting of entrepreneurial types into small firms based on preferences and skills (Elfenbein et al., 2010), and highlights the importance of accounting for heterogeneity in the demand side when looking at exit from entrepreneurship. Large firms may be better equipped in dealing with risky hires. Fourth, our findings suggest that the value of experimenting with new ideas (Manso, 2016) is not equally distributed among entrepreneurs, but it is influenced by their prior status in the labor market and by future employer attributes, similar to how certain employees learn about their fit for a new job by engaging in risky experiments in their current job (Chatterji et al., 2016).

Our evidence supporting the existence of uncertainty in hiring entrepreneurs suggests a reconsideration of the policy focus on reducing the stigma attached to failed entrepreneurs (e.g., Singh et al., 2015) toward measures that mitigate employers' costs of handling uncertainty. Furthermore, it is commonly believed in practice-oriented entrepreneurship communities that a quick exit is desirable if the initial outcomes are negative. Our results suggest that this view is a partial consideration at best and should be called into question. This paper also has implications for managers engaged in the war for talent. Hiring entrepreneurs who were former stars can be seen as a strategic investment decision under uncertainty. From a real options perspective, their lower hiring cost compared to poaching stars from other established firms might be attractive because of their high upside potential, despite the risk of bringing in bad hires. Ultimately, this study cautions against investigating the effect of an entrepreneurial spell on future wages without taking into account previous signals of ability, as employers do not consider the signal of an entrepreneurial experience in isolation but in combination with the signals emerging from the past paid employment experience (Falk and Zimmermann, 2017).

1.2 Conceptual Framework

Labor economists have long recognized that a fundamental problem in hiring is one of matching in the presence of information asymmetries (cf. Oyer and Schaefer, 2011, for an overview). Limited information about the true qualities and effort levels of workers implies that employers must make hiring decisions about a worker's future productivity under uncertainty (Spence, 1973). As employers are unable to discover the true ability of each worker prior to employment, they rely on information gathered from signals that have proven to be effective in predicting workers' productivity. Within the classic Spence (1973) signaling framework, we advance an explanation for the effect of an entrepreneurial spell on the re-entry wage, which focuses on two signals: group identification (Borjas and Goldberg, 1978) and previous salary (e.g., Barach and Horton, 2017; Hall and Krueger, 2012).

1.2.1 Signal noise, entrepreneurship, and wage penalty

Signal noise and entrepreneurship

Consider two comparable groups of potential job applicants. The first group consists of individuals who re-enter the wage sector immediately following a spell of entrepreneurship, i.e., the entrepreneurs, whereas the second group consists of workers without entrepreneurial experience, i.e., the employees. Employers observe a signal of group affiliation – entrepreneurs or employees – as well as signals about individual ability – including the previous wage – by considering the candidates' résumés via job interviews or secondary sources (Barach and Horton, 2017). In our empirical setting, all entrepreneurs have wage employment experience, so we can observe their salary just before the entrepreneurial spell.

This paper postulates that employers are more uncertain about the productivity of entrepreneurs than that of employees. In other words, an intermittent spell of entrepreneurship is less informative about the expected productivity in future wage work than a consecutive career trajectory in paid employment. We develop this intuition by drawing on theories of signal substitution in hiring decisions (Autor and Scarborough, 2008; Agan and Starr, 2018; Barach and Horton, 2017; Bertrand and Mullainathan, 2004). This literature demonstrates that in the absence of one or more pieces of information, employers put more weight on other correlated signals in their screening process.

A substantial volume of labor economics literature shows that employers rely extensively on available information about past employment to assess job candidates' fit (e.g., Altonji and Pierret, 2001; Greenwald, 1986; Pallais, 2014; Schönberg, 2007; Waldman, 1984). Among the available information, the previous wage is considered as a strong signal of future productivity, especially if the prior job is similar to the one offered (e.g., Barach and Horton, 2017). Retrieving this information is a common human resources (HR) practice. For example, Hall and Krueger (2012) show that 47 percent of workers reported that their employers had learned about their pay in their earlier jobs before making the offer. Another prominent signal in this literature is past promotions (Waldman, 1984). As the actual productivity of an employee is typically unobservable to the prospective employer, a promotion reveals to outside firms a positive signal about worker ability (Gibbons and Katz, 1991; DeVaro and Waldman, 2012; Moallemi et al., 2017). Finally, firms can also directly discern the ability of a new hire by contacting the listed references from past employers (Ioannides and Loury, 2004). In fact, a recent study finds that including a reference letter from a previous boss increases call-backs by more than 60 percent (Abel et al., 2017).

These conventional signals of ability are clearly less available to employers when they evaluate entrepreneurial experience. Therefore, we suggest that when hiring entrepreneurs, employers use substitute signals related to start-up performance, and that these signals are less accurate in predicting future productivity in the wage sector than the signals associated with regular employment experience.

More often than not, the entrepreneur will act as his/her own most suitable reference, as the employer and the employee are collapsed into the single entity of the founder (Lazear, 1981). Therefore, the retrieved information is likely to be biased. While firms can infer the previous wage from secondary independent sources, information about the earnings of an entrepreneur are typically self-reported. Moreover, compared with wages, entrepreneurial earnings are less likely to be correlated with productivity. The entrepreneur's decision on how much earnings to withdraw for himself/herself or whether to re-invest in the company is complex and follows a different logic compared with wage determination in established firms (e.g., Wasserman, 2012). Another piece of information that is likely to be biased is the reason for returning to the wage sector because founders tend to under-report failure or blame the external environment for it (Eggers and Song, 2015). Finally, unlike being assigned to high-level, high-quality jobs in established firms (DeVaro and Waldman, 2012; Waldman, 1984), taking on the founder role does not serve as a credible signal of ability, as individuals may become entrepreneurs for reasons other than economic rewards (Hamilton, 2000).

Signals of ability derived from an entrepreneurial experience may not only be noisier compared with those in the wage sector but also costlier to obtain. While the hiring firms may increase the accuracy of the signal by engaging in a costly search (Barron et al., 1985) to collect detailed information about the start-up and the team, they are often unwilling to do so. For example, Cahn et al. (2017) find that banks that can easily reconstruct information about start-up failure based on public bankruptcy files typically do not do so. Furthermore, directly observing the financial performance of an entrepreneurial venture is difficult for employers. While the yearly financial balance sheets are publicly available, only incorporated ventures (a minority of the start-up population) are required to publish them, often in a reduced form, and typically several months after the closure of the accounting year. Absent data on performance, employers may look at signals correlated with performance, such as the attraction of external financing (Chatterji, 2009), firm size (Elfenbein et al., 2010), and whether the firm is incorporated (Levine and Rubinstein, 2017). Yet, the presence of confounding factors, such as personal social networks, may reduce their signaling value.

Even if firms obtained credible information on start-up performance, evaluating the contribution of the entrepreneur to the performance of his/her venture remains difficult for employers. This is especially the case if the venture is co-founded or has early employees. Moreover, there may be exogenous (industry or macroeconomic) shocks that affect firm performance (e.g., Cahn et al., 2017) and that are therefore independent of founder ability. Lastly, many start-ups fail because of the poor quality of the identified business opportunity (Ries, 2011), which may be largely unrelated to the expected productivity of the founder in the wage sector.

In conclusion, fewer and less-accurate signals characterize the hiring of entrepreneurs. Therefore, we conjecture that *ceteris paribus*, a spell of entrepreneurship increases the uncertainty that employers face when assessing a new hire's future productivity.

Signal noise and wage penalty

Uncertainty about an entrepreneur's future productivity may have detrimental effects on his/her competitiveness in the labor market. Labor economics research has documented the role of uncertainty in corporate hiring, finding that employers tend to be averse to such hires (Hendricks et al., 2001) and that uncertainty around a candidate's expected productivity reduces the odds of being hired (Kuhnen and Oyer, 2016). We propose an additional implication of uncertainty: conditional on hiring, uncertainty discounts the offered wage. Therefore, entrepreneurs – the riskier hires – receive a wage penalty upon re-entering the labor market compared with observationally similar employees. This prediction is consistent with ample empirical evidence (e.g., Baptista et al., 2012; Failla et al., 2017; Kaiser and Malchow-Møller, 2011).

The main mechanism through which higher uncertainty can induce a pay cut is that employers will demand compensation for uncertainty in case the entrepreneur turns out to be a bad hire. The greater signal noise associated with the entrepreneurial experience increases the risk for the employer that the entrepreneur will not have sufficient ability or commitment to be productive in the assigned job task, resulting in a job mismatch (Jovanovic, 1979). The underlying assumption is that employers are concerned with uncertainty about the expected quality of a job match because it is costly for them to fire and replace an (unproductive) employee (e.g., Pfann, 2006; Serfling, 2016).

This assumption appears to be reasonable in light of the setting we choose for the empirical analysis. We use data from the Belgian labor market, which is characterized by strong employment protection institutions. For example, an employer needs to pay a fired employee a dismissal compensation equivalent to a three-month salary for each five years of tenure. Moreover, to strengthen the soundness of the assumption, we will restrict our sample to full-time permanent jobs. In the instance of a temporary job, the cost of a bad hire is much lower because the employer can simply choose not to continue the employment relation (Kuhnen and Oyer, 2016; Lazear, 1995).

Why, then, do firms hire candidates with an uncertain quality? Uncertainty about entrepreneurs' future productivity can be beneficial because it holds option value (Hendricks et al., 2001; Lazear, 1995). If the candidate turns out to be a good match, then offering a lower wage to former entrepreneurs is an attractive opportunity for firms to hire productive employees at a discounted rate. Entrepreneurs may be willing to accept a lower offer, fearing the possibility of unemployment and its long-term related consequences (Jacobson et al., 1993). Therefore, hiring entrepreneurs can be interpreted through a real option lens (Dixit, 1992) in which uncertainty may limit the amount of investments that are sunk, i.e., the offered wage, while allowing for learning about the upside potential.

1.2.2 Size of the penalty

We have claimed that uncertainty about future productivity explains the negative effect of an entrepreneurial experience on the re-entry wage. One way to test support for this claim is to consider whether the factors that predict variation across individuals and firms in the importance of uncertainty also predict variation in the size of the wage penalty. We draw on the extant labor economics literature to identify two sources of heterogeneity in uncertainty: an employee's position in the overall wage distribution and the time spent in entrepreneurship. Moreover, assuming that the importance of uncertainty depends on the cost of a poor match (Kuhnen and Oyer, 2016), we consider employer size as a source of firm-level variation in the costs of replacing a bad hire.

Rank in the wage distribution

We consider ability as one source of heterogeneity in the entrepreneurs' group with regard to the level of uncertainty faced by employers. Empirically, we will proxy ability by using individual position (rank) in the unconditional wage distribution immediately before the entrepreneurial spell. In line with previous research on entrepreneurship, we will distinguish between entrepreneurs who come from the top of the wage distribution, which we refer to as star employees, and those who come from the bottom (e.g., Astebro et al., 2011; Elfenbein et al., 2010; Groysberg et al., 2007). Using the previous wage as a proxy for ability is consistent with our signaling theoretical framework in which employers rely on previous wage as a signal of ability. We propose that the wage penalty is more severe for entrepreneurs who were high-ability employees.

First, employers have more difficulties in efficiently estimating the productivity of individuals with extreme values of the ability signal (Hendricks et al., 2001) because employers need to base their evaluations of these applicants on a smaller sample size than that for employees with moderate values of the ability signal. This is a particular disadvantage for entrepreneurs with a high signal, as the spell of entrepreneurship weakens the information value of their signal, whereas it is an advantage for entrepreneurs with a low-ability signal. This aversion discount could cause employers to prefer employee candidates with low-ability signal values (Hendricks et al., 2001).

Second, star entrepreneurs face higher opportunity costs when starting because of the higher wages and employment-related benefits they need to forgo (Amit and Cockburn, 1995; Campbell et al., 2012). These higher opportunity costs raise the threshold of performance required to induce a star employee to remain as an entrepreneur. Despite this, evidence that the entrepreneur returns (quickly) to the wage sector may induce the labor market to discount competing evidence about the founder's ability, including evidence from pre-entrepreneurship earnings.

Third, star employees tend to hold high-level jobs compared with employees who are lower ranked in the wage distribution. Yet, hiring entrepreneurs in high-level jobs may increase the cost of uncertainty because the minimum ability needed to perform effectively and the cost to the firm of ineffective performance are both greater in higher-ranked jobs (Pfann, 2006). At the highest level, an ineffective CEO can be devastating for a firm. It is also generally true that an ineffective engineer or accountant causes more damage than does an ineffective production worker. This remains true even if one can dismiss ineffective workers because turnover costs are significantly higher at higher ranks and discerning that higher-ranked workers are performing ineffectively takes more time.

In short, the empirical prediction we derive from these arguments is that the wage penalty is more severe for entrepreneurs whose previous wage is highly ranked in the overall wage distribution.

Length of the entrepreneurial spell

Another source of variation in the uncertainty perceived by hiring firms is the length of the entrepreneurial spell. We argue that uncertainty resolves over time in entrepreneurship for two reasons.

First, entrepreneurship-related signals become more accurate with time. Compared with longer spells, quick exits are less informative about the ability of the founder. Whether a quick exit is the result of low-ability individuals selecting out of entrepreneurship or of external economic shocks is difficult to disentangle, as entrepreneurs operating younger and smaller firms are more vulnerable to unanticipated negative shocks (Freeman et al., 1983). A quick exit may also reflect an entrepreneur learning about his/her preferences rather than his/her ability (Manso, 2016), or it may be caused by the poor quality of the business opportunity, which is largely unrelated to ability as an employee.

Second, entrepreneurship-related signals become more available over time. In the previous section, we argued that uncertainty depends not only on the noise of the signal but also on the availability of signals. Accordingly, we argue that over time, firms can make more informed choices without incurring additional search costs. For example, while little information exists about performance in the start-up phase, entrepreneurs who have survived for several years can provide more detailed sales and track records. Moreover, the likelihood that signals associated with firm performance, such as awards, external financing, or endorsements from prominent actors, become available increases over time (e.g., Stuart et al., 1999; Stinchcombe, 1965). If the noise obscuring ability is random, as is usually assumed, the mean of signals converges on the true ability as more signals are observed.

As a consequence, the correlation between the observed outcomes and ability is higher the longer the period over which the outcome is averaged. More importantly, the difference in precision between a high-noise signal (entrepreneurial experience) and a low-noise one (employment experience) becomes smaller the longer the time over which the outcome is averaged. Therefore, we expect the wage penalty to decrease with the duration of the entrepreneurial spell.

Employer size

We expect firms to be less concerned about the uncertainty of a candidate's fit in situations where the cost of replacing an employee who does not meet the performance expectations is lower (Kuhnen and Oyer, 2016). Therefore, we propose the effect of uncertainty on the wage penalty to be more pronounced when the entrepreneur is hired by firms that bear higher firing and replacement costs. While there might be several sources of firms' heterogeneity in the cost of non-performance, as we proceed empirically, we will exploit variation in the size of the new employer. There are several reasons why we would expect that larger firms bear lower costs. First, larger firms incur lower replacement costs (Kuhnen and Oyer, 2016). Finding a replacement for a worker who is revealed to be a poor match is costlier for small firms than for large firms because they often do not have dedicated HR departments or the ability to recruit from internal labor markets (Tate and Yang, 2015). Second, larger firms face relatively lower firing costs. In our empirical setting, the costs of firing a worker with a permanent contract mostly depend on his/her seniority at a firm – with a minimum of three months' salary, a sum that represents a much greater share of the total output for firms with only a handful of employees than for large multinationals. Third, the total productivity in large firms is less affected by the presence of an unproductive employee. Whereas in small firms, the marginal contribution of a worker is more directly associated with firm performance, *ceteris paribus*. Thus, the cost of non-performance is higher in smaller firms, independent of the occupation of the employee.

In conclusion, we hypothesize that the wage penalty is more severe for entrepreneurs who are hired by small employers than for those who are hired by large organizations.

To summarize, in the empirical analysis, we will test the following predictions:

H1: Entrepreneurs receive a wage penalty relative to observationally equivalent employees.

H2: The wage penalty is more severe for entrepreneurs

(a) who were in the upper tail of the wage distribution,

(b) who exited quickly, and

(c) who are hired by smaller firms.

1.3 Empirical Context

This paper uses data drawn from the Belgian labor market. The Belgian labor market, unlike that of the US, is perceived to be highly rigid. According to the Organisation for Economic Co-operation and Development (OECD), Belgium ranks third in terms of worker protection against individual and collective dismissals, just behind Venezuela and China. These labor market frictions increase the costs of firing unsuccessful hires and thus make Belgium a particularly suitable context to test the role of uncertainty in hiring. Moreover, Zimmer (2012) observes that Belgium has a high mismatch between labor supply and demand, driven by a shortfall in the relative share of highly skilled job seekers and, conversely, a relatively high share of low-skilled labor supply, for which the demand is rather weak. These features of the Belgian labor market corroborate the appeal of this context to test our theory because it implies that concerns about a person's productivity will likely play an important role in the hiring process.

The Belgian business landscape is characterized by low rates of entrepreneurial entry and exit. In 2015, new businesses accounted for only 6.40 percent of the total share of all businesses, the lowest percentage in Europe. Belgium does fairly well in terms of its regulatory framework, market conditions, access to finance, and entrepreneurial capabilities (De Mulder and Godefroid, 2016), but it has poor contract enforcement (Calvino et al., 2016), high paid-in minimum capital requirements (Dreher and Gassebner, 2013), and a minimal entrepreneurial culture (De Mulder and Godefroid, 2016). One important factor that may hamper entrepreneurship in Belgium is the relatively high administrative burdens required to start a business : according to the OECD's Product Market Regulation Indicators Database, Belgium ranks among the 10 countries with the highest administrative burdens on start-ups.

At the same time, 71 percent of Belgian businesses survive their first three years, and 63 percent survive their first five years.[1] While these numbers might suggest an above-average performance of Belgian entrepreneurs compared with those in other countries, they could also indicate low thresholds of performance (Gimeno et al., 1997) and low levels of experimentation (Landier, 2005; Manso, 2016), especially if combined with little growth. The findings of Geurts and Van Biesebroeck (2016) suggest the latter explanation. Using Belgian data, they show that *de novo* entrepreneurs contribute little to overall job creation, and much less than commonly believed based on official statistics.

The characteristics described above make the Belgian situation comparable to those in other Western European countries, such as France, Germany, Finland, and Sweden. Previous studies examining the returns from an entrepreneurial experience usually rely on data from relatively flexible labor markets and dynamic business environments, such as the US and Denmark (Kaiser and Malchow-Møller, 2011; Daly, 2015; Manso, 2016; Bruce and Schuetze, 2004; Williams, 2000). These contexts are characterized by a high tolerance for entrepreneurial experimentation and failure.

[1] source: Eurostat Business demography statistics:http://ec.europa.eu/eurostat/statistics-explained/index.php/

1.4 Data and Methodology

1.4.1 Data source and construction of a matched sample

We analyzed data on the Belgian labor market from the Data Warehouse Labor Market and Social Protection (DWH LM&SP), maintained by the Crossroads Bank for Social Security (CBSS). The DWH LM&SP is a linked administrative dataset that combines quarterly data from nearly 20 Belgian social security institutions, covering the full population of legal residents in Belgium. The data span all quarters between the first quarter of 2000 and the fourth quarter of 2015 and contain detailed information about individuals' demographics, employment status, and income.

The initial sampling frame consists of the population of full-time employees working in one job in wage employment in the first quarter of 2004, who either transitioned into entrepreneurship at some point between 2004 and 2015 (entrepreneurs) or who remained in wage employment for that period (employees). A person is assigned to the entrepreneurs' group if he/she is classified as self-employed for at least four consecutive quarters between the second quarter of 2004 and the fourth quarter of 2015. CBSS classifies a person as self-employed if, on the last day of a given quarter, his/her main job is entrepreneurship *and* he/she is not registered as an employee. Relying on this classification ensures that individuals affiliated with more than one firm in the form of a start-up and wage work were excluded, as these hybrid transitions follows different dynamics (Folta et al., 2010)[2].

To make our sample comparable to those used in previous studies examining the determinants and outcomes of a spell of entrepreneurship (e.g., Failla et al., 2017; Kaiser and Malchow-Møller, 2011; Nanda and Sørensen, 2010; Sørensen, 2007) and to reduce the likelihood that the results might be attributable to confounding factors, we impose additional restrictions. First, we restrict the sample to individuals aged 22–49 in the first quarter of 2004 to eliminate biases attributed to left and right censoring. Second, we exclude those with entrepreneurship experience between 2000 and 2003 because the

[2]In unreported analyses, we verified that our results are robust with respect to the inclusion of hybrid entries and exits in and out of entrepreneurship. The results of this analysis are available upon request from the authors.

dynamics of serial entrepreneurship are likely to be distinct from those of first-time entrepreneurship (Westhead and Wright, 1998). Third, in line with previous studies (e.g., Nanda and Sørensen, 2010; Sørensen, 2007), we exclude individuals working in the primary sector in the first quarter of 2004. Fourth, to avoid missing observations for individuals moving out of the country at a certain point in time, we restrict the sample to individuals who continuously resided in Belgium between 2004 and 2015.

Identifying the potential wage cost of a spell of entrepreneurship poses an important inferential challenge, as transition into entrepreneurship is not random. Various studies have shown that individuals self-select into entrepreneurship based on certain characteristics and incentives, such as a preference for non-pecuniary benefits (Hamilton, 2000) and a balanced skill set (Lazear, 2005). Thus, naive estimations of the outcomes would likely result in high model dependence and heavy reliance on extrapolation because of an insufficient overlap in covariate distribution (King et al., 2007; Stuart, 2010). To address potential biases caused by self-selection, we construct a matched sample of employees who are similar to the entrepreneurs on a range of observable characteristics. The fundamental counterfactual idea is that the wages of the matched employees represent the outcomes of the entrepreneurs had they not chosen to become self-employed. This method has been used to address issues of self-selection in previous studies measuring entrepreneurial outcomes (Campbell, 2013; Daly, 2015; Failla et al., 2017; Kaiser and Malchow-Møller, 2011; Manso, 2016).

We apply a combination of exact matching and propensity score matching (Rosenbaum and Rubin, 1983), particularly 1:1 nearest neighbor matching without replacement. An individual is considered as a good match to another individual who chooses to become entrepreneur if in the first quarter of 2004, *1)* the individual is in the same age category, has the same gender, lives in the same region, works in the same industry, and has the same average daily wage; and *2)* if their estimated propensity scores differ by no more than 0.002[3].

[3] The use of the specified distance also implies common support, i.e., there is a complete overlap in the distributions of the propensity scores between the entrepreneurs and employees in the matched sample. The chosen width is substantially smaller than the generally recommended caliper of 0.2 standard deviations of the linear propensity score (Austin, 2011).

To ensure that the results are not peculiar to our matching algorithm, we also applied coarsened exact matching (Iacus et al., 2012). The results of this exercise are shown in Table A2 in the Appendix. The mean values for most of the variables of the coarsened exact matched sample are very similar to those obtained from the propensity score matching. Therefore, we do not expect that our results would change significantly if we had used the sample obtained from the coarsened exact matching[4].

Matching variables

In the matching procedure, we include a series of variables that previous studies have shown to be related to transitions into entrepreneurship and wage work. One advantage of including many different variables rather than a small set of *predictors of convenience* is that this minimizes potential bias due to the omission of an important confounder (Stuart, 2010). The matching variables were measured in the first quarter of 2004, i.e., right before the individual becomes at risk of becoming an entrepreneur.

As a starting point, we include several demographic variables. Most people start businesses when they are well into their thirties or older (Evans and Leighton, 1989). Second, entrepreneurs are more likely to be male (Manso, 2016). Therefore, we consider individuals to be a good match if they are in the same age category and of the same gender. Blanchflower and Meyer (1994) suggests that civil status affects the entrepreneurial entry choice. We include a civil status variable that distinguishes between individuals with or without partners and indicates the number of children for whom they are responsible, as well as variables capturing whether a person's partner has a job and his/her average daily wage. Furthermore, to control for potential structural geographical differences in labor market dynamics, we match individuals on the region (Flanders, Wallonia, and the Brussels Capital region) they were living in the first quarter of 2004.

We include a variable measuring the number of jobs a person held between

[4] An important set of robustness checks of the results is to verify that they are consistent across a variety of matching algorithms. Yet, data access restrictions limited our ability to perform such robustness tests. As such, we suggest this limitation as a direction for future research.

2000 and 2004 as a proxy for entrepreneurs being jacks-of-all-trades (Lazear, 2005) or having a preference for (job) variety (Åstebro and Thompson, 2011). We also include a measure of how many quarters a person was unemployed between 2000 and 2004 to capture differences in labor market experience.

Smaller firms spawn entrepreneurs at a higher rate, and entrepreneurs coming from small firms perform differently than those coming from large firms (Elfenbein et al., 2010; Parker, 2009). Additionally, wages and wage growth are higher in larger firms, on average (Oi and Idson, 1999). Therefore, we include an employer size variable, measured as the number of employees at the firm the individual was affiliated with in the first quarter of 2004. An indicator for whether a person is working in the public or private sector is also added, as the type of sector affects the likelihood of becoming an entrepreneur (Özcan and Reichstein, 2009). To control for potential differences in switching costs emerging from differences in firm- or industry-specific human capital (Becker, 1993; Neal, 1995), we included an individual's tenure at his/her current employer, as well as his/her tenure in his/her current industry.

Astebro et al. (2011) and Elfenbein et al. (2010) show that a person's position in the wage distribution is an important predictor of entrepreneurial entry. Therefore, two individuals were matched if they earned the same average daily wage in the first quarter of 2004. CBSS provides information about an individual's average daily wage per quarter, which is defined by

$$\frac{(Quarterly\ normal\ remuneration + flat-rate\ remuneration)}{Nr.\ full-time\ remunerated\ days}$$

. All reported wages are gross wages and are provided by CBSS in classes of 10 euros. We also include a measure of an individual's annual wage growth to take into account potential negative wage shocks related to transitions into entrepreneurship.

Despite the wide range of covariates included in the matching procedure, no data are available on a person's education. Although this is a limitation of our study, we tried to minimize concerns about the confounding effects of unobserved ability by performing close matches on variables that are significantly correlated with ability, such as wage, wage growth, and time spent in unemployment.

Quality of the matched sample

Table A1 in the Appendix provides the summary statistics for the variables used in the matching process. Columns 1 to 3 show the summary statistics for the full sample *prior* to matching. The first column displays the average values for the entrepreneurs. The second column gives the corresponding values for the employees. The third column displays the standardized percentage bias[5]. One advantage of this balance diagnostic is that it is similar to an effect size (Rosenbaum and Rubin, 1985), which is preferred over the use of t-tests or p-values to assess balance (Stuart, 2010).

Entrepreneurs are more likely to be male, between 25 and 34 years old in 2004, and to have a partner with entrepreneurship experience. They have a higher likelihood of working in the private sector, particularly in the fields of construction, wholesale, retail trade, real estate, or professional services. They are less likely to be employed in the public administration, defense, or education sectors. Entrepreneurs are much more likely to be employed in smaller firms, and they are more than 50 percent less likely to come from the largest firms ($\geqslant$ 1000 employees). They also have a more varied job history, have lower tenure at their current employer, and spent more quarters in unemployment between 2000 and 2004. They have a slightly higher daily wage, but they do not differ from employees with regard to annual wage growth. Overall, these figures closely resemble those found in previous studies of entrepreneurship.

Columns 4 to 6 present the summary statistics for the entrepreneurs and employees *after* the matching procedure. Apart from perfectly balancing the variables on which exact matching was performed (age, region, industry, and daily wage), the matching process significantly improves balance on the remaining covariates: none of the variables has a standardized percentage bias greater than 2.3 percent in the matched sample, well below the 10 percent convention (Rosenbaum and Rubin, 1985). The pre-matching imbalances, which could influence the reliability of the estimates, have been removed. In total, we retain a matched sample of 64,946 individuals.

[5]The standardized percentage bias is the percentage difference of the sample means in the entrepreneurs' and employees' (full and matched) sub-samples as a percentage of the square root of the average of the sample variances of both groups (Rosenbaum and Rubin, 1985).

1.4.2 Additional sample restrictions

One shortcoming of relying on occupational data is that it does not provide information about the nature of the entrepreneurship spell. To mitigate concerns about necessity entrepreneurship, we restrict the analysis to matched pairs in which the entrepreneur transitions into entrepreneurship no later than four quarters after leaving paid employment (*Non-necessity*, column in Table 1.1). Therefore, individuals who enter entrepreneurship after a full year of unemployment or inactivity are excluded, as they might have become self-employed because of limited opportunities in the labor market. Similarly, as we are interested in entrepreneurs returning to the labor market, we only include entrepreneurs who return no later than four quarters after leaving entrepreneurship (*Return*).

To avoid issues of different pay schemes between full- and part-time or temporary jobs, we also restrict the sample to individuals who transition into entrepreneurship after leaving a full-time job and who re-enter the labor market via a full-time job (*Full-time job*). While this restriction leads to a significant drop in the number of observations, it allows us to obtain parameter estimates that are less likely to be biased because of the confounding effects of the different nature of remuneration between these types of jobs. However, this also urges caution in generalizing the results to workers who enter and/or exit entrepreneurship via part-time or temporary jobs.

Table 1.1 provides an overview of how the number of observations and the daily wage changed when we imposed these additional restrictions. In the final sample, we retain 2,735 entrepreneurs. The most significant drop in observations occurs when we restrict our sample to the entrepreneurs who return to wage employment within the sampling period. This is likely because a substantial share of the entrepreneurial spells in our data commenced some years after 2004 and, therefore, can be right censored.

Table 1.1: Sampling and restrictions

	Restriction criteria				
	Full sample	Matching	Non-necessity	Return	Full-time job
Entrepreneurs					
Individuals	87,614	32,473	23,792	5,565	2,735
Daily wage Q1 2004	124.88	109.19	109.53	105.28	108.95
Wage employees					
Individuals	835,969	32,473	23,792	5,565	2,735
Daily wage Q1 2004	118.51	109.19	109.53	105.28	108.95

Because we lose a substantial share of individuals from the full matched sample when we impose the additional restrictions, we want to verify how the retained sample differs from the individuals who have been dropped after imposing these restrictions. In Table A3 in the Appendix, we compare the means of the retained and dropped sub-samples over a range of demographic- and employment-related variables in the first quarter of 2004. In terms of average daily wage, the two groups are equal and earn around 109 euros per day. Small differences exist in terms of age, region, and household position. Yet, the retained sample has significantly fewer women than men. Furthermore, the individuals in the retained sample are more likely to be blue-collar workers than white-collar workers. In terms of industry, we observe that there are more workers in the construction sector and few in healthcare and support services. Individuals in the retained sample held more jobs, on average, and had about three months less tenure with their employer.

1.5 Results

1.5.1 Descriptive analysis

This study examines the relationship between a spell of entrepreneurship and the wages/wage growth of workers returning to paid employment. To measure the wage growth between the moment just before entering entrepreneurship and the moment of re-entering wage employment, we calculate the percentage of wage growth between the two periods and divide it by the duration of the intervening entrepreneurship spell. For the employees, we take the quarters of pre- and re-entry of their matched entrepreneur, so by construction, the spell lengths between both groups are equal.

Table 1.2: Summary statistics at the pre-entry and re-entry quarters

	Pre-entry				Re-entry			
	Entrepreneurs		Matched employees		Entrepreneurs		Matched employees	
	mean	sd	mean	sd	mean	sd	mean	sd
Avg. daily wage	127.631	52.34	127.726	46.72	136.625	59.60	147.086***	54.46
Δ wage					0.003	0.03	0.010***	0.02
Spell length					14.324	8.72	14.324	8.72
Employer change					0.817	0.39	0.230***	0.42
Employer tenure	16.277	11.81	20.423***	11.92	5.463	10.23	29.624***	16.49
Nr. of employers	2.793	2.26	2.165***	1.72	3.610	2.34	2.479***	1.97
Occupation								
Blue-collar	0.505	0.50	0.494	0.50	0.483	0.50	0.472	0.50
White-collar	0.455	0.50	0.450	0.50	0.474	0.50	0.462	0.50
Govt. official	0.040	0.20	0.056**	0.23	0.042	0.20	0.065***	0.25
Employer size								
< 5	0.136	0.34	0.094***	0.29	0.183	0.39	0.077***	0.27
5-9	0.117	0.32	0.093**	0.29	0.115	0.32	0.083***	0.28
10-19	0.119	0.32	0.107	0.31	0.114	0.32	0.105	0.31
20-49	0.180	0.38	0.171	0.38	0.162	0.37	0.170	0.38
50-99	0.083	0.28	0.095	0.29	0.068	0.25	0.096***	0.29
100-199	0.063	0.24	0.084**	0.28	0.061	0.24	0.092***	0.29
200-499	0.084	0.28	0.114***	0.32	0.085	0.28	0.112***	0.32
500-999	0.050	0.22	0.057	0.23	0.039	0.19	0.060***	0.24
⩾ 1000	0.167	0.37	0.186	0.39	0.172	0.38	0.205**	0.40
Employer sector								
Private	0.927	0.26	0.914	0.28	0.888	0.31	0.907*	0.29
Employer Industry								
Manufacturing	0.205	0.40	0.240**	0.43	0.144	0.35	0.235***	0.42
Electricity, gas, water	0.001	0.03	0.003	0.05	0.004	0.06	0.005	0.07
Construction	0.241	0.43	0.224	0.42	0.236	0.42	0.211*	0.41
Wholesale and retail trade	0.213	0.41	0.204	0.40	0.197	0.40	0.203	0.40
Hotels and restaurants	0.012	0.11	0.010	0.10	0.018	0.13	0.010*	0.10
Transport, storage, communication	0.088	0.28	0.088	0.28	0.099	0.30	0.092	0.29
Financial institutions	0.032	0.18	0.033	0.18	0.034	0.18	0.034	0.18
Real estate and professional services	0.130	0.34	0.119	0.32	0.136	0.34	0.119	0.32
Public administration, defence	0.032	0.18	0.039	0.19	0.056	0.23	0.046	0.21
Education	0.019	0.14	0.018	0.13	0.028	0.16	0.018*	0.13
Healthcare and support services	0.013	0.11	0.012	0.11	0.025	0.16	0.014**	0.12
Social and cultural services	0.011	0.11	0.009	0.10	0.020	0.14	0.012*	0.11
Observations	2,735		2,735		2,735		2,735	

Summary statistics of entrepreneurs and matched employees at the quarters of pre-entry and re-entry. Stars indicate significant differences between the two groups.
*p<0.05 **p<0.01 ***p<0.001

Table 1.2 provides the means and standard deviations of the employment-related variables for the entrepreneurs and matched employees in the last quarter before entry into entrepreneurship, the pre-entry quarter, and the first quarter in wage employment after exiting entrepreneurship. In the pre-entry quarter, entrepreneurs earn, on average, 127.63 euros per day, whereas the matched employees average 127.73 Euros. The difference is not significant, and it indicates that between the first quarter of 2004 (when the matching was performed) and the quarter of pre-entry, the wage trends of entrepreneurs and matched employees are equal. Yet, entrepreneurs have about four quarters less tenure with their employer, and they have worked for more employers. Furthermore, entrepreneurs are more likely to work for the smallest firms (1–9 employees). Therefore, it is important that we control for these factors in the regression analysis.

As Table 1.2 shows, at the quarter of re-entry, significant differences exist between entrepreneurs and the matched employees in terms of wage and wage growth. Entrepreneurs earn, on average, 136.63 euros per day, about 10.4 euros less than their matched counterparts. Entrepreneurs' estimated quarterly wage growth is 0.03 percent, 0.07 percentage points lower, or less than one third of the wage growth of the matched employees. Almost 82 percent of the entrepreneurs work for a different employer than the one they were working for pre-entry, compared with 23 percent of the matched employees. Similar to the quarter of pre-entry, there is a higher concentration of entrepreneurs in small firms, and entrepreneurs are less likely to work for larger employers.

Figure 1.1 displays the distribution of the durations of the entrepreneurial spells. On average, an entrepreneur remains for around 14 quarters, or 3.5 years, in entrepreneurship (cf. Table 1.2), but the distribution of spell lengths is right skewed: almost 50 percent of the spells last between one and two years, and almost 30 percent last between three and four years. These findings confirm previous observations that entrepreneurial spells tend to be short (cf. Manso, 2016; Dillon and Stanton, 2017). In this study, however, we only take into account entrepreneurial spells that effectively end and not the full range of entrepreneurial spells. The average spell length is therefore underestimated.

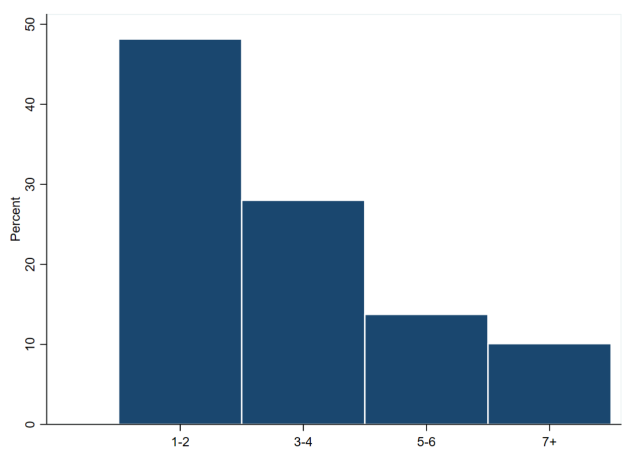


Figure 1.1: Duration of entrepreneurial spells (in years)

1.5.2 Tests against the data

The descriptive analysis showed that entrepreneurs earn significantly less than comparable wage employees when returning to paid employment. In the theory section, we argued that this penalty can be attributed to the increased uncertainty about job applicants with an entrepreneurial background (H1). To further corroborate our claims, we now turn to examining how heterogeneity in the degree and costs of uncertainty influences the size of the wage penalty.

Rank in the wage distribution

The theory predicts that the wage penalty increases for entrepreneurs with a higher ability signal (H2a). To verify these claims, we start by estimating the following three equations:

$$w_{i0} = \alpha_0 + \alpha_E Entrepreneur_i + \alpha_X X_{i0} + \alpha_I I_{i0} + \epsilon_0 \tag{1.1}$$

$$w_{i1} = \beta_1 + \beta_E Entrepreneur_i + \beta_W W_{i0} + \beta_{EW}(Entrepreneur_i * W_{i0}) + \beta_X X_{i1} + \beta_I I_{i1} + v_1 \tag{1.2}$$

$$\Delta w_i = \delta_1 + \delta_E Entrepreneur_i + \delta_W W_{i0} + \delta_{EW}(Entrepreneur_i * W_{i0}) + \delta_X X_{i1} + \delta_I I_{i1} + \xi_1 \tag{1.3}$$

where w_{i0} and w_{i1} are, respectively, the natural logarithm of the average daily wage in the quarter of pre- and re-entry, and Δw_i is the wage growth. $Entrepreneur_i$ is a dummy variable that takes on the value of one if the individual belongs to the group of entrepreneurs. X_{i0} and X_{i1} are vectors containing the observed characteristics of individuals and their employers at the quarters of pre- and re-entry. Specifically, in each model, we account for a person's gender, age, position in the household, region, occupation, employer sector, industry, size, and the length of his/her entrepreneurship spell. W_{i0} represents the average daily wage at the quarter of pre-entry, in deciles, and functions as a proxy for workers' ability. To allow for a differential effect of entrepreneurial experience at the various levels of wage distribution, we also include the interaction effect $Entrepreneur_i * W_{i0}$. I_{i0} and I_{i1} represent quarter fixed effects at the time of pre- and re-entry.

Equation (1.1) counts as a robustness check. Conditional on a range of observables, no significant differences should exist in terms of wages between the entrepreneurs and matched employees before entrepreneurship. If there are, issues related to positive or negative selection into entrepreneurship could influence the results of the estimations of the wage and wage growth at the quarter of re-entry into the labor market. Potentially significant differences in the pre-entry quarter should therefore be taken into account when interpreting the coefficients of w_{i1} and Δw_i. As we are particularly interested in the possible differences in wage penalty between the top and the bottom of the wage and ability rank, we estimate Equation (1.1) via quantile regressions for the 10th, 25th, 50th, 75th, and 90th percentiles to ensure that no significant differences exist between entrepreneurs and paid workers at distinct points of the wage distribution. We estimate equation (1.2) and (1.3) with and without the interaction effects.

Table 1.3 displays the results of the quantile regressions of equation 1.1 and the OLS regressions of equations (1.2) and (1.3).[6] Columns 1 to 5 report the results of the relationship between entering entrepreneurship and a person's wage in the last quarter of paid employment before transitioning to entrepreneurship at different points in the wage distribution. The results from the conditional analysis confirm those from the unconditional comparison of means (cf. Table 1.2): conditional on a variety of covariates, entrepreneurs do not earn significantly more or less before transitioning to entrepreneurship at any point in the wage distribution.

[6] We also estimated a seemingly unrelated regression (SUR) system on the same set of equations to allow for cross-equation error correlation. This did not alter the results.

Table 1.3: Quantile regressions on the average daily wage at the quarter of pre-entry, and OLS regressions on the average daily wage at the quarter of re-entry, and quarterly wage growth.

	Quantile regression on $ln(wage_0)$, percentile					OLS			
	10th	25th	50th	75th	90th	$ln(wage_1)$	$ln(wage_1)$	$\Delta ln(wage)$	$\Delta ln(wage)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Entrepreneur	-0.009	-0.001	0.002	0.009	0.008	-0.057***	0.017	-0.006***	0.003
	(0.007)	(0.006)	(0.006)	(0.006)	(0.008)	(0.006)	(0.014)	(0.001)	(0.002)
W_0, decile = 2						0.064***	0.089***	-0.008***	-0.005***
						(0.010)	(0.011)	(0.001)	(0.001)
W_0, decile = 5						0.185***	0.232***	-0.015***	-0.010***
						(0.011)	(0.012)	(0.001)	(0.001)
W_0, decile = 9						0.540***	0.625***	-0.023***	-0.013***
						(0.016)	(0.016)	(0.002)	(0.002)
W_0, decile = 10						0.790***	0.890***	-0.031***	-0.019***
						(0.020)	(0.020)	(0.002)	(0.003)
Entrepreneur#W_0, decile = 2							-0.040*		-0.005*
							(0.019)		(0.002)
Entrepreneur#W_0, decile = 5							-0.082***		-0.009***
							(0.020)		(0.002)
Entrepreneur#W_0, decile = 9							-0.157***		-0.019***
							(0.028)		(0.003)
Entrepreneur#W_0, decile = 10							-0.178***		-0.022***
							(0.032)		(0.004)
Constant	5.489***	5.382***	5.066***	4.910***	5.603***	4.454***	4.397***	0.008	0.001
	(0.108)	(0.099)	(0.108)	(0.106)	(0.099)	(0.114)	(0.113)	(0.017)	(0.017)
Observations	5,470	5,470	5,470	5,470	5,470	5,470	5,470	5,470	5,470
R-squared	0.331	0.419	0.464	0.458	0.422	0.645	0.651	0.153	0.171

Additional control variables (not displayed): age, gender, household position, region, occupation, employer sector, employer industry, employer size, employer tenure, nr. of jobs held, employer change, spell length, quarter fixed effects. Robust standard errors in parentheses. *p<0.05 **p<0.01 ***p<0.001.

Columns 6 and 7 in Table 1.3 show the results for the wage in the quarter of re-entry into paid employment. We find that, conditional on the wage before entering entrepreneurship, entrepreneurs earn 5.7 percent less than their counterparts in the control group (column 6). Furthermore, when we include the interaction effect between the entrepreneur dummy and the pre-entry wage rank (column 7), the results indicate that the penalty is larger the more an entrepreneur earned before entering entrepreneurship. Figure 1.2 displays the predictive margins of the wage at re-entry for entrepreneurs and the matched employees. The dashed line indicates the estimated margins for the entrepreneurs; the solid line is for the matched employees. Entrepreneurs coming from the lower tail of the wage distribution (1st and 2nd deciles) do not earn a lower wage than their employee counterparts, but the wage gap appears at the 5th and is widest at the 9th and 10th deciles.

Columns 8 and 9 in Table 1.3 display the results for the predicted quarterly wage growth between the quarter of pre-entry into entrepreneurship and that of re-entry into paid employment. The results confirm the findings that entrepreneurs incur a wage cost when returning to the labor market. On average, their quarterly wage growth is around 0.6 percent lower per quarter (column 8). However, the entrepreneurs who come from the upper tails of the pre-entry wage distribution are punished the most. Figure 1.3 displays the predictive margins for the entrepreneurs and employees of the estimated regression in column 5 of Table 1.3. Entrepreneurs coming from the upper tail of the pre-entry wage distribution not only have significantly lower wage growth than their matched counterparts, but their wage growth is also negative. Entrepreneurs in the top decile of the wage distribution have a predicted quarterly wage loss of 1.8 percent.

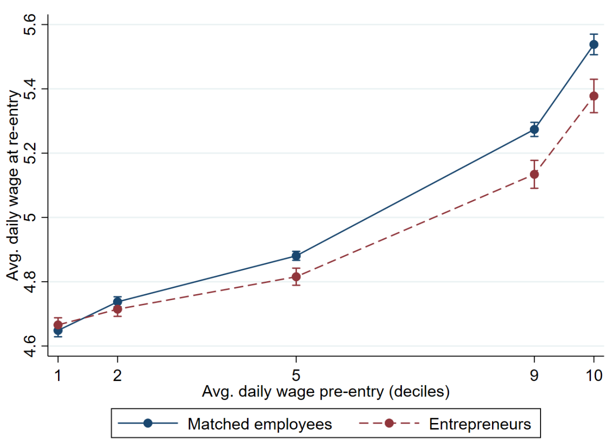


Figure 1.2: Plot of predictive margins of $\ln(\text{wage}_1)$ at different levels of the avg. daily wage at the pre-entry stage: entrepreneurs vs. matched employees

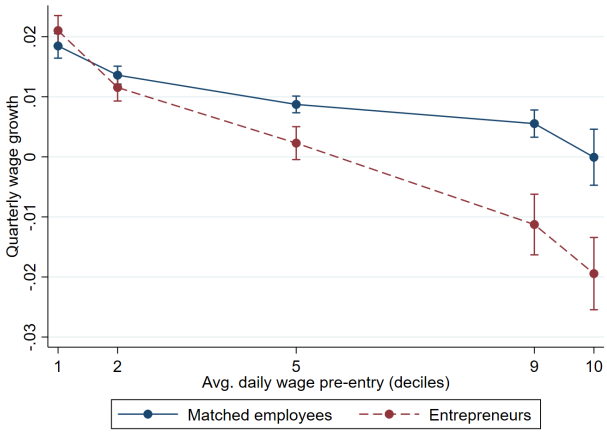


Figure 1.3: Plot of predictive margins of Δwage at different levels of the avg. daily wage at the pre-entry stage: entrepreneurs vs. matched employees

In general, external job applicants are riskier hires than workers taking up a new job from inside the firm (Bidwell, 2011). Therefore, an important concern regarding these results is that they might be driven by the fact that most employees in the control group do not change employers at the time the matched entrepreneur returns to wage work, so we might pick up the effect of uncertainty about the productivity of new hires, in general, rather than uncertainty specifically related with entrepreneurial experience. To address this concern, we check the sensitivity of the wage penalty to a counterfactual of matched employees who change employers within a year from the quarter the entrepreneur re-enters wage work (n=581). Figure 1.4 shows the quarterly wage growth for the sample in which the matched employee also changes jobs within the year the entrepreneur returns to wage work. The results are very close to those of the main analysis. If job changing per se were negatively associated with an individual's wage growth, we would expect a downward shift in the matched employee's wage growth plot. This is not the case. Therefore, our results do not appear to be driven by oversampling stayers in the control group.

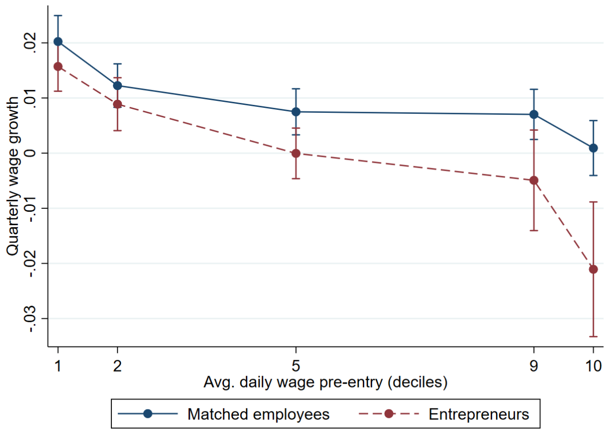


Figure 1.4: Plot of predictive margins of Δwage at different levels of the avg. daily wage at the pre-entry stage: entrepreneurs and matched employees who change jobs at the time entrepreneurs re-enter wage work

Length of the entrepreneurial spell

We next examine if the wage penalty declines with tenure in entrepreneurship (H2b). To test this conjecture, we compare the differences in quarterly wage growth between entrepreneurs and their matched counterparts for entrepreneurs who exit quickly versus those who survive for a number of years. To do so, we estimate the following model:

$$\begin{aligned}\Delta w_i = \theta_1 + \theta_E Entrepreneur_i + \theta_W W_{i0} + \theta_D D_i + \theta_{EW}(Entrepreneur_i * W_{i0}) \\ + \theta_{ED}(Entrepreneur_i * D_i) + \theta_{WD}(W_{i0} * D_i) \\ + \theta_{EWD}(Entrepreneur_i * W_{i0} * D_i) + \theta_X X_{i1} + \theta_I I_{i1} + \psi_1\end{aligned} \tag{1.4}$$

The parameters included in equation 1.4 are those of equation 1.3, but we add a three-way interaction effect between the entrepreneur dummy, a person's position in the wage distribution W_{i0}, and the duration of his/her entrepreneurial spell D_i(or his/her matched counterpart's spell for the control group).

Table 1.4 displays the marginal effects obtained from the OLS regression of Equation 1.4. Each cell represents the difference in wage growth between the entrepreneurs and the matched employees for a certain spell duration (all spells, 1-2, 3-4, 5-6, and 7+ years) at a certain level of pre-entry wage (1st, 2nd, 5th, 9th, and 10th deciles). The results show that entrepreneurs coming from the lowest pre-entry wage deciles (1st and 2nd) never incur a penalty. However, for those who earned a relatively high wage before entering entrepreneurship (5th, 9th, and 10th deciles), the wage penalty is most pronounced when they exit in the first two years after entry, and it gradually declines over time. After five years of survival in entrepreneurship, no wage growth difference can be observed between entrepreneurs and their matched counterparts at any level of pre-entry wage.

Table 1.4: Quarterly wage growth over different spell durations: marginal effects of entrepreneur at different deciles of avg. daily wage pre-entry.

	Spell duration (years)				
Pre-entry wage (decile)	All	1-2	3-4	5-6	7+
1st	0.003*	0.006	0.003	0.002	-0.001
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
2nd	-0.001	-0.004	0.001	0.002	-0.001
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
5th	-0.006***	-0.010***	-0.002	-0.003	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
9th	-0.017***	-0.022***	-0.013**	-0.005	0.001
	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)
10th	-0.018***	-0.027***	-0.010**	-0.008	-0.004
	(0.004)	(0.006)	(0.004)	(0.004)	(0.003)
Observations	5,470	2,634	1,532	752	552

Standard errors in parentheses. *p <0.05 **p<0.01 ***p<0.001

Employer size

So far, we have observed that employees from the top of the wage distribution are penalized for attempting entrepreneurship, but only if they exit quickly. We now turn to our final prediction, namely that the penalty is larger for entrepreneurs hired by small employers (H2c).

To examine whether entrepreneurs are more penalized in small firms, we regress the quarterly wage growth on a triple interaction term between the entrepreneur dummy, the pre-entry wage (deciles), and a small firm dummy, which captures all firms with fewer than 20 employees. We also include a variable measuring the size of the employer at the quarter of pre-entry. In our data, the size of the employer before and after entrepreneurship is positively correlated, indicating that entrepreneurs who worked in small (large) firms before entrepreneurship are likely to work in small (large) firms after entrepreneurship, so adjusting for pre-entry employer size minimizes concerns that the results might be driven by wage differences between small and large firms before entrepreneurship. Additional control variables that are equal to those in Equation 1.4 are also included. In doing so, we can compare the wage growth across different wage deciles between entrepreneurs moving to small (large) firms against employees working in small (large) firms. As we only observed a penalty for short spells, we restrict the analysis to entrepreneurs returning to wage work within four years.

Table 1.5 displays the marginal effects obtained from the OLS regression. Each cell represents the difference in wage growth between entrepreneurs and the matched employees, respectively, for small and large employers at a certain level of the pre-entry wage (1st, 2nd, 5th, 9th, and 10th deciles). The results show that the penalty for former entrepreneurs is roughly the same in small and large firms for individuals in the lower and the median deciles of the pre-entry wage distribution. However, large discrepancies between small and large firms occur for those who earned relatively more before entering entrepreneurship: entrepreneurs coming from the top wage decile prior to entry into entrepreneurship and who find employment at a large firm after their entrepreneurial spell have an estimated 1.4 percentage points lower quarterly wage growth than similar employees in large firms without entrepreneurial experience. For entrepreneurs moving back to small firms, this penalty is more than three times larger; the estimated quarterly wage growth is 4.7 percentage points less for entrepreneurs coming from the top of the wage distribution compared with employees coming from the top of the distribution.

A possible alternative explanation for these findings is that entrepreneurs who move back to small firms are of lower ability than those who find employment at a large firm. To test this possibility, we regress the entrepreneurial performance of the group of entrepreneurs on the probability of joining a small

Table 1.5: Quarterly wage growth by employer size: marginal effects of entrepreneur at different deciles of avg. daily wage pre-entry.

	Employer size	
Pre-entry wage (decile)	Small	Large
1st	0.009**	-0.001
	(0.002)	(0.003)
2nd	-0.003	-0.003
	(0.002)	(0.002)
5th	-0.007*	-0.009**
	(0.003)	(0.003)
9th	-0.032***	-0.017***
	(0.008)	(0.004)
10th	-0.047***	-0.014***
	(0.009)	(0.005)
Observations	4,166	4,166

Only spells shorter than five years. Standard errors in parentheses.*p<0.05 **p<0.01 ***p<0.001

firm. The individuals' entrepreneurial performance is measured by their average yearly earnings during their entrepreneurial spell.[7] We would expect a negative relationship between entrepreneurial performance and the probability of joining a small firm if entrepreneurs of lower ability are more likely to join small firms after they fail. We display the results using both the raw entrepreneurial earnings and the log of the entrepreneurial earnings, as the measure is highly skewed and hence the results may be driven by this feature of the variable.

The results of the regressions of entrepreneurship earnings and ln(average entrepreneurship earnings)[8] on the probability of joining a small firm after entrepreneurship are reported in columns 1 and 2 of Table 1.6. The coefficients on entrepreneurial earnings are close to and not statistically significantly different from zero. Therefore, we find no evidence that entrepreneurial ability influences the probability of moving to a small or large employer after entrepreneurship.

Finally, we examine one situation in which uncertainty around an entrepreneur's future productivity is presumed to be absent, i.e., when the en-

[7] We ran the same regressions only using the last reported earnings before exiting entrepreneurship. The results remain the same.

[8] 291 entrepreneurs reported only zero earnings during their entrepreneurship spell. Therefore, they are dropped in the regression, in which we use the log-transformation of the earnings.

Table 1.6: OLS regressions on the probability of joining a small employer at the quarter of re-entry in wage employment

VARIABLES	(1) Raw measure	(2) Log measure
mean entrepreneurial earnings	-0.000	
	(0.000)	
ln(mean entrepreneurial earnings)		0.013
		(0.033)
W_0, decile = 2	-0.110	-0.121
	(0.131)	(0.145)
W_0, decile = 5	-0.020	-0.045
	(0.148)	(0.164)
W_0, decile = 9	-0.360*	-0.453*
	(0.168)	(0.180)
W_0, decile = 10	-0.332	-0.410*
	(0.177)	(0.190)
Constant	-0.397	-0.553
	(0.877)	(0.929)
Observations	1,630	1,339
Pseudo R^2	0.149	0.150

Additional control variables (not displayed): age, gender, household position, region, industry, pre-entry employer size, spell length, quarter fixed effects. Standard errors in parentheses. *p<0.05 **p<0.01 ***p<0.001.

trepreneur goes back to the same firm he/she worked for before entrepreneurship. In this case, employers can rely on the revealed worker's productivity during the first employment period rather than on the noisy signal of the entrepreneurial spell. We regress the quarterly wage growth on a triple interaction term between the entrepreneur dummy, a dummy indicating whether a worker changed employers between the pre- and re-entry quarters, and the worker's position in the wage distribution. We restrict the sample to the entrepreneurs (and their matched counterparts) who returned to wage employment within four years.

Figure 1.5 displays the marginal effects of a spell of entrepreneurship for the workers in the top decile of the wage distribution, split based on whether or not they changed employers. The left bar shows the difference in wage growth between the entrepreneurs and employees who work for the same employer in the pre- and re-entry quarters (employer change = no). The right bar shows the difference in wage growth between the entrepreneurs and employees who move to a different employer. The results show that entrepreneurs who return to the same employer as before entrepreneurship do not incur a penalty compared with employees who work consecutively for the same employer, whereas entrepreneurs who move to a different employer incur a significant penalty compared with employees who have switched employers. These results corroborate our theoretical claims, as they show that the penalty occurs only in the presence of information asymmetries between job applicants and prospective employers.

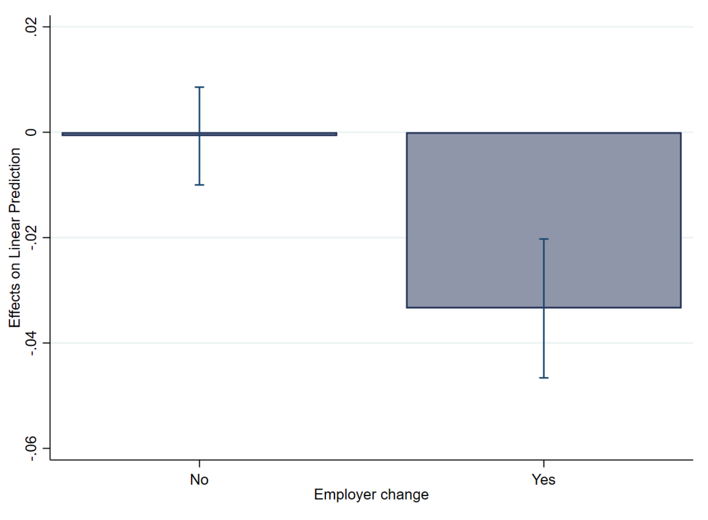


Figure 1.5: Marginal effects of entrepreneur at the top 10 % of the pre-entry wage distribution and for spells < five years by whether they work for the same employer in the pre-entry and re-entry quarters

1.6 Robustness Checks

Sensitivity of the wage penalty to sample restrictions

In an attempt to buttress the likelihood that we are capturing the direct relationship between a spell of entrepreneurship and future wages, we took steps that further reduce the influence of confounding factors. We did so by imposing restrictions on the sample of entrepreneurs who return to the wage sector (Return) by a) not including necessity entrepreneurs and b) only including entrepreneurs who entered entrepreneurship from and exited entrepreneurship to a full-time job. We now relax these restrictions to assess how sensitive our results are to these modeling decisions.

Full matched sample. We estimate the quarterly wage growth of all entrepreneurs and their matched counterparts who, at some point during the sample period, re-enter wage work after a spell of entrepreneurship (17,208 out of 32,473), regardless of the time in unemployment or out of the labor force before and after entrepreneurship, and including part-time and temporary workers. Figure A1 in the Appendix shows the results. The pattern of the wage penalty is close to that of the restricted sample used in the main analysis (Figure 1.3). Yet, there is now a significant wage premium for entrepreneurs coming from the lowest wage decile, as well as a stronger penalty for the stars. The stronger wage penalty suggests that our estimates for the stars observed in the restricted sample are conservative. The wage premium is explained by the next exercise.

Relaxing full-time job. We now turn to relaxing the full-time job restriction. About 24 percent of the entrepreneurs re-enter into a part-time job, compared with 10 percent in the control group. As CBSS calculates the average daily wage for part-time jobs as if the person had worked a full-time week of 38 hours, any structural differences in hourly remuneration between these different types of contracts will bias the results. Figure A2 shows the results only for the entrepreneurs who re-enter into a part-time job and their matched counterparts (n = 4,166). We find results similar to the full sample (Figure A1) but different from the restricted sample (Figure 1.3): there is a significant wage premium for those who earned little before entering entrepreneurship, and a stronger penalty for the stars. This might be an indication of an hourly wage premium (penalty) for low-paid (high-paid) part-time jobs compared with similar full-time jobs. Therefore, it appears that the wage premium found in the full sample can be explained by the part-time job measurement error. To further investigate whether the premium at the left tail is driven by part-time workers, we look at a third category of job contracts, i.e., temporary contracts, which include all short-term and irregular contracts, mostly temporary and seasonal work. About 19 percent of the entrepreneurs re-enter into such a temporary job, although these are mostly entrepreneurs coming from the lowest wage deciles. Figure A3 displays the results for these entrepreneurs and matched employees (n = 3,276). For the lowest wage deciles, the results are similar to those of the restricted sample, further suggesting that the wage premium in the full sample is driven by the measurement error associated with taking on a part-time job. For the stars, the estimate of the wage penalty is much larger than before, but it also very noisy, as only 24 individuals from the 10th wage decile re-enter into a temporary job.

Relaxing non-necessity. We now relax the non-necessity restriction at both entry in entrepreneurship and re-entry into wage work to see whether including necessity entrepreneurs alters our estimates. Figure A4 illustrates the estimates of the wage growth for the sample of entrepreneurs who enter entrepreneurship after more than one year of unemployment or out of the labor force (necessity entry are 8.4 percent of all entries). These individuals might enter entrepreneurship because of limited outside options. The figure shows a similar pattern as the restricted sample with a slightly stronger penalty for stars. Finally, we estimate the wage growth for the sample of entrepreneurs who re-enter wage work after more than one year in unemployment or out of the labor force (necessity exit are13 percent of all exits). These entrepreneurs might experience difficulties in finding a suitable job. Figure A5 shows that the wage growth pattern mimics that of the restricted sample, but estimates at the top are very noisy because of few stars experiencing a necessity exit.

Overall, this exercise shows that our results are robust to different sample specifications. The estimates of the wage penalty for stars appear rather conservative in the restricted sample we use for the main analysis, as the penalty for stars observed in these larger samples appears even stronger.

1.6.1 Sensitivity of the wage penalty compared to the matched group

Counterfactual of movers in the year of entry into entrepreneurship. In the foregoing analyses, we already verified that our main results are not driven by the confounding effects of the uncertainty of hiring external job candidates, in general (cf. Figure 1.4). To further strengthen the argument that our results are not driven by the potential negative effects of job changing per se, we repeat this exercise for the sample of entrepreneurs and matched employees who change employer within one year from the quarter the entrepreneur enters self-employment (n=488). Figure A6 displays the predicted wage growth at different levels of the wage deciles for this sample. We find that the estimated wage penalty is more compressed at the top deciles than in the full sample, although it is still significant ($p < 0.05$). The smaller penalty is caused by an upward shift in the estimated wage growth of the entrepreneurs coming from the top of the wage distribution, rather than by a downward shift in the wage growth of the control group. This evidence is inconsistent with the concern that movers might have lower wage growth. We further investigate the origins of the smaller penalty for stars compared with the main control group by comparing entrepreneurs who have a job-changing matched counterpart at entry into entrepreneurship with those who do not. The results are displayed in Table A4 (columns 2 to 3). Entrepreneurs with a mover matched counterpart have a significantly lower pre-entry wage than those without. This is not surprising, as star employees are less likely to move but, conditional on moving, are more likely to start a company (e.g., Campbell et al., 2012); this is also confirmed in our data. Therefore, using a counterfactual of movers means shifting the pre-entry wage distribution to the left. As a result, the smaller penalty in the top decile corresponds to a lower decile (i.e., 9th) in the main control group, which further strengthens our key result.

Counterfactual of not yet entrepreneurs. A fundamental assumption throughout this study is that the wage trend of entrepreneurs would have been parallel to that of the matched employees after entering entrepreneurship had they not become entrepreneurs. Although we matched the two groups on their wages in 2004 and the wage trends between 2000 and 2004, and a comparison of the wages at the pre-entry quarter showed no significant differences in wage growth, this assumption might still possibly be violated on the basis of unobservables. Certain unobservable traits related to selecting into entrepreneurship and into lower-paying firms (e.g., Elfenbein et al., 2010) may lead to a downward bias in the estimate of the negative wage growth for the entrepreneurs. To address this possibility, we exploit the variation in timing of entry and exit out of entrepreneurship. Specifically, we compare the wage growth of a group of entrepreneurs who exit entrepreneurship and re-enter wage employment no later than the first quarter of 2011 with the wage growth of a randomly selected group of entrepreneurs who did not enter entrepreneurship before the second quarter of 2011 and who always stayed in wage employment between the first quarter of 2004 and the first quarter of 2011[9]. To estimate the wage growth of the second group of not yet entrepreneurs, we duplicate the pre- and re-entry quarters of the individuals in the first group and calculate the wage growth in between these two quarters by using the formula explained in section 1.4. Figure A7 shows the wage growth of the entrepreneurs who re-enter wage work before or at the first quarter of 2011 and of the group of not yet entrepreneurs (n = 2,476). The results display a pattern that is strikingly similar to the ones shown before. Compared with workers who are about to become entrepreneurs but are still employed, former entrepreneurs, on average, are penalized, and the wage penalty is most severe for stars. These findings bolster the claim that our results are not driven by unobservable traits shared by current, nascent, and former entrepreneurs.

1.7 Alternative Explanations

Previous research in labor economics and entrepreneurship suggests a few potential explanations for why entrepreneurs may be penalized when returning to wage work. We describe these below and investigate their potential to offer alternative interpretations for our set of empirical findings.

Human capital depreciation

The observed wage penalty of entrepreneurs is consistent with models of human capital depreciation (Mincer and Ofek, 1982), either because former entrepreneurs' job-, firm-, or sector-specific skills atrophy during their spell of entrepreneurship (Williams, 2000) or because they lose out on valuable on-the-job training opportunities (Koellinger et al., 2015; Mincer and Ofek, 1982). The penalty for employees from the right tail of the wage distribution would then result from a higher atrophy rate for occupations that require high ability (Polachek, 1981). The most intuitive implication of these models is that the

[9]This makes the group of not yet entrepreneurs similar to our original control group of employees who always stay in wage employment between the first quarter of 2004 and the last quarter of 2015.

penalty should increase with tenure in entrepreneurship, if we assume that the depreciation rate is positive. However, the empirical evidence in Table 1.4 shows the opposite: the wage penalty diminishes with time in entrepreneurship. Moreover, human capital depreciation would imply a penalty even for entrepreneurs returning to their previous employer, which is inconsistent with our results (Figure 1.5).

Expected job mismatch

A symmetric explanation suggests that the human capital entrepreneurs develop is specific to that setting and is therefore not transferable to the wage sector. This argument has roots in the jack-of-all-trades theory, which predicts that entrepreneurs possess generalist skills, but employers value specialist skills (Lazear, 2005). Accordingly, the employer considers the candidate's choice to become entrepreneurs as a signal of potential skill mismatch and reacts by discounting the offered wage.

While mismatches are often interpreted in terms of skills, employers may also expect a mismatch between individual preferences and firm attributes (Jackson, 2013). Entrepreneurial preferences, such as tastes for autonomy (Hamilton, 2000), variety (Åstebro and Thompson, 2011), and risk (Fairlie, 2002), are at odds with the organizational culture of established firms (Özcan and Reichstein, 2009). As entrepreneurship is a choice, employers may discriminate against entrepreneurs based on an anticipated mismatch, a phenomenon more broadly known as taste-based discrimination (Koellinger et al., 2015). A straightforward prediction of this mechanism is a homogeneous penalty for all entrepreneurs, which contrasts with the significant heterogeneous penalties we find (Table 1.3).

In practice, however, established firms vary in the extent to which they mimic the organizational setting of start-ups. Most notably, small firms are closer to start-ups; both require a broader, balanced skill set, compared with large firms, which require narrowly defined and specialized jobs (Sørensen, 2007). Accordingly, a previous study suggests a higher transferability of human capital between small firms and start-ups (e.g., Elfenbein et al., 2010). Moreover, small firms appear as an attractive setting for entrepreneurial employees, also based on preference matching (Parker, 2009). These arguments predict a stronger penalty for entrepreneurs who join large firms because of the higher odds of job mismatch. Yet, the empirical evidence reported in Table 1.5 indicates a lower penalty in large firms.

Finally, there are organizational settings that mimic not only entrepreneurship but also occupations. Managers tend to have generalist skills (Lazear, 2012), and CEOs with generalist skills are paid more (Custódio et al., 2013). Therefore, the loss of human capital upon re-entering the wage sector is expected to be lower for those entrepreneurs hired in managerial positions. If we assume that entrepreneurs from the top tail of the wage distribution before entering entrepreneurship are more likely to hold managerial positions ex-post, then we would expect a weaker penalty for star employees. However, this is inconsistent with our key finding that stars are more penalized.

Stigma of failure

While the job mismatch rationale hinges on the information signal of entry into entrepreneurship, the stigma of failure explanation relies on the notion that (quickly) exiting out of entrepreneurship holds signaling value. With only limited information regarding job applicants' productivity, recruiters are likely to rely on generalization and stereotypes on the basis of observable characteristics: "most self-employed applicants have failed, and this is probably one of them" (Koellinger et al., 2015, p 148.). These generalizations are at the origins of statistical discrimination (Altonji and Pierret, 2001). The labor market may hold the impression that entrepreneurs' ability is tightly coupled with the quality of their venture and that entrepreneurial earnings are tied to start-up performance. Therefore, a quick exit may signal firm failure and, in turn, poor ability. This explanation implies a homogeneous wage penalty across entrepreneurs, which is inconsistent with our heterogeneous effects (cf. Table 1.3).

Sorting

So far, we have discussed demand-side candidate explanations, and, thus, we have implicitly assumed that the lower returns of entrepreneurs are the outcome of an adverse labor market treatment. However, there might be room for supply-side alternative mechanisms based on entrepreneur sorting behaviors. Entrepreneurs may sort into firms that pay them a lower salary in exchange for non-pecuniary benefits, as the theory of compensating differentials predicts (Rosen, 1986). Non-pecuniary benefits are well recognized to be particularly valuable for entrepreneurs (Hamilton, 2000). This explanation offers different testable implications based on whether the preference for non-pecuniary incentives is the result of the entrepreneurship *treatment* or of innate traits.

A treatment effect that changes individual preferences would predict a more severe pay cut for entrepreneurs with longer entrepreneurial spells. The preference for non-pecuniary over pecuniary benefits becomes stronger the longer the time in entrepreneurship. Reinforcing this mechanism, the larger the non-pecuniary benefits, the lower the performance threshold, which contributes to delay exit (cf. Gimeno et al., 1997). Again, this prediction is contradicted by the evidence in Table 1.4.

Individuals with innate preferences for non-pecuniary benefits may sort into firms that mimic the entrepreneurial setting, such as small firms that offer, on average, lower salaries. If innate unobserved preferences drive our results, then we should observe a lower penalty when we use as counterfactual the group of not yet entrepreneurs. Assuming that this control group shares the same innate preferences as the group of former entrepreneurs is reasonable. However, the results displayed in Figure A7 do not support the hypothesis that our findings are driven by differences in innate preferences between entrepreneurs and employees.

Labor market frictions

An alternative explanation for why entrepreneurs who were star employees are penalized more severely could be that entrepreneurs coming from high-paid jobs have difficulties finding a similar position when they return to the labor market. However, this is unlikely to be the case. It is well established that the unemployment rate is highest among less able workers, which implies that higher-ability workers can more easily find jobs. Moreover, in a recent paper, Lazear et al. (2018) show that the vacancy rates are highest for high-paying, high-quality jobs, as high-ability individuals are more flexible and can do a wide variety of jobs, whereas low-ability workers are not able to perform in high-quality positions. An unreported analysis on our own data confirms that stars are less likely to exhibit unemployment spells upon returning to the wage sector.

Finally, it is possible that adverse selection out of entrepreneurship might partly drive the results. In others words, the penalty may occur because employers offer lower wages, assuming that mainly low-ability entrepreneurs return to paid employment. However, the possibility of adverse selection is difficult to reconcile with the finding of an increasing penalty for entrepreneurs with higher wages before entrepreneurship. Suppose that prospective employers do not observe the pre-entrepreneurship signals but only ask for details about current earnings and entrepreneurship performance, and this information asymmetry can induce adverse selection. While prospective employers are less informed than prospective employees, they are not uninformed because pre-entrepreneurial wages and entrepreneurship earnings are correlated. In this case, we would expect to see entrepreneurs with low pre-entrepreneurship wages only accepting jobs for which they are unqualified. Knowing this, however, employers would impose a greater penalty on job applicants whose observable signals are low. Our main finding that the penalty is higher when pre-entrepreneurship wages are high seems inconsistent with the presence of significant adverse selection effects.

In conclusion, the above discussion suggests that while each of these alternative mechanisms appears to be consistent with some of the findings, their implications are inconsistent with the full set of empirical evidence presented in this paper. Thus, our uncertainty-based model seems the most coherent explanation behind our overall empirical patterns.

1.8 Concluding Remarks

Discussion

The empirical literature documents a negative relationship between a spell of entrepreneurship and subsequent wages, but it remains largely silent about the origins of this adverse treatment. This study advances and tests a theory that explains why a spell of entrepreneurship negatively affects individuals' wages upon returning to the wage sector.

Building on both the entrepreneurship and labor economics literatures, we proposed that an intermittent spell of entrepreneurship is a noisier signal about expected productivity in future wage work than a continuous career in paid employment, and that this uncertainty will negatively affect entrepreneurs' future wages when they re-enter the labor market. From this theoretical framework, we derive a number of testable predictions. Entrepreneurs are expected to receive a lower wage than observationally equivalent employees because of their higher risk of non-performance on the job. The wage penalty is more severe for entrepreneurs (1) who were in the upper tail of the wage distribution prior to the entrepreneurial spell, (2) who exited quickly, (3) and who are hired by smaller firms because of these firms' higher costs of handling hiring risks.

We test these predictions by using a new dataset of matched entrepreneurs and employees from Belgium, covering all quarters between 2000 and 2015. We obtain a number of results that strongly support the proposed mechanism and none that leads us to reject it. First, we find that entrepreneurs, on average, are penalized, but the penalty is more severe for those coming from the right tail of the wage distribution and absent for those who earned a relatively low wage before entrepreneurship. Second, the penalty for stars disappears if entrepreneurs survive for five or more years, in line with the intuition that uncertainty about a signal resolves over time. Third, large firms penalize less, in line with the notion that large firms bear lower replacement costs in case of a bad hire. Finally, entrepreneurs returning to their previous employer are not penalized, consistent with the information value of the signal being close to zero. Our results are robust to different counterfactuals and sampling restriction strategies. Moreover, we sort out the most salient competing explanations, which further reinforce the validity of our theory.

Some limitations might affect the interpretation and generalizability of the findings. First, the matching algorithm will produce biased outcomes if the transition into entrepreneurship is related to unobservable factors not captured in the model used to estimate the propensity score. To mitigate this possibility, we have matched entrepreneurs and employees on an extensive range of variables. Furthermore, the findings from a robustness check in which we compare former entrepreneurs' wages with those of employees who will become entrepreneurs in the future in our sample do not indicate that our results are biased by the unobservable traits and preferences shared by all entrepreneurs and which might correlate with their wage. However, the possibility remains that we cannot fully control for unobserved heterogeneity.

Second, the external validity of the results to the broader population of entrepreneurs and employees could be affected by the matching approach used in the paper. As a robustness check, we also performed a coarsened exact matching (Iacus et al., 2012). The output suggests that the results are not peculiar to our matching algorithm [10]. Moreover, the set of tests showing the robustness of our findings to relaxations of several sampling restrictions at least partially mitigates concerns of external validity, suggesting that the key results can be extrapolated to the broader population of entrepreneurs. Finally, the Belgian business landscape is characterized by low entrepreneurial dynamism, i.e., relatively low entry and exit, and by high labor market frictions that reduce the value of experimentation. Therefore, a potential avenue for future research is to verify whether the results can be replicated in more dynamic entrepreneurial contexts and flexible labor markets, such as those of the US and Scandinavian countries.

Implications

The findings also have implications for practice. It has almost become a doctrine in practitioners' entrepreneurship community (e.g., incubators) that failing fast is divine, as entrenched in the lean start-up movement. The idea is to avoid the sunk cost effect which may cause individuals to pursue opportunities even if the initial evidence indicates that the opportunity is not promising in its current form. This paper shows that outside option career considerations may question the attractiveness of failing rapidly. We suggest that high-ability entrepreneurs may consider the pivot option of the lean start-up as more desirable than the fast fail option, in which they fine-tune and adjust the business

[10] An additional robustness test would be to examine if the results are consistent in an n:1 matched sample in order to verify if the findings are not attributable to the idiosyncratic characteristics of the individuals in the current sample. Unfortunately, data access restrictions prevented us from performing such analyses.

to better fit the market, allowing them to sustain the business and avoid the wage penalty.

Our findings also have implications for policy makers. First, the uncertainty in hiring entrepreneurs suggests that employers would value probationary contracts that offer the option to learn about the upside potential of these risky workers at a limited cost. As it is usually difficult for employers to poach star employees from other firms, hiring them from entrepreneurship via probationary contracts might prove an attractive recruitment strategy[11]. This would persuade employers not to disregard a pool of applicants with high potential, including the possible valuable skills gained in entrepreneurship (Campbell, 2013). Second, our result that the wage penalty declines with tenure in entrepreneurship might be consistent with the tendency of underperforming ventures to delay exit (e.g., Failla et al., 2017). Again, more flexibility in the use of probationary arrangements could also contribute to alleviating this labor market friction and allow employers to rely on the observed productivity during the probation period, rather than on noisy signals of entrepreneurial performance, such as survival. This would in turn increase the value to experiment without the risk of getting stuck. Third, our evidence that stars are penalized after short spells of entrepreneurship adds to the debate about the need for more entrepreneurial quality or quantity, which has recently shifted toward quality (e.g., Guzman and Stern, 2017; Belenzon et al., 2017). On the one hand, we caution highly paid employees against using entrepreneurship as an experimentation device (e.g., Kerr et al., 2014), as it bears the risk of a substantial wage penalty; on the other hand, we encourage workers with low ability signals to test the entrepreneurial waters. Overall, this paper improves our understanding of the costs associated with policy measures that promote experimentation with entrepreneurial ideas.

[11] This implication is particularly intriguing considering a reform in Belgium in 2014 that abolished this type of contract.

Chapter 2

The Wage Persistence Puzzle: Earnings Trajectories of Former Entrepreneurs*

2.1 Introduction

About 35 percent of individuals start a business at least once during their working life (Hincapié, 2020) and most of these entrepreneurs return to paid employment within five years (Dillon and Stanton, 2017). Despite the importance of the phenomenon, the consequences of this type of work experience are not well understood. Recently, entrepreneurship scholars have looked into the short-term labor market effects (e.g. Baptista et al., 2012; Koellinger et al., 2015). This empirical literature documents that entrepreneurs who return to paid employment typically incur a wage penalty at the time they re-enter the labor market (Bruce and Schuetze, 2004; Failla et al., 2017; Kaiser and Malchow-Møller, 2011; Mahieu et al., 2019), with a few exceptions (Hyytinen and Rouvinen, 2008; Luzzi and Sasson, 2016). Yet, the importance of such

* This chapter is based on joint work with Francesca Melillo (SKEMA Business School) and Peter Thompson (Georgia Intstitute of Technology). Earlier versions of the chapter have been presented at DRUID 2019, the 2019 annual meeting of the Academy of Management, and the 2019 SEI Consortium.

an earnings decline essentially depends on whether former entrepreneurs can quickly recover from these initial losses or not.

Surprisingly, very little is known about the longer-term effects of an entrepreneurial experience in paid employment. To the best of our knowledge, so far only Manso (2016), using US data, explored this question and finds evidence that the initial losses are temporary and the wages of former entrepreneurs catch up relatively quickly. However, since there is substantial heterogeneity among the self-employed (e.g. Hurst and Pugsley, 2011), and the outcomes of a spell of entrepreneurship may depend on the features of the labor market (Mahieu et al., 2019), he is careful to acknowledge the results may vary by empirical context, dataset, or time period analyzed. This is something that has also been pointed out by, for example, Åstebro (2012).

This article's primary contribution is therefore to document the magnitude and temporal pattern of the wage losses of former entrepreneurs compared to a counterfactual where they would have remained in wage work, up to five years after entrepreneurship. To do so, we rely on novel administrative matched employer-employee data from Belgium, a country characterized by a labor market with substantially higher employment protection and lower job mobility than the US, but similar to many Western European countries like the Netherlands, Germany, Italy, or France.[1] We construct a dataset that contains quarterly earnings histories between 2000 and 2016 for a large comprehensive sample of employees who become entrepreneurs between 2004 and 2016, and a matched sample of workers who stay in paid employment during that period.

In line with previous studies, we find that former entrepreneurs incur significant losses at the time of re-entry in the wage sector. We also document a remarkable persistence to these losses: the entrepreneurs' relative wages only slightly recover in the first year after they have returned to wage work, and the bulk of the losses persist for years afterwards. These long-term losses are split between a penalty in terms of hours worked per quarter (60 percent of the penalty) and a penalty in terms of a lower daily wage (40 percent of the penalty). On average, former entrepreneurs earn 30 percent less per quarter and earn a full-time-equivalent daily wage that is 14 percent lower than their

[1] OECD indicators of employment protection:
http://www.oecd.org/employment/oecdindicatorsofemploymentprotection.htm

matched counterparts five years after exiting entrepreneurship, and there is no evidence of a catching up effect within the sample period. We find that former entrepreneurs are more likely to start part-time or temporary jobs, and to change jobs. These factors explain much of the penalty in terms of hours worked per quarter but none of the daily wage loss. Our findings suggest a persistent wage penalty: former entrepreneurs earn significantly less than similar employees without entrepreneurial experience years after exiting entrepreneurship.

The existence of a persistent wage penalty for returning entrepreneurs affects our understanding of entrepreneurial earnings in several related ways. First, studies that compare entrepreneurs' earnings with those of their wage-earning counterparts will systematically overestimate the expected present value of entering entrepreneurship if post-entrepreneurship earnings are not measured. Second, it provides an explanation for Hamilton (2000) finding that entrepreneurs persist in running their businesses despite earning less than observationally equivalent workers. Third, it makes explanations of the private equity premium puzzle (cf. Moskowitz and Vissing-Jørgensen, 2002) that rely on the real option value of being able to return to wage work (e.g. Vereshchagina and Hopenhayn, 2009) less compelling.

Having documented the persistence of the wage penalty, we then explore how it varies across groups within the sample, in an attempt to identify potential explanations. Section 2.4 focuses on two factors that we interpret as the result of market frictions. The first of these is the tendency of a significant fraction of returning entrepreneurs to maintain an active involvement in their business while also earning a wage, perhaps driven by difficulties in both selling and closing an underperforming business. We call such returnees hybrid entrepreneurs, and find that controlling for hybrid status explains all the remaining hours worked penalty. However, hybrid status explains none of the daily wage penalty. Second, we explore differential wage penalties for individuals that became entrepreneurs out of necessity and those that did so to pursue an opportunity. Necessity entrepreneurs are often driven into business creation because of limited labor market options, and in a frictional world, these individuals may continue to face the same limited options when the time comes to return to wage work. However, accounting for necessity and opportunity entrepreneurship has no explanatory power for the daily wage penalty.

Section 2.5 focuses on factors that we interpret collectively as indicators of signaling problems – whether this is about an entrepreneurs' ability or the expected quality of the match between firm and former entrepreneur. We argue later that signaling problems are likely to be more severe when an entrepreneur re-enters the workforce in a different industry than previously, when they are older, when they begin employment in a larger, more bureaucratic firm, and when their pre-entrepreneurship wage and entrepreneurial earnings provide conflicting signals about ability. We find all of these factors influence the wage penalty. However, there remains an economically meaningful wage penalty for most entrepreneurs that is unexplained. This is the persistent wage penalty puzzle.

2.2 Empirical Setting and Data

2.2.1 The Belgian Data

To investigate the earnings trajectories of former entrepreneurs, we use a matched sample of entrepreneurs and employees, extracted from the Belgian Datawarehouse Labor Market & Social Protection (DWH LM&SP). The Datawarehouse contains quarterly socio-economic data from all Belgian social security institutions, and provides a wealth of demographic information on the full Belgian population, as well as detailed information about their employers, employment status, and income for all quarters between 2000 and 2016. In terms of labor market and entrepreneurial dynamics, Belgium is comparable to various Western European countries, such as France, Germany, Finland, and Sweden and thus complement previous studies using data mainly from the US and Denmark (Manso, 2016; Failla et al., 2017).

These administrative data have several advantages over data used in similar studies. First, we have a larger sample of entrepreneurs than are followed in typical household surveys like the PSID or the National Longitudinal Surveys (NLS) in the US, or the European Household Community Household Panel, used in previous studies to estimate the returns from a spell of entrepreneurship (e.g Bruce and Schuetze, 2004; Daly, 2015; Dillon and Stanton, 2017; Hyytinen and Rouvinen, 2008; Manso, 2016). This allows us to apply statistical techniques frequently used in the program-evaluation literature to more reliably estimate the cost of entrepreneurial experience, including the cost related to wage growth that workers would have received in the absence of a spell of entrepreneurship.

Second, we are able to track workers' quarterly wages over a relatively long period of time, between 2000 and 2016. This allows us to examine the evolution of earnings from short-term to long-term. Most studies estimating the returns to entrepreneurial experience focus either on estimating the short-term earnings – i.e. the wage in the year of re-entry in the wage sector – or the lifetime earnings.[2]

Third, an important variable in the DWH LM&SP is the nomenclature used to describe a person's socio-economic status. Based on information from all the participating social security institutions, each quarter an individual is assigned to a code that best represents his position in the labor market (for example, whether he is solely working in wage work, self-employment, or both), thereby offering a fine-grained picture of the individual labor market status [3]. This allows us to date with high accuracy the quarter in which some workers separate from their employers and become entrepreneurs, and vice versa. Therefore, we are able to also capture short entrepreneurial spells, something that is not fully possible with annual data. We also know whether a worker is simultaneously employed in wage work and self-employed, so-called hybrid entrepreneurship (Folta et al., 2010). This allows us to examine to what extent gradual selection in and out of entrepreneurship impacts workers' wages.

[2]One exception is Campbell (2013) who estimates the returns in a similar manner as we do. However, he focuses on startups in the Californian semiconductor industry during a period of economic boom and high intensity of venture capital investments in that sector, which may positively impact the returns to entrepreneurial experience. Also, the study does not distinguish between the returns of entrepreneurs who remain self-employed, and those who exit and re-enter the wage sector. Last, his data doesn't allow to distinguish between founders and individuals who join startups as early employees. This may be problematic as entrepreneurs and joiners seem to be rather distinct groups with unique differences in motivations (Sauermann, 2018)

[3]A description (in Dutch) of the complete nomenclature is available on: http://www.bcss.fgov.be/nl/dwh/

2.2.2 Sample Construction

In line with previous papers investigating the causes and consequences of a spell of entrepreneurship (e.g. Failla et al., 2017; Sørensen, 2007; Nanda and Sørensen, 2010), our initial sampling population consists of all full-time employees working in one job in the first quarter of 2004, between the age of 22 and 49, not working in the primary sector at that moment in time, and without entrepreneurial experience between 2000 and 2004. We also restrict our sample to inhabitants of Belgium between 2004 and 2016, to avoid losing observations because of individuals moving abroad. We put the age restriction to avoid that we do not observe individuals during the whole sampling period because they still are in education or are close to retirement. We exclude employees in the primary sector, since dynamics of entrepreneurial activity may be substantially different in this sector and to be comparable with previous studies (Nanda and Sørensen, 2010; Özcan and Reichstein, 2009). Finally, we exclude those with entrepreneurial experience because the dynamics of serial entrepreneurship are likely to be distinct from those of first-time entrepreneurship (Westhead and Wright, 1998).

We assign individuals who become entrepreneurs at some point between 2004-2016 to the treatment group and those who instead remain solely in wage employment during that time frame in the control group. We label individuals as "entrepreneurs" if, at some point during the sample period, their socio-economic status code switches to "working as self-employed". This group entails all individuals whose only source of income comes from entrepreneurial activities. This means that we exclude from the entrepreneurs group individuals who, for some time, simultaneously work in wage work and self-employment but who never make the transition to being solely self-employed. Since our aim is to estimate the influence of a spell of entrepreneurship on future wages, keeping these individuals in the sample might introduce a confounding effect because we would not be able to tease out the impact of entrepreneurship from changes in their salaried job conditions.

From this initial population, we use propensity score matching to select all pairs of employees who became entrepreneurs and employees with the same ex ante probability of transitioning into entrepreneurship, but who remained continuously employed. The motivation behind using a matched sample of employees with similar backgrounds and wage trends between 2000 and 2004 is that it reduces concerns that any diverging career or wage trajectories between entrepreneurs and their counterparts are due to (un)observable differences that are either time-invariant or caused by events in the periods before 2004. Therefore, this setup allows us to at the same time verify if entrepreneurs' careers potentially diverge from employees who are comparable to them, but also to explore when and how this happens. This is particularly important as events that are conducive to becoming entrepreneurs may also explain the ex-post earnings trajectories. We discuss in detail the variables used in the matching procedure, and the main balance statistics that corroborate the success of our matching procedure in Section 1.4 of Chapter 1.

Since we are interested in estimating workers' wages after entrepreneurship, the focal sample in this study are the matched pairs of whom the entrepreneur exits out of entrepreneurship and goes back for at least one quarter to paid employment. These are in total 19,704 individuals, around 30% of the full matched sample of 64,473 individuals. About 65% of all the entrepreneurs remains self-employed until the end of the sampling period, while circa 5% exits entrepreneurship but does not return to wage work within the sampling frame. The relatively low exit numbers compared to samples used in other studies (e.g. Manso, 2016; Dillon and Stanton, 2017) can in part be explained by the high costs of terminating a business in Belgium, which lowers the threshold of exiting (Gimeno et al., 1997).

Finally, we restrict the sample to entrepreneurs who take a job with a firm different than the one they had worked for pre-entrepreneurship. This is because the process of hiring and retaining entrepreneurs returning to the same employer is likely to be structurally different from the one of entrepreneurs taking on a job at a different firm (Shipp et al., 2014; Swider et al., 2017) and thus previous work finds no wage penalty for these entrepreneurs (Mahieu et al., 2019). After imposing these sample restrictions, we retain a sample of 16,542 entrepreneurs and their matched counterparts. We observe these individuals for 68 quarters between 2000 and 2016.

2.2.3 Variables

Our dependent variables are measures of employee earnings, which we operationalise in two ways. The first is the logarithm of a worker's 'normal' remuneration in each quarter, *Ln(Real Quarterly Wage)*, where 'normal' remuneration includes any (pretax) remuneration that is not a severance payment or a bonus. Quarterly wages are reported in 2004 Euros. The second is *Ln(Real Daily Wage)*, the natural logarithm of a worker's average daily wage in paid employment in a certain quarter. The average daily wage is measured as the reported quarterly wage divided by the number of paid full-time working days per quarter for full-time workers, or as the quarterly wage divided by the number of hours worked times 7.6 for part-time workers (i.e. we assume a 38-hour week for full time employees).

The main independent variables we employ, other than those used in our matching procedure, are as follows:

Time Since Entrepreneurship is a series of dummy variables indicating the number of quarters since a person has exited entrepreneurship and returned to wage work (or the number of quarters until a person leaves wage work for entrepreneurship for the quarters before the entrepreneurial spell). We define a spell of entrepreneurship as all the subsequent quarters an individual's socio-economic status equals "working as self-employed". This means that their only source of income comes from entrepreneurial activities. Hence, we start counting the quarters since entrepreneurship from the moment an entrepreneur starts earning a wage in paid employment again, even if he is still registered as self-employed. Likewise, we define a person's last quarter in wage work before entrepreneurship as the last quarter in which he is in wage work, even when he is already registered as self-employed.

Entrepreneurial Earnings indicates an entrepreneur's reported annual income. This concerns the professional income minus operating expenses and losses. The annual income can be zero if the entrepreneur actually had an income of zero or if he had a negative income.[4]

Employer Change is a dummy equaling one if a person starts working for

[4]The Social Security of the self-employed automatically reports negative incomes as zero. Therefore, we do not know for entrepreneurs who made a loss in a certain year, the magnitude of these losses

a new employer in a certain quarter. This includes employer-to-employer moves, self-employment-to-employer moves, and moves from unemployment to employment. We use this variable to verify if an entrepreneur's wage losses depend on his job-hopping behavior after entrepreneurship.

Job contract type is an indicator for whether the worker has a full-time, part-time, or special contract. The category Special encompasses employees with very short / irregular contracts (interim, seasonal, occasional work in agriculture, and occasional work in hospitality).

Hybrid is a dummy indicating whether a person's socio-economic status equals "working in paid employment and as self-employed" in a certain quarter. These are entrepreneurs who are still working in paid employment while establishing their business, or who return to wage work while still retaining their business.

2.2.4 Descriptive statistics

Table 2.1 presents descriptive statistics for the sample of entrepreneurs who return to wage work and their matched employees. Panel A and B of Table 2.1 report demographic and employment characteristics in the quarter before the entrepreneurs enter into entrepreneurship (or the equivalent quarter for the matched employees). The figures resemble those reported by previous studies of entrepreneurship. A majority of entrepreneurs are in their thirties, tend to be male, and work for small and private employers at the moment of entry into entrepreneurship. Most individuals live in Flanders, the region with the highest economic growth in the last decades. On average, entrepreneurs earn 5655 Euros in the last quarter before entry into entrepreneurship, or circa 109 Euros on average per day, which is significantly less than the matched employees. This indicates that in the periods between 2004 and entry into entrepreneurship, entrepreneurs' wages on average grow slower than those of the control employees. However, the entrepreneurs' wages also exhibit greater variance, which is in line with the findings of Åstebro and Thompson (2011) that entrepreneurs are more likely to be drawn from the tails of the wage and ability distribution. When comparing mean quarterly wages before entrepreneurship (Panel B of Table 2.1) with average earnings in entrepreneurship (Panel C), we observe that the typical entrepreneur earns significantly less than the typical wage worker but entrepreneurial earnings have a much higher variance, corroborating key stylized facts in the literature on the returns to entrepreneurship (cf. Åstebro and Chen, 2014, for an overview).

While all individuals in our sample were working full-time in wage employment in 2004, the descriptive statistics in Panel B of Table 2.1 show that around 18 percent of our entrepreneurs works in a non-full-time position right before entrepreneurhsip, compared to only 7 percent of the control employees. Also, about 34 percent of the observations has a hybrid status right before entry, in line with the findings of Folta et al. (2010). Finally, previous studies (Astebro et al., 2011; Failla et al., 2017), document that prospect entrepreneurs have a tendency to job hop. Consistently, Table 2.1 indicates that around 9 percent of the entrepreneurs in our sample had started working for a new employer in the quarter before they entered into entrepreneurship, compared to only 2 percent of the control employees.

A more interesting and novel picture emerges when considering the temporal patterns of these employment behaviors of entrepreneurs. First, to gain a deeper understanding of the non-full-time and hybrid employment dynamics, we plot the job contract type and hybrid variables by the time to entry into entrepreneurship (or time to entrepreneurial entry of the matched entrepreneur in case of the control employees). Figure 2.1a shows that an increasing share of entrepreneurs found their businesses while still working in a job, and gradually select into non-full-time positions closer to the date of full-time entrepreneurship. Intriguingly, an analogous picture can be observed after the entrepreneur re-entered the wage sector. Almost 60 percent is still self-employed when starting to work for an employer after full-time entrepreneurship, and more than 45 percent returns to a non-full-time job after entrepreneurship. These numbers decline sharply in the first periods after re-entry in wage work. Yet, even six years after entrepreneurship, former entrepreneurs still have a considerably higher likelihood of working in part-time or temporary jobs, compared to their matched counterparts, and compared to the periods before they became entrepreneur.

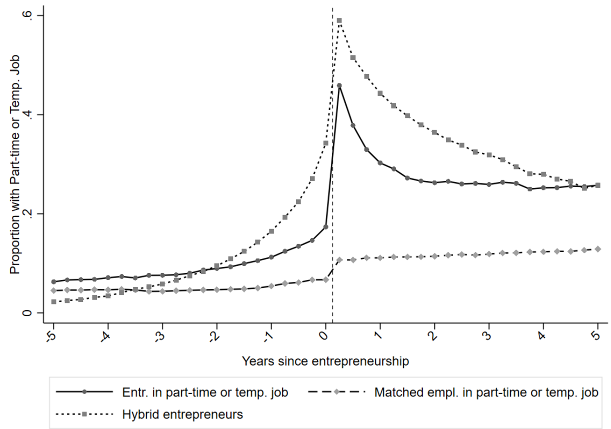
Second, while previous studies only look at the pre-entry average job hopping trend, Figure 2.1b plots the employer changing rates over time to document temporal patterns. Similar to Figure 2.1a, future entrepreneurs change jobs and employers more often closer to the date of entry into entrepreneurship, with a surge in the period right before they transition to self-employment. Furthermore, the rate of job moves by former entrepreneurs peaks in the in the first year after they have re-entered wage work. This rate only gradually decreases in the years afterwards, and remains significantly higher than the employer changing rates of the matched counterparts in the control group. While the job hopping tendency of prospective entrepreneurs has been attributed to a taste for variety or intent to develop generalist skills (Åstebro and Thompson, 2011; Lazear, 2005), the documented surge right before entrepreneurship seems to suggest that these individuals experience a job mismatch (Astebro et al., 2011) as we will further investigate in the multivariate analysis.

Returning to the summary statistics in Table 2.1, Panel D reports average and median quarterly and daily wages of entrepreneurs and their matched counterparts at different moments in time after the entrepreneurs have re-entered paid employment. There are two main findings: first, and in line with previous work, we see that entrepreneurs incur a significant wage penalty in the short-term after re-entry; one year after entrepreneurship, the average entrepreneur earns a wage of circa 5450 Euros per quarter (108 Euros per day), while the average employee in the control group earns almost 7000 Euros per quarter (121 Euros per day). Second, and more surprising, is that this initial wage penalty appears to be persistent over time. Five years after entrepreneurship, the wage of the average entrepreneur has increased only up to 5780 Euros per quarter (109,5 Euros per day), slightly more than what he earned right before entering entrepreneurship, and well below the wage of the average employee in the matched control group.

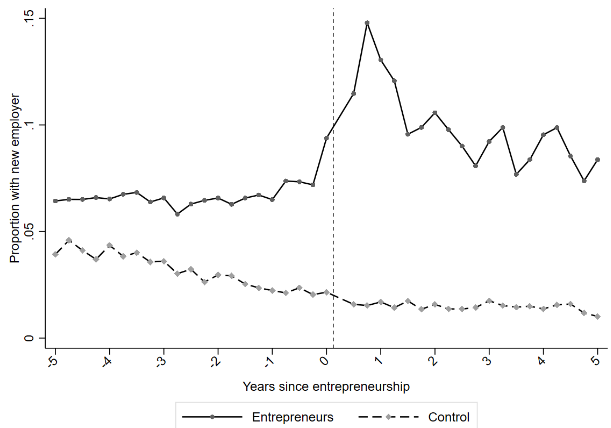
Table 2.1: Descriptive statistics for the sample of entrepreneurs returning to wage work and their matched employees.

	Entrepreneurs			Matched Employees		
	mean	median	sd	mean	median	sd
Panel A - Demographics						
Age						
22-24	0.015	0	0.12	0.017	0	0.13
25-29	0.176	0	0.38	0.174	0	0.38
30-34	0.296	0	0.46	0.289	0	0.45
35-39	0.240	0	0.43	0.242	0	0.43
40-44	0.163	0	0.37	0.162	0	0.37
45-49	0.080	0	0.27	0.085	0	0.28
50-54	0.026	0	0.16	0.028	0	0.16
55-59	0.004	0	0.06	0.004	0	0.06
Male	0.771	1	0.42	0.771	1	0.42
Household position	ref.			ref.		
Living with parents	0.096	0	0.29	0.103	0	0.30
Single	0.143	0	0.35	0.125	0	0.33
Cohabiting - no children	0.171	0	0.38	0.176	0	0.38
Cohabiting - 1 child	0.204	0	0.40	0.210	0	0.41
Cohabiting - 2 children	0.240	0	0.43	0.250	0	0.43
Cohabiting - 3 or more children	0.091	0	0.29	0.085	0	0.28
Head 1 parent family - 1 child	0.015	0	0.12	0.015	0	0.12
Head 1 parent family - 2 or more children	0.011	0	0.11	0.011	0	0.10
Household resident	0.010	0	0.10	0.010	0	0.10
Other	0.018	0	0.13	0.015	0	0.12
Region	ref.			ref.		
Flanders	0.766	1	0.42	0.771	1	0.42
Wallonia	0.211	0	0.41	0.209	0	0.41
Brussels	0.023	0	0.15	0.020	0	0.14
Panel B - Employment						
Real quarterly wage (in 2004 EUR)	5654.918	5215	3988.39	6564.455	6023	2875.68
Real average daily wage (in 2004 EUR)	108.872	103	51.28	114.885	106	40.62
Job contract type	ref.			ref.		
Full-time	0.824	1	0.38	0.933	1	0.25
Part-time	0.136	0	0.34	0.064	0	0.25
Special	0.039	0	0.19	0.003	0	0.05
Hybrid	0.342	0	0.47	0.000	0	0.00
Employer tenure	16.521	14	13.16	22.755	22	13.20
New employer	0.092	0	0.29	0.022	0	0.15
Occupation	ref.			ref.		
Blue-collar worker	0.490	0	0.50	0.468	0	0.50
White-collar worker	0.493	0	0.50	0.487	0	0.50
Govt. official	0.017	0	0.13	0.045	0	0.21
Employer size	ref.			ref.		
< 5	0.141	0	0.35	0.081	0	0.27
5-9	0.103	0	0.30	0.085	0	0.28
10-19	0.113	0	0.32	0.106	0	0.31
20-49	0.161	0	0.37	0.160	0	0.37
50-99	0.083	0	0.28	0.094	0	0.29
100-199	0.073	0	0.26	0.087	0	0.28
200-499	0.088	0	0.28	0.115	0	0.32
500-999	0.057	0	0.23	0.065	0	0.25
>= 1000	0.180	0	0.38	0.207	0	0.40
Employer sector	ref.			ref.		
Private	0.943	1	0.23	0.915	1	0.28
Public	0.057	0	0.23	0.085	0	0.28
Employer Industry	ref.			ref.		
Manufacturing	0.217	0	0.41	0.266	0	0.44
Electricity, gas, water	0.002	0	0.04	0.002	0	0.05
Construction	0.167	0	0.37	0.154	0	0.36
Wholesale and retail trade	0.216	0	0.41	0.218	0	0.41
Hotels and restaurants	0.026	0	0.16	0.015	0	0.12
Transport, storage, communication	0.081	0	0.27	0.090	0	0.29
Financial institutions	0.028	0	0.17	0.031	0	0.17
Real estate and professional services	0.162	0	0.37	0.118	0	0.32
Public administration, defence	0.019	0	0.14	0.029	0	0.17
Education	0.017	0	0.13	0.018	0	0.13
Healthcare and support services	0.045	0	0.21	0.044	0	0.21
Social and cultural services	0.018	0	0.13	0.015	0	0.12
Panel C - Entrepreneurship						
Duration of entrepreneurial spell	12.493	10.00	9.58			
Yearly entrepreneurial income	16011.067	11750.00	61083.88			
Panel D - Subsequent Outcomes						
Real Quarterly Wage (in 2004 Euros)						
1st quarter after entrepreneurship	3164.368	2454.92	2968.26	6954.906	6325.90	3221.97
1 year after entrepreneurship	5454.905	5098.85	3487.25	6957.772	6312.07	3276.21
2 years after entrepreneurship	5590.664	5189.56	3524.45	6985.657	6344.80	3262.92
5 years after entrepreneurship	5780.086	5395.96	3466.29	7051.980	6412.29	3349.73
Real Daily Wage (in 2004 Euros)						
1st quarter after entrepreneurship	103.876	94.19	46.69	121.158	110.83	45.24
1 year after entrepreneurship	107.976	97.94	48.57	121.269	110.83	46.80
2 years after entrepreneurship	108.730	98.81	47.03	121.694	111.25	46.03
5 years after entrepreneurship	109.535	102.06	47.33	122.676	113.39	47.57
Individuals		8271			8271	

Notes: The statistics in *Panel A and B* refer to the last quarter before entry into entrepreneurship (or the equivalent quarter in case of the matched employees).



(a) Rates of part-time or temporary jobs, and hybrid entrepreneurs



(b) New employer rates

Figure 2.1: Proportion of focal entrepreneurs and their matched counterparts in part-time or temporary jobs, and proportion of hybrid entrepreneurs (Figure 2.1a), and proportion of entrepreneurs and their matched counterparts starting to work for a new employer (Figure 2.1b) by time since entrepreneurship. Since, by definition, all entrepreneurs start working for a new employer in the first quarter of re-entry in wage employment, we omit this quarter for graphical reasons in Figure 2.1b.

2.3 Estimation

The descriptive statistics document (unconditional) wage losses for entrepreneurs in both the short- and long-terms,. However, we also observed important dynamics that may affect the estimates of the earnings trajectories of former entrepreneurs. In this section, we will explicitly take these factors into account, to see how they affect the apparent persistent wage losses for former entrepreneurs.

2.3.1 Methodology

In the analysis, we adopt the methodology of Jacobson et al. (1993) to calculate estimates of wage loss. These estimators have been previously applied in combination with propensity score matching by Couch and Placzek (2010) to estimate the earnings losses of displaced workers, and by Campbell (2013) to measure the returns to start-up experience in California's semiconductor industry. We define entrepreneurs' wage losses as the difference between their actual and expected wages had the events that led to their spell of entrepreneurship not occurred. More precisely, our definition of the wage loss is the difference in expected wages at a certain date between individuals who experience a spell of entrepreneurship and the matched individuals who remain in wage work throughout the sample period. This definition of wage losses allows the events that lead workers to become entrepreneurs to affect wages prior to entry in entrepreneurship. Additionally, this definition compares entrepreneurial entry at a certain date to an alternative that rules out a spell of entrepreneurship at that date, and at any time in the future.

To estimate wage losses corresponding to this definition, we specify the following statistical model to represent workers' wage trajectories:

$$w_{it} = \alpha_i + \beta_t + \sum_{k \geq -20} D_{it}^k \delta_k + \epsilon_{it} \tag{2.1}$$

where w_{it} is the natural logarithm of the quarterly (daily) wage of employee i in quarter t. The β_t's are dummies capturing the general wage trend within the sample. The dummy variables D_{it}^k, $k = -20, -19, ..., -2, -1, 0, 1, 2, ...$, jointly indicate time since entrepreneurship. In particular, we let $D_{it}^k = 1$ if, at date t, individual i has exited entrepreneurship k quarters before. Similarly, if k is negative, individual i completely left wage work and entered entrepreneurship $-k$ quarters later[5]. This choice ensures that we compare different cohorts of entrepreneurs' wages to a common standard and simplifies the interpretation of several of our empirical results. The parameters δ_k, therefore, summarize how wages differ from those more than 20 quarters prior to entrepreneurship. The reason to set the minimum value of k to -20 is that while the matching removed wage differences between entrepreneurs and non-entrepreneurs between 2000 and 2004 (cf. table A1), diverging trends might start occurring after 2004.[6] The "fixed effect" α_i captures permanent observable and unobservable characteristics of individuals, and that were potentially not accounted for in the matching procedure (like, for example, risk preferences). Robust standard errors, ϵ_{it}, are clustered at the individual level. We estimate Equation (2.1) applying the fixed-effects within-estimator.

Our estimation approach generalizes the "difference-in-differences" technique, which uses a comparison group to estimate the wage changes that would have occurred in the absence of entrepreneurship, by allowing the effects related to a spell of entrepreneurship to vary by the number of quarters relative to the entrepreneurial spell. Measuring wage losses related to entrepreneurship using this specification has several advantages over methods used in other studies to estimate the returns to entrepreneurial experience. For example, Daly (2015) compares the present discounted value of individuals' income following entry into entrepreneurship with earnings of observationally similar individuals who never become self-employed to examine if the difference is positive or negative. However, this approach does not take into account that the magnitude of the differences might vary over time, which may mask important temporal dynamics. Second, this method is only adequate if the earnings are not influenced by unobserved individual differences or time-varying factors.

[5]Alternatively, $D_{it}^k = 1$ if worker i entered entrepreneurship in quarter t - k.

[6]As a robustness test, we also set the minimum value of k to -24 and -28. This did not alter the results. This is not surprising, given that the matching removed observable differences between entrepreneurs and control employees between 2000 and 2004.

2.3.2 Results

In our empirical specification, wage losses associated with a spell of entrepreneurship are defined as the difference between the wages of employees before and after entrepreneurship and their expected wages had they remained in wage work throughout the sample period (matched group). Below, we report estimates of that difference for each quarter beginning with the 20th quarter prior to entrepreneurship and ending up with the 24th quarter after entrepreneurship. We graphically plot the estimated coefficients, along with their 95% confidence intervals, against the number of quarters before or after employees' spell of entrepreneurship. We choose not to visually show the estimated coefficients for the first quarter in wage work after entrepreneurship. In fact, the results show exceptionally large drops in this quarter (cf. also Panel D of Table 2.1), probably because individuals returning into wage work usually do not start working at the exact beginning of a quarter. Hence, including these coefficients would make the graphical representation and interpretation of the results rather cumbersome.

Figure 2.2 displays the estimated wage losses using the model specified in Equation 2.1, controlling for individual and time fixed-effects. Time 0 indicates the last quarter an individual is employed in wage work before becoming fully self-employed. The dashed vertical line marks the separation between the periods before and after entrepreneurship. We define the first period after entrepreneurship (time = 1) as the moment when the entrepreneurs, at least partly, again receive a wage in paid employment. This means that we "collapse" all the periods in between the moment of leaving paid work for entrepreneurship, and the moment of returning to the paid sector, since we do not observe wage information for the entrepreneurs during that time.

In line with previous studies (Bruce and Schuetze, 2004; Mahieu et al., 2019; Kaiser and Malchow-Møller, 2011; Failla et al., 2017), we find that entrepreneurs incur substantial wage losses at the time of re-entry into the wage sector. 4 quarters after entrepreneurship, we find that entrepreneurs earn circa 36 percent below their expected levels per quarter, and around 14 percent less on average per day. The difference between these two magnitudes is by construction a result of the much higher rate of part-time or temporary work undertaken by returning entrepreneurs, a fact already noted in Figure 2.1a. Thus, about 60 percent of the quarterly earnings is due to differences in hours worked, while the remaining 40 percent is due to the differences in the wage rate.

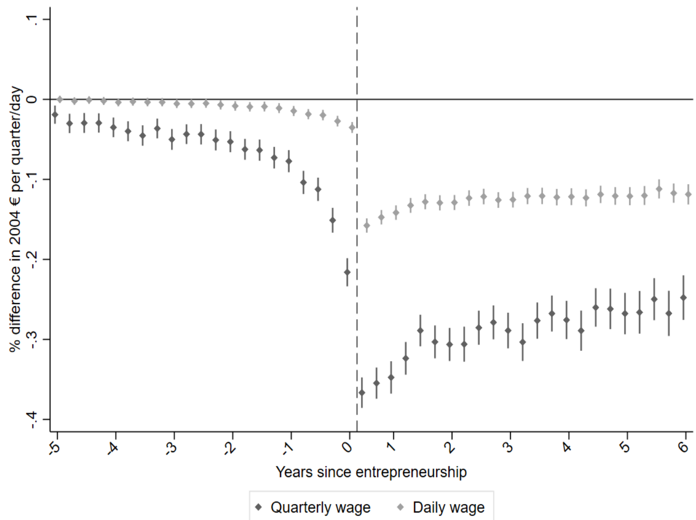


Figure 2.2: Daily wage losses of entrepreneurs returning to wage work. These and all subsequent regression coefficient plots include individual and time fixed effects. Vertical bars represent 95% confidence intervals.

Figure 2.2 shows that entrepreneurs already start losing out on wages in the quarters leading up to their full-time entry into entrepreneurship, and this is driven primarily by a decline in hours worked. Future entrepreneurs' wages start to diverge meaningfully from their expected levels around three years before entry into entrepreneurship, and the gap widens dramatically during the three quarters immediately. Clearly, some entrepreneurs are being pushed there by their current employment circumstances. This is not surprising. What is more surprising is that the initial wage penalty after entrepreneurship does not decay. Even five years after their spell of entrepreneurship, former entrepreneurs' estimated quarterly earnings remain around 27 percent below their expected levels and the daily wage is still about 12 below expectations. Thus, while the results indicate that entrepreneurs' wages slightly recover in the initial quarters after returning to wage employment, the estimated losses remain relatively stable in the years afterwards. The results suggest that former entrepreneurs' wages will not catch up to their expected levels, at least in the medium term.

Figure 2.2 documents that 60 percent of the earnings penalty results from a decline in hours worked post-entrepreneurship. Some of this decline will be attributable to working in part-time jobs, while some will be the result of a more frequent loss of continuity in employment within any quarter resulting from the higher job-switching rates of entrepreneurs (see Figure 2.1a and, Astebro et al. (2011)). If these factors are good predictors of hours worked, including them as controls will close the gap between our two measures of the wage penalty. Of course, the daily wage is also not necessarily independent of the type of work being done by entrepreneurs. Thus, if these controls are also correlated with unobserved traits that are negatively related to wages, we will also see a reduction in the residual penalty as measured by the daily wage.

In Figure 2.3, therefore, we include indicators for whether an individual is working in a full-time, part-time, or temporary job, and whether he changed jobs in that quarter. As expected, the large difference between the two measures of the wage gap is considerably reduced, even though our use of dummies for these controls leaves room for variation within each category (for example, entrepreneurs in part-time jobs work fewer hours on average than workers in part-time jobs). That is, job-switching frequency, and employment in part-time and temporary work collectively explain a large fraction all the difference in hours worked between entrepreneurs and employees. However, these two controls explain none of the daily wage gap.

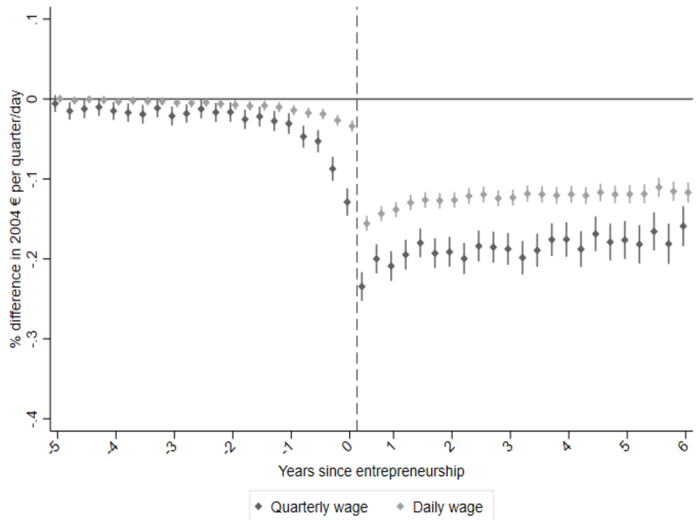


Figure 2.3: Quarterly (dark) and daily (light) wage losses of entrepreneurs returning to wage work, controlling for job contract type (full-time, part-time, temporary), and employer switches.

Another way to examine the persistence of the wage penalty is to estimate the extent to which earnings in the first job after re-entry can predict earnings five years later. In Table 2.2 we regress the quarterly and daily wage differences between entrepreneurs and their matched counterparts after five years on the initial difference when the entrepreneur re-enters wage work. The results in Column 1 indicate that a 10 percent increase in the initial quarterly wage penalty is related to only a 1.4 percent increase in the penalty after five years. Results for the daily wage in Column 2 show a much stronger relationship: a 10 percent increase in the initial penalty is associated with a 6 percent increase in the daily wage penalty after five years.

Table 2.2: The role of the first job after entrepreneurship

	(1) Quarterly penalty after five years	(2) Daily penalty after five years	(3) Quarterly penalty after five years	(4) Daily penalty after five years
Re-entry quarterly penalty	0.144***		0.142***	
	(0.015)		(0.015)	
Re-entry daily penalty		0.601***		0.599***
		(0.027)		(0.027)
Employer change			0.028	0.020
			(0.029)	(0.012)
Constant	0.297	-0.101	0.283	-0.111
	(0.613)	(0.095)	(0.603)	(0.102)
Observations	2,545	2,545	2,545	2,545
R-squared	0.277	0.395	0.277	0.395

Regressions of the quarterly and daily wage losses five years after entrepreneurship on the initial quarterly/daily wage losses at the quarter of re-entry in wage work. Controls include job change, job contract type, and hybrid status. Robust standard errors in parentheses. *** p<0.001, **p<0.01, *p<0.05.

The finding that the initial quarterly earnings penalty is much less persistent than the daily wage penalty suggests different mechanisms lie behind the hours worked and the daily wage penalties. Initial hours worked are a poor predictor of future hours worked, even though the sample mean hours worked are similar upon re-entry and after five years. This is consistent with churn among entrepreneurs, who move in and out of part-time work over the five-year post-entry period.

In contrast, the results for the daily wage penalty suggest that individual entrepreneurs face challenges in changing their circumstances if they are penalized upon returning to the wage sector. This challenge is not eliminated by switching jobs, either: in Columns 3 and 4 we include a dummy for whether the entrepreneur changed firm in the first five years after entrepreneurship, and find that job switching has no ability to undo the persistence of the initial daily wage penalty.[7]

[7] There is a well-established literature showing that initial job outcomes can have long-lasting effects. Baker et al. (1994) find that cohorts who earn less when they join a firm will continue to earn below-average wages years later. Oyer (2006) finds that Ph.D. economists who graduate during a downturn are less likely to obtain a placement at a high-ranked institution, and have lower productivity and a lower probability of working at a high-ranked institute fifteen years later. Schoar and Zuo (2017) find that managers who start their career during a recession are more likely to do so in a small, lower paid firm, and end up heading smaller firms and receiving lower wages years later. Similarly, Oyer (2008), Kahn (2010) and Oreopoulos et al. (2012) all document persistent earnings declines of graduating from college in a recession.

2.4 Heterogeneous penalties I: Frictional explanations

In this section and the next, we search for explanations for the observed earnings penalties by examining how they differ across subgroups of entrepreneurs. This section focuses on two potential explanations that we interpret as resulting from market frictions. The first of these, hybrid entrepreneurship, arises when entrepreneurs continue to operate their business despite returning to paid employment. We interpret hybrid entrepreneurship as a result of market frictions for two reasons: first, because agency problems may make it difficult to assign business operations to a paid employee, and second because entry costs create an option value for keeping an underperforming business open. Hybrid entrepreneurs may suffer an earnings penalty both through hours worked and the daily wage. Hybrid entrepreneurs may decide to work fewer hours in paid employment than entrepreneurs who completely exit self-employment, to be able to keep on dedicating part of their working hours on their own business, similar to entrepreneurs who enter self-employment via a hybrid route (Folta et al., 2010). This would lead to lower quarterly wages, but would not directly result in a lower daily wage rate. However, hybrid entrepreneurs are also more likely to take a job with fewer responsibilities than their ability and qualifications might otherwise allow, either by choice or because prospective employers view them as less committed, so that their daily wage may also be reduced.

The second frictional explanation we investigate is the extent to which the wage penalty is driven by returning necessity entrepreneurs, who were driven into entrepreneurship by limited job prospects that have not resolved themselves by the time the entrepreneur returns to wage work. Although we matched employees on their wage in 2004, the results so far consistently show a sharp drop in quarterly earnings and a small decline in the daily wage in the periods before entry into entrepreneurship. This suggests that a sizeable fraction of the entrepreneurs in our sample became self-employed out of necessity, as opposed to having discovered a valuable business opportunity and being motivated to pursue an entrepreneurial career (Astebro et al., 2011; Hurst and

Pugsley, 2011; Levine and Rubinstein, 2017, 2018; Schoar, 2010).

2.4.1 Hybrid Entrepreneurs

To test whether hybrid entrepreneurs work fewer hours than non-hybrid entrepreneurs, we regress hours worked in the first quarter in wage work after entrepreneurship on hybrid status. We also include contract type, to ensure the relationship between hybrid entrepreneurship and hours worked is not simply due to hybrid entrepreneurs being more likely to take on part-time or temporary contracts. Column 1 of Table 2.3 shows indeed that hybrid entrepreneurs on average work 14 hours fewer per quarter than non-hybrid entrepreneurs, controlling for contract type. Furthermore, when we restrict the sample to only entrepreneurs who work in part-time or temporary jobs in Column 2, we find that, on average, hybrid entrepreneurs work about 25 hours fewer per quarter.

Table 2.3: Hours worked and hybrid entrepreneurs

Hours worked	(1) Full sample	(2) Part-time and temporary workers only
Part-time	-124.891***	
	(3.118)	
Temporary	-88.736***	
	(3.716)	
Hybrid	-14.244***	-25.530***
	(2.989)	(4.093)
Constant	216.320**	278.863***
	(73.838)	(4.093)
Observations	8,127	3,731
R-squared	0.184	0.051

Regressions of hours worked in the first quarter in wage work after entrepreneurship on hybrid status and job contract type. Robust standard errors in parentheses. *** p<0.001, **p<0.01, *p<0.05.

Figure 2.4 plots the earnings penalty after controlling for hybrid status. Consistent with the evidence in Table 2.3, the gap remaining from Figure 2.3 between the quarterly earnings penalty and the daily wage penalty has been eliminated That is, adding an indicator for hybrid status fully explains the hours worked penalty. However, there is no difference between Figures 2.3 and 2.4 in the daily wage penalty, so hybrid entrepreneurs are not penalized on their daily wages.

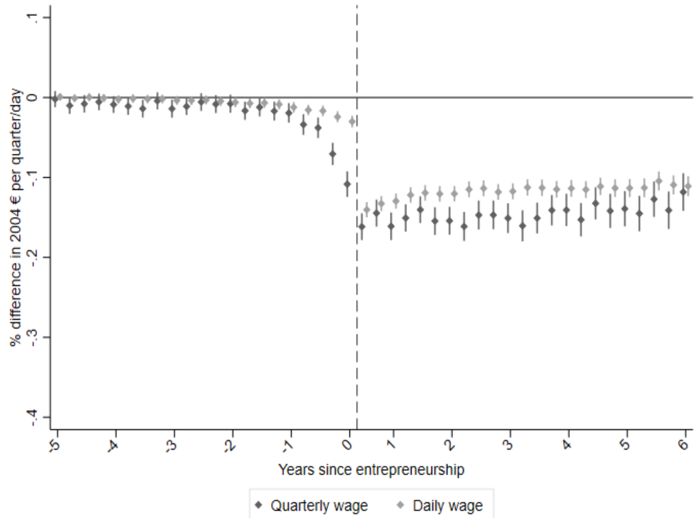


Figure 2.4: Quarterly (dark) and daily (light) wage losses of entrepreneurs returning to wage work controlling for *hybrid* status. Regressions include controls for job contract type and employer switches.

2.4.2 Opportunity and necessity entrepreneurs

To test whether necessity entrepreneurs drive the decline in pre-entry wages and wage losses after entrepreneurship, we follow Levine and Rubinstein (2018) who find that opportunity entrepreneurs are positively selected on wages, whereas necessity entrepreneurs are negatively selected on wages. The existence of both positive and negative selection into entrepreneurship leads to the stylized fact that entrepreneurs are drawn from the tails of the wage distribution (Astebro et al., 2011; Elfenbein et al., 2010). Accordingly, we re-estimate our model excluding the entrepreneurs in the bottom 25 percent of the wage distribution before entrepreneurship. In unreported analyses (available upon request), we also find that these entrepreneurs are mainly responsible for the higher job switching behavior immediately before entry, reinforcing the notion that these individuals select into entrepreneurship due to a lack of attractive job options in the regular labor market. In the regressions, we control for employer changes, job contract type, and hybrid status.

Figure 2.5 shows the estimated wage losses for the opportunity entrepreneurs in the top 75 percent of the pre-entry wage distribution. In line with our expectations, the picture shows that the previously observed wage losses before transitioning into entrepreneurship have now disappeared. Importantly, the figure shows that the persistence wage penalty is not the result of negative selection, as opportunity-driven entrepreneurs still incur significant losses after entrepreneurship which are very similar in magnitude to those observed in Figure 5 which included the necessity entrepreneurs.

In sum, it appears that the wages of necessity-driven entrepreneurs fall below their expected levels both before and after a spell of self-employment. In contrast, entrepreneurs coming from high-paying jobs experience a faster wage growth than observationally equivalent employees before entrepreneurship, yet they still incur significant wage losses after entrepreneurship. Therefore, to eliminate possible confounding effects of negative selection into self-employment that may upwardly bias the estimates of the wage losses, we restrict the sample from here onwards to entrepreneurs coming from the top 75 percent of the wage distribution.

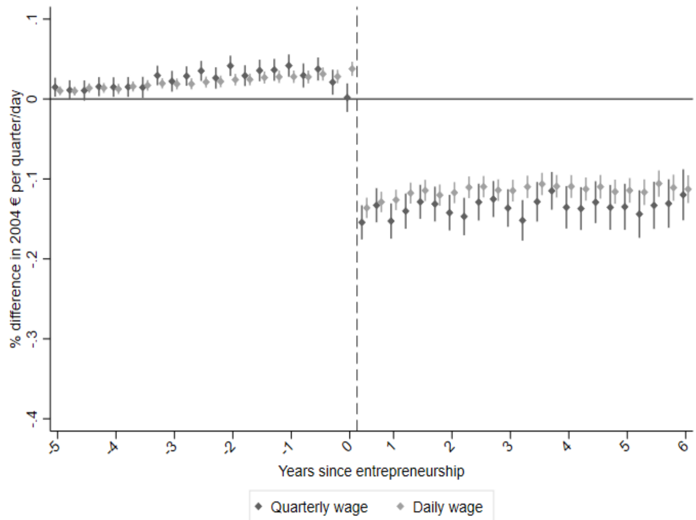


Figure 2.5: Quarterly (dark) and daily (light) wage losses of entrepreneurs returning to wage work, excluding necessity entrepreneurs (i.e., entrepreneurs with a wage immediately before entrepreneurship that is in the bottom 25% of the unconditional wage distribution). Regressions include controls for job contract type, employer switches, and hybrid status.

2.5 Heterogeneous penalties II: Signaling explanations

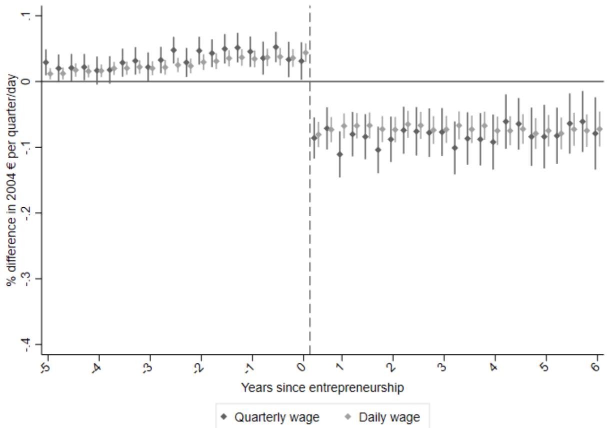
In this section, we examine the wage penalties across subgroups of opportunity entrepreneurs for which beliefs about their ability or about the match quality are likely to vary. We examine four factors that distinguish entrepreneurs by: (i) whether they returned to wage employment in an industry different from their pre-entrepreneurship employment spell; (ii) age; (iii) firm size; and (iv) their entrepreneurial earnings.

2.5.1 Industry Switching

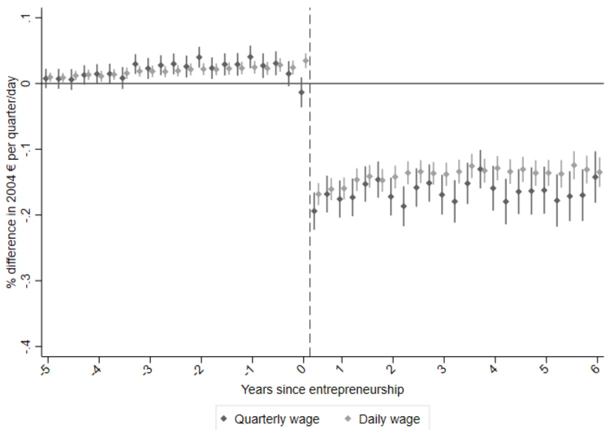
A substantial fraction of entrepreneurs become self-employed in an industry different than the one they were employed in before: in our data, almost 75 percent of all entrepreneurs changes industries (at the NACE 2-digit level).

Similarly, about 66 percent moves to a different industry after entrepreneurship than the one they were employed in before entering self-employment. We expect that industry switchers are more penalized compared to industry stayers. Lack of industry experience increases uncertainty in hiring (Kuhnen and Oyer, 2016). Moreover, an important part of the human capital is industry-specific (Neal, 1995), and thus entrepreneurs who change industries are more penalized (Eggers and Song, 2015; Kaiser and Malchow-Møller, 2011).

If industry switching accounts for part of the wage losses, then we would expect larger wage losses for entrepreneurs who start working in a different industry after entrepreneurship than those who start working in an industry they were employed in pre-entrepreneurship. We test for this possibility by estimating the wage losses separately for industry switcher and stayers. In Figure 2.6, we find that the wage losses are about 11 percent per quarter or 7 percent per day for entrepreneurs who return to the same industry after entrepreneurship as they worked in before entrepreneurship. This is roughly half of the penalty incurred by industry switchers: they incur wage losses of 19 percent per quarter or 15 percent per day. Hence, industry switching is indeed related to larger wage losses after entrepreneurship.



(a) Industry Stayers



(b) Industry Switchers

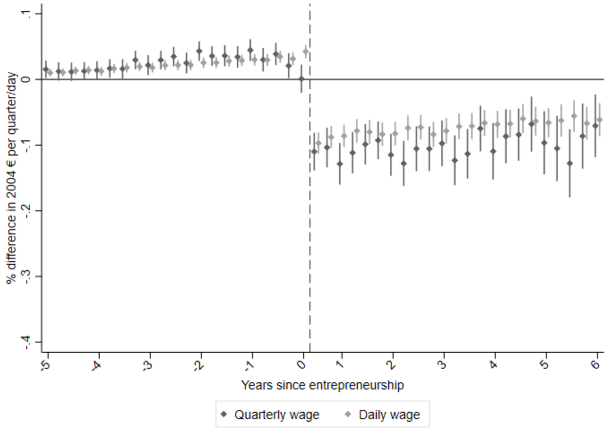
Figure 2.6: Quarterly (dark) and daily (light) wage losses of opportunity entrepreneurs returning to wage work, Panel A (B) reports coefficient estimates for entrepreneurs that start working in the same (a different) NACE industry after entrepreneurship as they worked in right before entering entrepreneurship (at the 2 digit level).

2.5.2 Age

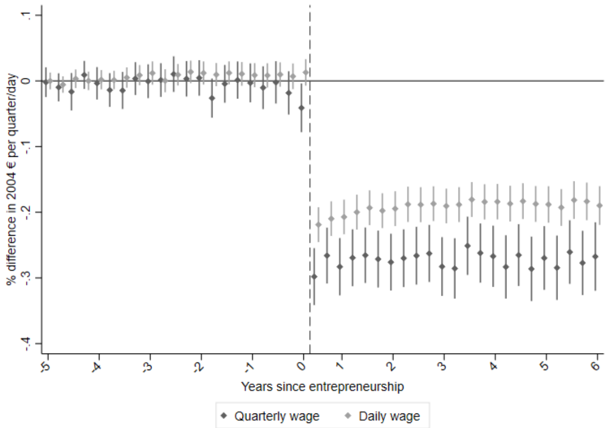
We next examine heterogeneity in the wage penalty according to the entrepreneur's age. We expect to observe a larger penalty for older workers.

Experimenting with job and careers is common in the early stage of an individual career, as a way to learn about their own skills (e.g. Antonovics and Golan, 2012). Experimentation with jobs among young workers is thus viewed more favorably by prospective employers in comparison to older workers. Hence, having experimented with an entrepreneurial career (e.g. Kerr et al., 2014) should be less penalizing for early stage career workers. Moreover, absent a clear signal in the regular labor market given the scarce work experience, the employer may not hold strong performance expectations, limiting the downsides in case of failure.

We test this by estimating the wage losses separately for the group of entrepreneurs who are 40 years or younger at the time of re-entry in wage work and for the group of entrepreneurs who are older than 40. The findings presented in Figure 2.7 show that the wage losses after five years out of entrepreneurship are 9 percent per quarter or 5 percent per day. This is significantly less than for older workers: entrepreneurs aged over 40 when re-entering wage work incur wage losses of 30 percent per quarter or 21 percent per day. This is in line with the idea that older entrepreneurs may have more difficulties in finding a suitable job.



(a) Age ≤ 40



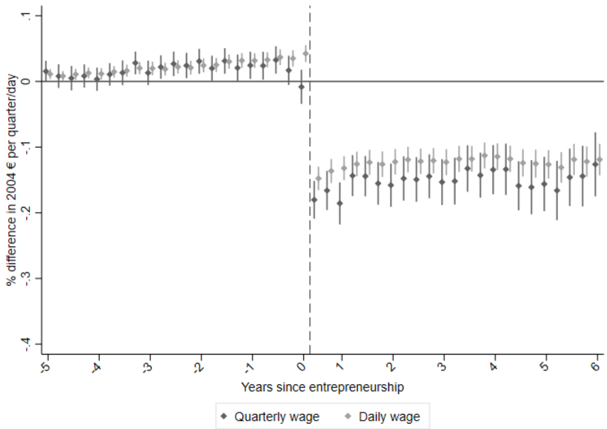
(b) Age > 40

Figure 2.7: Quarterly (dark) and daily (light) wage losses of opportunity entrepreneurs returning to wage work. Panel A (B) reports coefficient estimates for entrepreneurs aged younger than or equal to (older than) 40 years at time of re-entry into wage work.

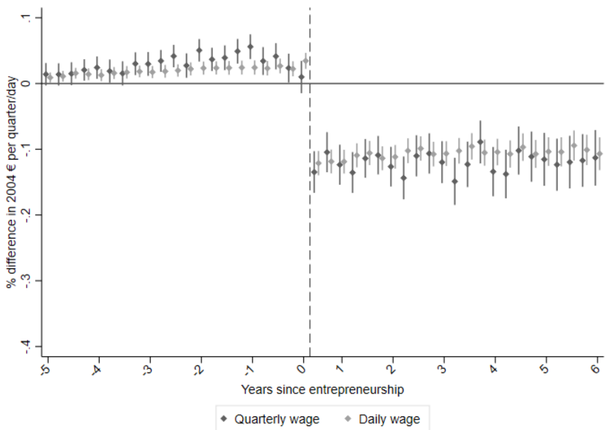
2.5.3 Firm Size

Entrepreneurs are more likely to come from small firms (Elfenbein et al., 2010; Sørensen, 2007) They are also more likely to return to small employers: in our data, 44 percent of the entrepreneurs start working for a firm with less than 50 employees, compared to 38 percent of the matched employees. This may be because of several reasons: small firms are more similar to the entrepreneurial context in terms of on-the job skill requirements with a range of skills that is broader than that typically required in a large firm where tasks are more narrowly defined. Also, small firms may offer more non-pecuniary benefits such as greater work autonomy, which may attract entrepreneurs who have been shown to have a preference for being one's own boss (Hamilton, 2000). In this sense, uncertainty regarding the fit of former entrepreneurs may be less of a concern among small employers. However, given that the marginal hire affects a larger relative share of total firm output in small firms, concerns regarding productivity may therefore be more pronounced (Mahieu et al., 2019). Also, because otherwise identical workers earn more when working for a large firm (Oi and Idson, 1999), the higher propensity of former entrepreneurs to work for a small firm may explain part of the wage losses.

We examine this possibility by separately estimating the wage losses for entrepreneurs that join firms with fewer or more than 50 employees. The results, in Figure 2.8, indeed indicate that entrepreneurs that return to small employers incur larger wage losses, although the magnitude of the difference is modest: on average, entrepreneurs that start working for a firm with fewer than 50 employees experience wage losses of 17.5 percent per quarter or 14 percent per day. Entrepreneurs moving to a firm with 50 or more employees earn 15 percent per quarter or 11 percent per day below their expected levels, five years out of entrepreneurship. Hence, while these results indeed suggest the size of the post-entrepreneurship employer captures a small fraction of the penalty, both entrepreneurs returning to small and to large firms continue incur substantial losses.



(a) n < 50



(b) n ≥ 50

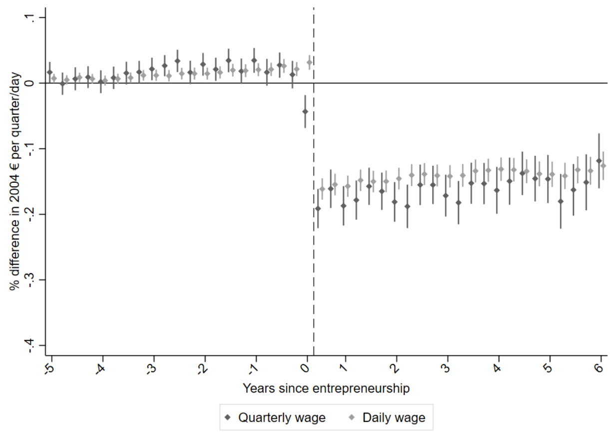
Figure 2.8: Quarterly (dark) and daily (light) wage losses of opportunity entrepreneurs returning to wage work. Panel A (B) reports coefficient estimates for entrepreneurs who start working for small (large) firms when re-entering paid employment.

2.5.4 Entrepreneurial Earnings

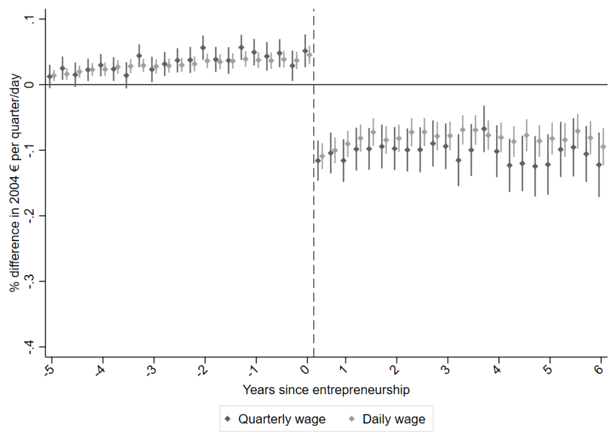
We now examine whether differences in entrepreneurial earnings can explain the persistent wage losses. It is possible, for example, that low entrepreneurial earnings are a signal of low ability not only in entrepreneurship but also in the wage sector, or that entrepreneurs with limited earnings face liquidity constraints that limits job search such that match quality is reduced.

To test for this, we compare the wage losses for entrepreneurs in the top and bottom 50 percent of the entrepreneurial earnings distribution. We do this for the restricted sample of entrepreneurs (and their matched counterparts) coming from the top 75 percent of the pre-entrepreneurship wage distribution. Panel A of Figure 2.9 displays the estimated wage losses for the entrepreneurs in the bottom 50 percent of the entrepreneurial earnings distribution. Five years after entrepreneurship they earn around 14 percent below their expected levels. The wage losses for low earners are significantly more than for the entrepreneurs in the top 50 percent of the entrepreneurial earnings distribution, whose penalties per quarter and per day are both about 10 percent. Hence, differences in entrepreneurial earnings explain part of the penalty, but not much.

One reason for the failure of entrepreneurial earnings to offer a signal about ability may be that that it is not independent of other signals. Our reading of the prior literature on the re-entry wage for former entrepreneurs (e.g. Baptista et al., 2012; Failla et al., 2017; Luzzi and Sasson, 2016; Mahieu et al., 2019; Manso, 2016), suggests that not only entrepreneurial earnings but also the pre-entrepreneurship wage may be used as signals of ability. For example, Mahieu et al. (2019) develop and test a signaling theory of the wage penalty, arguing that an entrepreneurial spell increases uncertainty around a job applicant's future productivity, so risk-averse employers offer former entrepreneurs a lower wage to compensate for the hiring risk. They predict that the so-called stars, i.e. entrepreneurs coming from the top of the pre-entry wage distribution (Astebro et al., 2011) are the riskiest hires, and, accordingly, find that stars incur a larger penalty at re-entry. If both pre- entrepreneurship wages and entrepreneurial earnings have value as signals of ability, it is unlikely that their effects on the wage penalty are independent of each other. For example, low entrepreneurial earnings are unlikely to induce a prospective employer to revise its prediction of an entrepreneur's ability downwards if it has already observed a low pre-entrepreneurship wage, but it may do so if the pre-entrepreneurship wage was high. To sum up, then, we conjecture that the pre-entrepreneurship wage is positively associated with the magnitude of the wage penalty, while entrepreneurial earnings are negatively associated with the penalty, but the sensitivity of the penalty to variations in entrepreneurial earnings is greater for individuals that earned a high pre-entrepreneurship wage.



(a) Bottom Earners



(b) Top Earners

Figure 2.9: Quarterly (dark) and daily (light) wage losses of opportunity entrepreneurs returning to wage work, Panel A (B) reports coefficient estimates for entrepreneurs with business earnings below (above) the median.

To test this conjecture, we regress the difference between entrepreneurs and their matched controls in the log of the daily wage at the time of re-entry on entrepreneurial earnings, the pre-entry wage and their interaction, controlling for time effects. We then plot the predicted values of the dependent variable against values of the two regressors. Figure 2.10 shows a contour plot of the results. Holding entrepreneurial earnings constant, we indeed observe that those with higher wages prior to entry into entrepreneurship are penalized more, consistent with Mahieu et al. (2019). Holding the wage before entrepreneurship constant, we also find that those earning more as entrepreneur have on average lower penalties. However, we also observe our expected interaction effect: differences in performance matter more for the stars than for those earning less before entry into entrepreneurship.

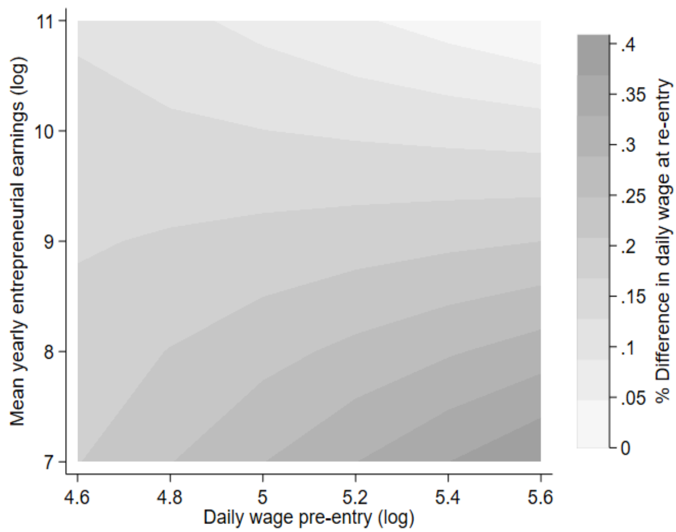


Figure 2.10: Contour plot of a regression of the difference in daily wage between entrepreneurs and their matched counterparts in the first quarter after entrepreneurship on the daily wage in the last quarter before entrepreneurship, yearly entrepreneurial earnings, and their interaction term for entrepreneurs with above median wages before entrepreneurship..

2.6 Conclusion

We find that former entrepreneurs incur substantial wage losses years after they have returned to paid employment. Part of these losses can be explained by former entrepreneurs working less hours per quarter than equivalent employees without entrepreneurial experience, because they are more likely to have non-full-time job contracts and to change employers. However, a significant fraction of the losses is due to former entrepreneurs earning a lower daily wage. We explore various explanations for this observation, which we categorize as frictional and signaling. While frictional explanations have no explanatory power, signaling-based explanations can explain some, but not all, of the daily wage penalty for most entrepreneurs. More specifically, entrepreneurs who change industries, are older, move to smaller employers, or have lower entrepreneurial earnings all incur higher losses. These individuals are arguably more uncertain hires, and will therefore incur a larger penalty in a context with strong employment protection (Mahieu et al., 2019). Yet, there remains an economically meaningful wage gap between employees with and without entrepreneurial experience that remains unexplained.

These findings add to the nascent literature estimating the long-term returns from entrepreneurial experience in paid employment (e.g. Manso, 2016). In particular, our results highlight that the returns may differ substantially across labor markets, and that concerns regarding productivity and fit of former entrepreneurs may be more pronounced in contexts characterized by high employment protection, and, accordingly, relatively little job mobility.

The finding that the magnitude of the losses at time of re-entry in wage work to a great extent predicts those years later also suggests that former entrepreneurs cannot easily change their circumstances. This bears some similarities to the theoretical and empirical literature showing that initial job outcomes can have long-lasting effects (e.g. Gibbons and Waldman, 2006; Oreopoulos et al., 2012; Oyer, 2006, 2008; Schoar and Zuo, 2017). Exploring the types of jobs and work contexts entrepreneurs move to after entrepreneurship might therefore be a fruitful avenue for future research to better understand what drives the observed wage gap.

Ultimately, our study also holds implications for nascent entrepreneurs. Whereas prior work has highlighted that entrepreneurship holds option value because one can always return to paid employment (Vereshchagina and Hopenhayn, 2009; Manso, 2016), our results suggest that the magnitude of this value is not independent from how well one performs in entrepreneurship, labor market status before entry, and how they relate to each other. In particular, for star employees, entrepreneurship might become a double-edged sword in the sense that they are more likely to perform well financially in entrepreneurship, but they will also be penalized most in the labor market if they turn out to be poorly performing entrepreneurs.

Chapter 3

Picking Up The Pieces: Natural Disasters, Firm Dynamics, and the Demand for Reconstruction*

3.1 Introduction

The magnitude and frequency of economic shocks caused by natural hazards are substantial and expected to rise over the coming years (IPCC, 2014). For example, in the United States, hurricanes alone caused more than $345 billion in damages between 2000 and 2015, more than any other type of natural hazard[1]. Global warming and the increasing sea surface temperature has also been associated with the growing number and magnitude of hurricanes in the North Atlantic (Webster et al., 2005; Elsner et al., 2008). Furthermore, the clustering of economic activity along the U.S. Atlantic coast has increased the share of the population at risk of exposure to hurricanes (Rappaport and Sachs, 2003; Pielke et al., 2008).

* This chapter is based on a single-authored working paper with the same title.
[1] Estimates from the National Oceanic and Atmospheric Administration (NOAA)

By now, a large body of work has examined the direct damages and the aggregate economic and labor market consequences of natural hazards and extreme weather, including how they affect employment and job creation[2]. Recent studies indicate that overall economic activity in affected regions appears to recover relatively quickly (Deryugina et al., 2018; Groen et al., 2019; Kocornik-Mina et al., 2020; Okazaki et al., 2019), but can also lead to persistent disruptions in severely damaged areas (Basker and Miranda, 2018; Boustan et al., 2020). Unsurprisingly, this recovery period in the initial years after a natural hazard is accompanied by a surging demand for workers in sectors related to rebuilding and recovery (Belasen and Polachek, 2009; Dolfman et al., 2007; Groen et al., 2019). Hence, the rate at which economies recover depends on the ability of firms engaged in these activities to address this increased demand (Noy, 2009; Hallegatte and Dumas, 2009; Crespo Cuaresma et al., 2008).

So far, the literature has mostly ignored the question which firms respond to this rise in local demand for reconstruction by employing more workers. In particular, previous studies have overlooked the potentially differential response in job creation through the formation of new firms to that by the expansion of existing firms. This is surprising, given that a growing theoretical and empirical literature has emphasized the role of new firm creation in understanding how economies respond to aggregate shocks (Adelino et al., 2017; Bernstein et al., 2018; Clementi and Palazzo, 2016; Decker et al., 2018; Sedláček and Sterk, 2017). Because startups are such important contributors to net job creation (Haltiwanger et al., 2013), possible barriers that limit firm entry may therefore mute employment growth and slow down recovery (Pugsley and Sahin, 2019). Furthermore, a closer examination of how startups and established firms respond to economic shocks caused by natural hazards has implications for models of firm dynamics, and contribute to our knowledge about the mechanisms underlying startup growth.

This paper examines how job creation in startups and existing firms in sectors related to rebuilding and recovery changes following a natural hazard. In the empirical analysis, I use county-level data for the North Atlantic Basin area on employment by firm age and sector that span all quarters in the period be-

[2] See Cavallo and Noy (2011); Kousky (2012); Dell et al. (2014); van Bergeijk and Lazzaroni (2015) for reviews of the literature.

tween 2000-2015. I estimate how employment changes in the years following a hurricane strike. To do so, I employ a staggered differences-in-differences framework, comparing counties that experience a hurricane between 2000 and 2015 with those that do not. This setup allows me to empirically examine the relative responsiveness in terms of job creation by new and existing firms to the increased demand for rebuilding work following a hurricane.

This approach rests on two key features of firms in sectors associated with rebuilding and recovery after a natural hazard. First, the Disaster Relief and Emergency Assistance Act imposes that expenditure of federal funds for debris clearance, distribution of supplies, reconstruction, and other major disaster or emergency assistance activities goes to firms located in affected regions. This minimizes concerns that also non-local firms experience a demand shock which could bias the results. Second, it is unlikely that in these sectors startups have a technological advantage over older firms (or vice versa) that would make them especially suited to address rising demand.

I find that local employment in sectors related to rebuilding and recovery increases by 4.3 - 7.6% one to three years after a hurricane strike. This is in line with the findings of previous papers that demand for labor in these sectors rises in the initial years after a natural hazard. I do not observe a similar increase in other sectors, which suggest that the employment effect in recovery sectors is not driven by county-wide changes in the labor market. Furthermore, employment also increases with the strength of a hurricane: counties experiencing wind speeds of 43 m/s or more (Category 2 on the Saffir-Simpson scale) see an estimated increase in employment of 10.7 - 18.9%. Counties experiencing wind speeds between 33 - 42 m/s (Category 1 on the Saffir-Simpson scale) only see an increase of 2.6 - 4.6%, but this effect is statistically insignificant. These results suggest hurricane damage, and, hence, the demand for rebuilding, rise non-linearly with wind speed (cf. also Emanuel, 2011).

When I disaggregate the results by firm age, I find several noticeable differences. I estimate that employment in startups increases significantly in the first four years after a hurricane, with a peak of almost 24% above expected levels after one to two years. This implies that job creation through new venture formation accounts for nearly 23% of excess total job creation in these sectors following a hurricane. Given that startups on average account for only

4.1% of total employment, this shows that startups disproportionally respond to local demand shocks. I do observe an increase in firms aged 6 years or older as well, but the estimated increase is much smaller: two years after a hurricane, employment is circa 9.1% above its expected levels, less than half of the increase in employment through new venture creation. This equals nearly 68% of excess job creation, although firms aged 6 years or older account for nearly 86% of total employment in a county, on average. On the other hand, I observe only small increases in job creation by firms aged between two and five years old, proportionate to their respective shares of total employment.

Furthermore, and similar to the findings at the aggregate level, when I split up the changes by strength of the hurricane winds, the results show that stronger hurricanes lead to larger employment increases: one year after a county is hit by a hurricane at Category 2 strength, startup employment goes up by almost 29%, whereas employment in firms of 6 years or older increases by nearly 16%. Again, there is no noticeable change in employment in firms aged 2 - 5 years.

Finally, the results also show that employment goes up in firms aged two to three years, 4 to 5 years after a hurricane. These are the startups founded two years earlier and that have survived so far. These findings indicate that jobs created by startups as a result of rising demand for restoration after a hurricane are not especially short-lived, suggesting that they are not the result of overreaction by entrants.

What explains the disproportionate job creation by startups? Standard models of firm dynamics (Hopenhayn, 1992; Clementi and Palazzo, 2016) attribute a role to firm entry because decreasing returns to scale production technology or factor adjustment costs inhibit existing firms to fully accomodate economic shocks. If recruiting new employees involves significant costs, revenue gains from positive demand shocks will partly spill over to incumbent workers' wages, because it is cheaper to retain them than to hire new workers (Kline et al., 2019). Such rent-sharing can explain why established firms do not accomodate completely for the rise in demand, because part of the revenue gains pass-through to incumbent employees instead of being used to hire new workers. Instead, startups, by virtue of being new, can hire employees without paying higher salaries. The results from analyses on the average monthly earnings of workers indeed show that wages in established businesses rise following a hurricane, unlike those of startup employees which remain similar to their pre-hurricane levels, in line with the predictions of theories of rent-sharing.

This paper contributes to the literature on the economic impacts of natural hazards in developed countries. Previous studies on this topic have focused on a region's GDP (Strobl, 2011), broad labor market (Belasen and Polachek, 2009), government transfer payments (Deryugina, 2017), housing prices (Murphy and Strobl, 2010; Liao and Panassié, 2019), or migration (Boustan et al., 2020). Recently, a number of papers have argued that aggregate measures of labor market outcomes may mask substantial heterogeneity in responses between individuals (Deryugina et al., 2018; Groen et al., 2019), or between firms (Barrot and Sauvagnat, 2016; Cole et al., 2019; Seetharam, 2018; Elliott et al., 2019). I add to this literature by showing new and established ventures display different post-disaster job creation dynamics.

I also contribute to the literature highlighting the importance of the new venture creation response to economic shocks of various kinds (Adelino et al., 2017; Decker et al., 2017, 2018; Bernstein et al., 2018). In particular, my results confirm the findings of previous papers that new firms account for a disproportionate share of new jobs created in a region when investment opportunities arise. Furthermore, the finding that firms founded in the wake of a natural hazard remain larger than their counterparts in unaffected regions when they age, seems to suggest economic conditions at the time of founding play an important role in explaining startup growth (Sedláček and Sterk, 2017; Clementi and Palazzo, 2016).

Finally, these findings hold implications for disaster management policies. Anecdotal evidence suggests reconstruction and recovery from natural hazards is often messy and slow, contrary to evacuations and life-saving first response which are quite effective[3]. This paper suggests that fostering the creation of new firms to assist in reconstruction may help to speed up this process.

[3] e.g. http://www.nydailynews.com/new-york/nyc-behind-restoration-projects-5-years-sandy-article-1.3594544

3.2 Background and Data

3.2.1 Geographic Area of Study

Hurricanes that affect the United States are tropical cyclones that form over the Atlantic Ocean. Tropical cyclones are strongest when they are situated above the ocean, and usually weaken quickly when they hit land, because they are no longer being fed by the energy from the warm ocean waters. Hence, counties close to the coast experience the strongest impact. Because typically only the geographic area relatively close to the coast is affected by hurricanes, I restrict the sample to counties in 19 states bordering or close to the the Northern Atlantic Ocean. These states are: Alabama, Connecticut, Delaware, Florida, Georgia, Louisiana, Maine, Maryland, Massachusetts, Mississipi, New Hampshire, New Jersey, New York, North Carolina, Pennsylvania, Rhode Island, South Carolina, Texas, and Virginia. As a robustness check, in Section 3.6 I further restrict the sample to only coastal watershed counties that are closest to the ocean.

3.2.2 Hurricane Exposure

North Atlantic cyclones are classified by their maximum sustained surface wind speed (peak one-minute wind at the standard meteorological observation height of 10 m over unobstructed exposure). Cyclones with one-minute sustained winds that exceed 33 m/s (64 kn) are categorized as a hurricane on the Saffir-Simpson hurricane wind scale. I will use this cutoff value to determine whether a county is exposed to a hurricane in a certain quarter or not. As shown by Deryugina (2017), counties that experience hurricane-strength winds incur substantial structural damage to buildings, and destruction of inventory, contrary to neighboring counties that do not experience winds of hurricane strength. Although the damage caused by a hurricane depends on both wind-speed, flooding/excess rainfall, and storm surge, a commonly adopted assumption in the literature is that the latter two effects, which are much more difficult to model, are highly correlated with wind speed and therefore wind speed serves as a good proxy for the potential damage due to a hurricane strike (Emanuel, 2011). Furthermore, because hurricane damage rises non-linearly with wind speed, in the empirical analysis I will contrast the impact of Category 1 and Category 2 hurricanes, with maximum wind speeds of 33-42 and 43-49 m/s respectively[4].

To track which counties are exposed to a hurricane in a certain quarter, I use the `stormwindmodel` software package developed by Anderson et al. (2018) to calculate maximum sustained wind speeds at the population mean center locations for all U.S. counties for all quarters between 2000 and 2015. As a starting point, I use 6-hourly location and maximum wind speed information from the Hurricane Data second generation (HURDAT2) "Best Track" hurricane track data from the National Hurricane Center[5] for all Atlantic-basin tropical storms between 1988 and 2015, and impute it to 15-minute intervals. This imputation uses a natural cubic spline, with the degrees of freedom set as the number of timed observations for the storm in the input data divided by two. Based on the imputed location and intensity data, the software allows users to model wind speeds at grid points in the United States using a model for wind speed developed by Willoughby et al. (2006). This model is a family of piecewise continuous parametric profiles where the profile wind is proportional to a power of radius inside the eye and decays exponentially outside the eye with a smooth transition across the eyewall. Based on information about the hurricane's center, and the maximum wind and its radius, the model converts position and intensity into a geographical distribution of winds. As shown by Willoughby et al. (2006), this model is preferred over the commonly used model of Holland (1980) where the wind decreases too rapidly with distance from the maximum both inside and outside the eye. Furthermore, this approach of estimating wind speeds at different geographical locations is more conservative than the approach of Deryugina (2017) who assumes that all counties located within the estimated maximum wind speed radius (MWSR) experience the maximum sustained wind speed occurring within the circula-

[4] I observe no exposure to wind speeds of Category 3 or higher. While this may be surprising at first sight with, for example, hurricane Katrina occurring in the sample time frame, this is likely because maximum sustained wind speeds are modeled at each county's population mean center. Since most county centers are not directly at the storm's center, most counties of landfall will have a lower maximum sustained wind speed than the storm's maximum sustained wind.

[5] Available from: https://www.nhc.noaa.gov/data/#hurdat

tion of the system, regardless of their distance to the center of the hurricane[6].

Between 2000 and 2015, 2 to 14 hurricanes formed over the Atlantic Ocean each year, with an average of 7 per year. However, not all of these make landfall at hurricane strength. 17 storms caused hurricane-strength wind speeds in at least one county, with an average of 6 counties being hit by one hurricane. Furthermore, the sample period contains eight years in which no counties were hit by a hurricane. In particular, in the years 2000, 2001, and 2015 there are no hurricane strikes, which implies that I observe at least two years before a hurricane, and one year after the hurricane, for all counties that were at some point affected. This is important for the empirical strategy explained in section 3.3.

Figure 3.1 shows the geographic distribution of hurricane strikes between 2000 and and 2015. In total, 87 counties were hit at least once by a hurricane during the sample period. The white-colored counties are the ones that were not affected by a hurricane during the sampling period, and that will serve as the control group.

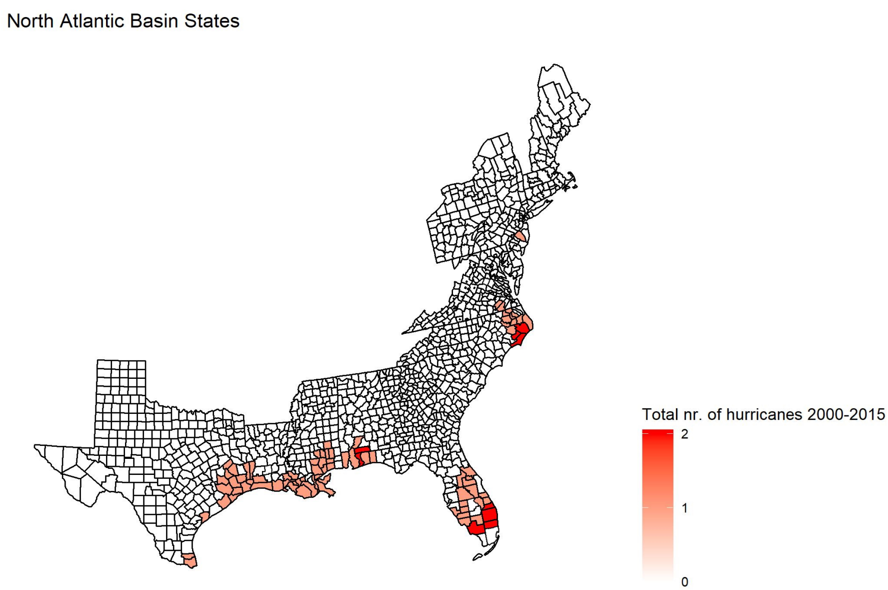
3.2.3 Economic Data

To estimate the economic impact of hurricanes, I use publicly available county-level data from the U.S. Census Quarterly Workforce Indicators (QWI). The QWI is derived from the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee data, which covers 95% of U.S. private jobs, and provides information regarding employment and wages for firms in the private sector[7]. In the main analysis of the paper, I rely on data disaggregated by sector and by firm age. The five different firm age categories are (in years): 0-1, 2-3, 4-5, and 6+. I consider firms in the 0-1 years-old category as “startups”.

[6] In fact, a comparison with the data of Deryugina (2017) revealed that her estimated wind speeds are substantially higher than those derived from the model of Willoughby et al. (2006), especially for counties further away from the center of the hurricane. While it is difficult to say which approach is more reliable, this highly suggests that the approach used in this paper is a more conservative one.

[7] The coverage of the QWI increases over time. The data covers 18 states in 1995, 42 in 2000 (the first sample year in this paper), and all 50 states plus the District of Columbia in 2015 (the last year I consider). In 2000 the data covers 15 of the 19 hurricane-prone states. By 2003 all states are included in the focal sample, except for Massachussetts which is included only since 2011.

Figure 3.1: Spatial distribution of hurricanes, 2000-2015



One advantage of the QWI is that the primary building block to construct the aggregated measures is at the worker-firm-quarter level. This means that a new establishment will only be labeled as a startup when it is a separate legal entity, and not a newly formed establishment of an existing firm.

Employment is measured as the total number of stable jobs, i.e., the number of jobs that are held on both the first and last day of the quarter with the same employer for firms in each age category (*Emps*). Wages are measured as average monthly earnings of employees with stable jobs (*Earns*). This measure reflects the earnings of workers who worked for a full quarter at the same firm, i.e., workers who were registered at the same firm on the first and the last day of a certain quarter. Hence, workers who intermittently change firms are also included, but this is likely to be a very small number of people. It is also important to note that full-quarter does not equal full-time, but will also include the wages of part-time or temporary workers (as long as the duration of the contract is longer than 3 months). All wages are reported in 2015 U.S. dollars. Because I will compare employment and earnings outcomes across firms in the different age categories and to maintain a consistent sample across outcomes, I restrict the sample to counties that have nonmissing employment and earnings data for all different firm age categories in a given sector.

I supplement the QWI data with information about counties' population and workforce in the year 2000 (i.e., before any county is affected by a hurricane) from several other sources. Data about a county's population comes from the Surveillance Epidemiology and End Results (SEER) population database. Information about land area comes from the Census Bureau Summary Files. Information about a county's labor force and unemployment rate come from the Local Area Unemployment Statistics (LAUS). Data about the total number of workers employed, the amount of retail establishments, and average wages in the retail sector come from the County Business Patterns (CBP). From this data I also construct measures of population density, measured as the number of inhabitants per square mile, and business density, measured as the number of establishments per square mile. Finally, the housing prices used in robustness tests come from the Federal Housing Finance Agency (FHFA) House Price Index (HPI) at the county-level. The FHFA HPI is a yearly weighted, repeat-sales index, and it measures average price changes in repeat sales or refinancings on the same properties.

3.2.4 Summary Statistics

Table 3.1 reports summary statistics for the main variables of interest. The average county had a population of circa 114,113 inhabitants and a population density of 447 persons per square mile in 2000. On average, around 47.1% of the inhabitants were in the labor force in 2000. Circa 44,512 individuals were employed, with an average annual wage of $21,568. The unemployment rate in the median county was around 4.44% in the average county in 2000.

In the analysis, I mainly focus on sectors related to rebuilding and recovery activities. I define these as the sectors that will likely experience a labor demand shock because they are involved in activities associated with rebuilding and recovering from storm damage. These are Construction (two-digit NAICS 23), Finance and Insurance (NAICS 52), Real Estate and Rental and Leasing (NAICS 53), and Administrative and Support and Waste Management and Remediation Services (NAICS 56). I will also compare employment outcomes in these sectors with those across all industries, and for the Retail & Food and Accomodation (NAICS 44-45 and NAICS 72), and Manufacturing (NAICS 31) sectors separately. Following Mian and Sufi (2012), the Retail & Food and Accomodation (NAICS 44-45 and NAICS 72) can be classified as nontradable sectors that are especially dependent on local demand, whereas the Manufacturing sector is categorized as tradable, being mostly dependent on non-local demand[8]. Examining aggregate outcomes as well as potential employment changes in the tradable and nontradable sectors helps me to ensure that the results are not driven by aggregate changes in local labor markets, demand for goods and services, or output.

Between 2000 and 2015, on average around 10,903 individuals are employed in sectors associated with rebuilding per quarter. This is more than in the Manufacturing sector which has an average of 10,213 employees, but less than in the Retail & Food and Accomodation sectors which employ on average 11,111 workers per county per quarter. Combined, these sectors account for 83% of total employment in the average county. Average monthly earnings in rebuilding sectors equal $2,725, which is slightly higher than the cross-industry average of $2,615. Unsurprisingly, workers in the retail & food and accomodation sectors earn significantly less, averaging only $1,474 per month. Manufacturing workers earn the highest wages, with average earnings equaling $3,561 per month.

When I break down the employment statistics by firm age for the recovery and rebuilding sectors, several notable differences occur. First, old firms account for the overwhelming majority of employment: firms aged 6 years

[8]These definitions match the ones of Mian and Sufi (2012) as closely as possible, given that the LEHD data are not broken down by four-digit NAICS industries.

or older employ on average 9,360 individuals per county, or nearly 86% of total employment. Startups account for a substantially smaller share of total employment with only 462 employees, or slightly more than 4% of total employment on average. Firms aged 2-3 years and 4-5 years old employ respectively 487 and 478 workers on average, or 4.8% and 4.7% of total employment. It is useful to keep these proportions in mind when I discuss how hurricanes affect net employment creation by firm age category. In particular, the regressions in Section 3.4.2 are set up such that the coefficients add up to the total aggregate response within these sectors.

Table 3.1: Summary Statistics

	N	Mean	Std. Dev.	p25	p50	p75
2000 County Characteristics						
Total population	72596	114113.32	255153.26	16251.00	35917.00	93791.00
Population density (persons/square mile)	72596	447.07	2637.67	31.86	69.57	193.12
Business density (establishments/square mile)	72596	14.13	142.75	0.55	1.37	4.49
Labor force	72596	47.14	5.49	43.72	47.46	50.98
Employment	72596	45264.99	128702.60	3282.00	9528.00	30229.00
Avg. annual wage	72596	21567.63	6006.92	17794.76	20561.57	23992.16
Unemployment rate	72596	4.44	1.62	3.40	4.20	5.20
Average monthly earnings						
All Industries	72596	2615.19	914.90	1976.11	2473.82	3052.79
Recovery and Rebuilding	51042	2724.66	1002.18	2005.75	2566.45	3223.48
Retail & Food and Accomodation	56412	1474.26	376.85	1186.33	1456.92	1707.41
Manufacturing	26325	3561.21	1384.94	2560.62	3314.43	4240.29
Employment						
All Industries	72596	38658.34	110432.57	2733.00	8080.00	25729.00
Recovery and Rebuilding	51042	10903.05	30816.03	960.00	2180.50	7178.00
Retail & Food and Accomodation	56412	11111.01	24386.75	1315.50	3087.50	9591.50
Manufacturing	26325	10212.98	14659.64	3078.00	5783.00	11341.00
Recovery employment by firm age						
Startups	51042	461.57	1262.57	38.00	104.00	347.00
2-3 year-olds	51042	538.79	1464.01	43.00	122.00	409.00
4-5 year-olds	51042	529.18	1436.65	41.00	120.00	394.00
6+ year-olds	51042	9360.36	26964.59	777.00	1806.00	6008.00

Monetary values are in 2015 dollars. Sample includes all counties in 19 hurricane-prone states (total number of counties is 1,193). Sectors related to recovery and rebuilding comprise the Construction (two-digit NAICS 23), Finance and Insurance (NAICS 52), Real Estate and Rental and Leasing (NAICS 53), and Administrative and Support and Waste Management and Remediation Services (NAICS 56).

Table 3.2: Mean differences between hurricane and non-hurricane counties

	No Hurricane	Hurricane	t-statistic
2000 County Characteristics			
Total population	108348.77	192748.03	-2.98
Population density (persons/square mile)	461.17	251.08	0.72
Business density (establishments/square mile)	14.69	6.26	0.53
Labor force	47.32	45.31	3.30
Employment	43415.73	74526.36	-2.16
Avg. annual wage	17821.64	18343.61	-0.96
Unemployment rate	4.42	5.02	-3.31
Average monthly earnings			
All Industries	2605.05	2741.19	-10.54
Recovery and Rebuilding	2711.71	2874.38	-9.93
Retail & Food and Accomodation	1467.93	1547.90	-13.62
Manufacturing	3537.93	3778.47	-8.34
Employment			
All Industries	36319.89	67699.15	-20.16
Recovery and Rebuilding	10105.35	20128.97	-19.97
Retail & Food and Accomodation	10345.67	20023.79	-25.58
Manufacturing	10104.51	11225.64	-3.67
Recovery and Rebuilding employment by firm age			
Startups	416.00	988.65	-27.94
2-3 year-olds	486.69	1141.30	-27.54
4-5 year-olds	477.56	1126.18	-27.81
6+ year-olds	8711.51	16864.83	-18.55

Monetary values are in 2015 dollars. Sample includes all counties in 19 hurricane-prone states (total number of counties is 1,193). Sectors related to recovery and rebuilding comprise the Construction (two-digit NAICS 23), Finance and Insurance (NAICS 52), Real Estate and Rental and Leasing (NAICS 53), and Administrative and Support and Waste Management and Remediation Services (NAICS 56).

In Table 3.2, I compare characteristics of counties that do and do not experience at least one hurricane during the sampling period. Counties affected by a hurricane between 2000 and 2015 have a larger population in 2000, but a smaller population and business densities on average, although the mean differences for the last two are not significantly different from zero. Non-hurricane counties have a slightly larger share of the the population that is in the labor force and a lower unemployment rate in 2000. Employment and earnings across sectors are all higher in hurricane-affected counties between 2000 and 2015.

Differences in levels are not problematic for estimation because I include county fixed effects in the empirical specification. However, differences in levels may indicate differences in trends. To minimize concerns about differences in pretrends, I try to control for these differences by interacting the county characteristics in the year 2000 with a quarter dummy to allow for differential effects over time (Acemoglu et al., 2004; Hoynes and Schanzenbach, 2009).

3.3 Empirical Strategy

This paper aims to study firm responses to local hurricane strikes. Throughout the analysis, identification relies on the conjecture that occurrence of a hurricane is uncorrelated with unobservable local economic shocks, conditional on the location and time. This is reasonable because the complex nature of the relationship between oceanic and atmospheric variables and hurricanes make forecasting hurricane tracks and intensity even only several days in advance an extremely difficult exercise[9].

I estimate a staggered difference-in-differences model with multiple pre- and post-hurricane indicators, which is useful for gauging the overall pattern of the impact of a hurricane. In addition, the coefficients for the pre-hurricane periods help assess any pretrends. In particular, I regress outcomes on a set of indicators for the years since a hurricane, ranging from 4 ore more years before to 6 or more years after a hurricane. I control for county and year-quarter fixed effects, an indicator for whether a county experienced at least one hurricane between 1990 and 1999, and also include year-quarter indicators interacted with each of the following 2000 characteristics: Total population in a county (log), labor force, the unemployment rate, population density (persons/square mile), business density (establishments/square mile), total employment (log), and the average annual wage (log). Specifically, the estimating equation is:

$$Y_{ct} = \sum_{\tau=-4,\,\tau\neq-1}^{6+} \beta_\tau H_{c\tau} + \boldsymbol{X}_{c,2000}\alpha_t + \beta_h H_{c,pre\,00} + \alpha_c + \alpha_t + \epsilon_{ct}, \tag{3.1}$$

where Y_{ct} is a particular outcome variable for county c in quarter t, such as the log of employment. The variable $H_{c\tau}$ is an indicator equal to one if

[9]For example, the National Hurricane Center's (NHC) average 5-day hurricane track forecast errors have averaged 550 kilometers in the last few years: https://www.aoml.noaa.gov/hrd/tcfaq/F6.html.

the county experienced a hurricane τ years earlier (or $-\tau$ years later if τ is negative), and zero otherwise. I include indicators for $\tau = 4$ or more years before a hurricane to 6+ years after a hurricane. I omit the year before a hurricane strike, so the estimated coefficients should be interpreted as the change relative to the year before the hurricane. A small number of counties in the sample are affected twice by a hurricane. In this case, I use only the first instance of a hurricane between 2000 and 2015 in that county.

Because stronger hurricanes generally cause more damage than weaker hurricanes, I also estimate the same equation but with separate indicators for Category 1 wind speeds (33-42 m/s) and Category 2 wind speeds (43-49 m/s). The variables α_c and α_t are county and year-quarter fixed effects capturing stable differences between counties and macro-economic shocks. The set of interactions $\boldsymbol{X}_{c,2000}$ allows the year-quarter fixed effects to differ by linear 2000 characteristics (cf. Table 2.1). Finally, $H_{c,pre\,00}$ is an indicator equal to one if a county experienced a hurricane between 1990 and 1999. Standard errors are clustered at the commuting zone level[10].

3.4 Results

This section presents the main findings linking hurricanes to employment changes across sectors and for firms in different age categories. The expectation is that there is an increase in employment in sectors associated with recovery and rebuilding in the years after a hurricane, but not per se in other sectors. I start by estimating the impact on total employment by sector. Because the aggregate outcomes may mask substantial heterogeneity among firms, in the next part, I split up the results for employment by firm age.

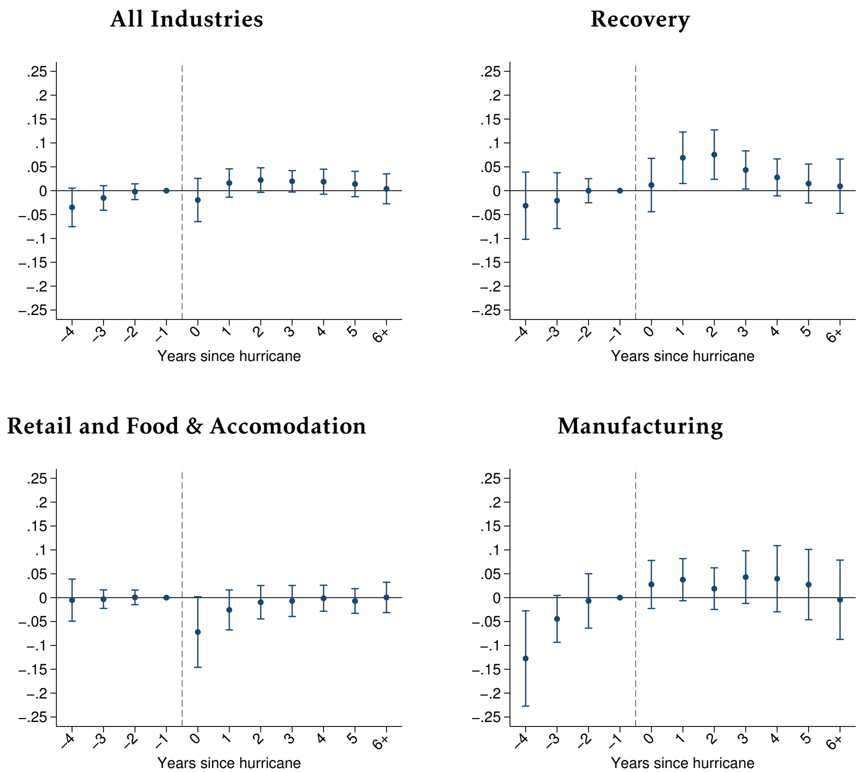
3.4.1 Post-Hurricane Employment Dynamics by Sector

Figure 3.2 displays the results for total employment by sector. I find no significant change in employment in the years following a hurricane for all industries combined, consistent with the findings of Deryugina (2017). However, outcomes differ across sectors. In line with the expectations that demand

[10] I link counties to commuting zones using a county-to-commuting-zone bridge provided by the Economic Research Service of the U.S. Department of Agriculture

for labor in sectors associated with rebuilding and recovery goes up, I observe a significant increase in employment in these sectors in the initial years after a hurricane: two years after a hurricane, employment in these sectors has increased by 7.9%. Also, the increase is only temporary: employment returns back to its pre-hurricane levels after 4 or more years. I do not observe an increase in the first four quarters after a hurricane. This 'lag' can probably be explained by the fact that it takes time for hurricane victims to seek financial aid from insurance companies or federal agencies, and, hence, demand for rebuilding and restoration will not immediately go up after a hurricane (Liao and Panassié, 2019). I do not observe a similar increase in net job creation in the Retail and Food & Accomodation or the Manufacturing sectors. In fact, the results show that employment in the Retail and Food & Accomodation sectors drops in the initial period after a hurricane, but recovers relatively quickly. This may be because businesses need to temporarily cease operations due to storm damage or because of a drop in local demand around the time of a hurricane strike (Basker and Miranda, 2018).

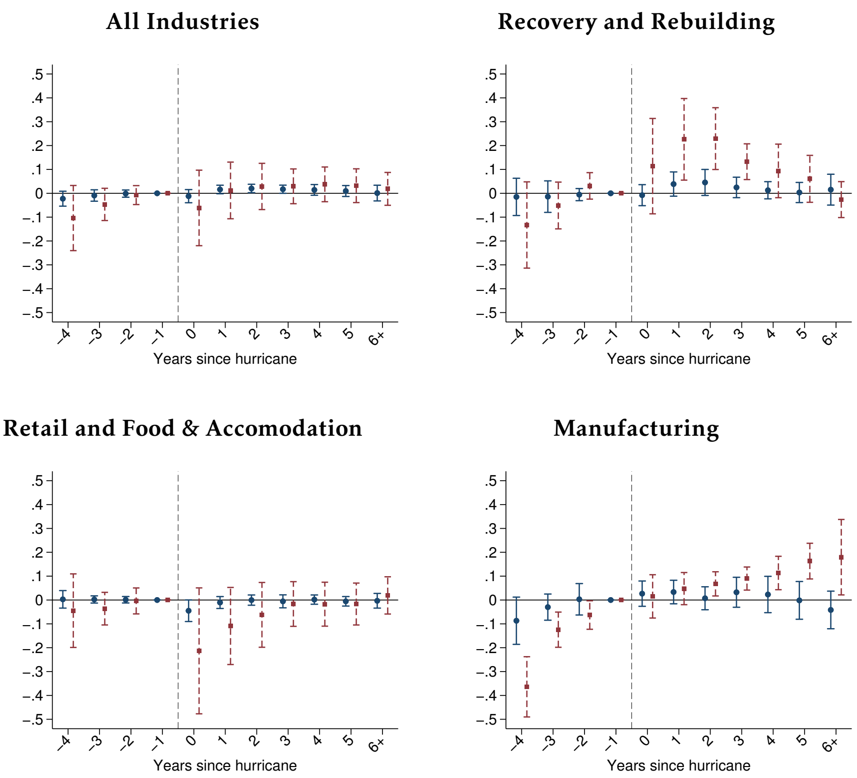
Figure 3.2: The Effect of Hurricanes on Employment by Sector



Notes: Point estimates and 95 percent confidence intervals from regressions of $ln(Emps)$ on years since a hurricane strike for All Industries, and for the sectors associated with Recovery and Rebuilding, and the Retail and Food & Accomodation, and Manufacturing sectors separately are shown. Standard errors are clustered at the commuting zone level. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Figure 3.3 shows the results for hurricane Category 1 and Category 2 winds separately. Again, I find no changes in net job creation for all industries combined, regardless of the strength of the wind speeds. The findings for the sectors associated with rebuilding and recovery, on the contrary, show that the previously observed increase in employment is more pronounced when a county experiences stronger winds: I estimate that employment is 18% above its pre-hurricane levels, 2 years after a county experienced hurricane Category 2 winds, compared to only 3.8% when a county is hit by Category 1 winds. This supports the notion that storm damage, and, hence the demand for restoration rises non-linearly with wind speed (Emanuel, 2011). Employment also experiences a stronger drop in the Retail and Food & Accomodation sectors, although the estimates are noisy and not significantly different from zero. The results also indicate that employment in the Manufacturing sector starts to rise from two years after a Category 2 hurricane, although the estimates display strong positive pretrends and therefore cast doubt on whether this is due to the impact of a hurricane strike or due to confounding influences.

Figure 3.3: The Effect of Hurricanes on Employment in Firms of Different Ages - Category 1 (Blue) vs. Category 2 (Red) Hurricanes



Notes: Point estimates and 95 percent confidence intervals from regressions of $ln(Emps)$ on years since a hurricane strike for All Industries, and for the sectors associated with Recovery and Rebuilding, and the Retail and Food & Accomodation, and Manufacturing sectors separately are shown. The blue estimates are for Category 1 hurricanes, the red estimates are for Category 2 hurricanes. Standard errors are clustered at the commuting zone level. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

3.4.2 Post-Hurricane Employment Dynamics by Firm Age

Total Employment

So far, I have found that employment in sectors related to rebuilding and recovery increases significantly in the initial years after a hurricane. Now, I examine whether firms of different ages contribute differently to the rise in net job creation in these sectors. As a first step, I estimate the effect of a hurricane on employment for firms in the five different age categories separately. The results in Table 3.3 indeed show strong heterogeneity. I estimate that employment through new firm formation (Column (2)) rises with nearly 24% in the first year after a hurricane, and remains elevated in the next two years as well. This is over three times more than estimated increase in overall employment (Column (1) and Figure 3.2)). I also observe a significant increase in employment among firms aged six years or older (Column (5)), but the effect size is substantially smaller: two years after a hurricane, employment in old firms is circa 6.6% above its pre-hurricane levels. The results for firms aged 2-3 years and 4-5 years (Columns (3) and (4)) also show employment increases in the initial years after a hurricane, but the estimates are noisy and of a magnitude similar to old firms.

Interestingly, I do observe a significant increase in employment in firms of 2-3 years-old between four and five years after a hurricane. These are exactly the startups founded two years earlier, shortly after a hurricane, and that have survived so far. Similarly, net job creation in firms of 4-5 years-old is significantly above its pre-hurricane levels five years after a hurricane. These findings suggest that jobs created by startups in the early aftermath of a natural hazard are not particularly short-lived, but that these firms remain larger than their counterparts in non-affected regions for several years.

Table 3.3: Impact of hurricanes on Recovery and Rebuilding Employment by Firm Age

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
4+ years before hurricane	-0.023	0.015	-0.087	-0.044	-0.016
	(0.030)	(0.044)	(0.054)	(0.079)	(0.038)
3 years before hurricane	0.015	-0.045	-0.046	-0.061	0.031
	(0.029)	(0.051)	(0.065)	(0.043)	(0.036)
2 years before hurricane	-0.001	0.062	-0.054	0.022	0.001
	(0.015)	(0.055)	(0.059)	(0.047)	(0.015)
0 years after hurricane	0.014	0.058	0.073	-0.045	0.012
	(0.033)	(0.068)	(0.075)	(0.059)	(0.031)
1 year after hurricane	0.072**	0.238***	0.054	0.098	0.051
	(0.032)	(0.060)	(0.060)	(0.068)	(0.030)
2 years after hurricane	0.079**	0.234***	0.068	0.072	0.066**
	(0.027)	(0.056)	(0.078)	(0.060)	(0.024)
3 years after hurricane	0.047*	0.197***	0.115	0.012	0.027
	(0.022)	(0.058)	(0.067)	(0.057)	(0.021)
4 years after hurricane	0.028	0.101	0.119**	0.074	0.028
	(0.023)	(0.053)	(0.041)	(0.053)	(0.024)
5 years after hurricane	0.015	0.011	0.135**	0.101*	0.015
	(0.024)	(0.053)	(0.050)	(0.047)	(0.025)
6+ years after hurricane	-0.023	0.103*	0.047	0.029	-0.025
	(0.029)	(0.052)	(0.067)	(0.072)	(0.028)
Observations	51,025	51,025	51,025	51,025	51,025
R^2	0.992	0.902	0.901	0.895	0.990

Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Table 3.4: Impact of hurricanes on Recovery and Rebuilding Employment by Firm Age: Category 1 vs. Category 2 hurricanes

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
Panel A. Category 1 hurricanes					
4+ years before hurricane	-0.001	0.009	-0.081	-0.050	0.011
	(0.023)	(0.048)	(0.060)	(0.076)	(0.026)
3 years before hurricane	0.030	-0.010	-0.072	-0.061	0.050
	(0.031)	(0.049)	(0.069)	(0.053)	(0.039)
2 years before hurricane	-0.009	0.058	-0.042	0.008	-0.011
	(0.015)	(0.058)	(0.062)	(0.049)	(0.017)
0 years after hurricane	-0.011	0.058	0.067	-0.040	-0.013
	(0.021)	(0.066)	(0.065)	(0.048)	(0.019)
1 year after hurricane	0.028	0.203***	0.077	0.082	0.018
	(0.021)	(0.060)	(0.049)	(0.060)	(0.021)
2 years after hurricane	0.038	0.174**	0.091	0.081	0.033
	(0.020)	(0.054)	(0.077)	(0.061)	(0.021)
3 years after hurricane	0.015	0.150*	0.119	0.050	0.007
	(0.016)	(0.067)	(0.067)	(0.050)	(0.016)
4 year after hurricane	0.010	0.085	0.122*	0.095	0.008
	(0.017)	(0.054)	(0.053)	(0.053)	(0.018)
5 years after hurricane	0.004	-0.015	0.123*	0.107	0.004
	(0.023)	(0.058)	(0.058)	(0.061)	(0.023)
6+ years after hurricane	-0.016	0.105	0.105	0.048	-0.019
	(0.034)	(0.064)	(0.058)	(0.079)	(0.032)
Panel B. Category 2 hurricanes					
4+ years before hurricane	-0.132	0.025	-0.031	0.035	-0.156
	(0.081)	(0.067)	(0.092)	(0.180)	(0.110)
3 years before hurricane	-0.077	-0.176	0.130	0.000	-0.097
	(0.054)	(0.134)	(0.138)	(0.067)	(0.065)
2 years before hurricane	0.040	0.024	-0.063	0.068	0.058
	(0.028)	(0.087)	(0.094)	(0.070)	(0.040)
0 years after hurricane	0.126	0.000	0.033	-0.019	0.124
	(0.099)	(0.149)	(0.183)	(0.173)	(0.109)
1 year after hurricane	0.179*	0.172*	-0.111	0.084	0.166
	(0.087)	(0.077)	(0.174)	(0.193)	(0.108)
2 years after hurricane	0.180**	0.288*	-0.113	-0.044	0.158**
	(0.063)	(0.141)	(0.139)	(0.167)	(0.058)
3 years after hurricane	0.102**	0.228	-0.024	-0.182	0.093*
	(0.032)	(0.132)	(0.109)	(0.165)	(0.039)
4 years after hurricane	0.082	0.078	-0.019	-0.095	0.089
	(0.051)	(0.112)	(0.101)	(0.150)	(0.051)
5 years after hurricane	0.050	0.123	0.051	-0.028	0.050
	(0.045)	(0.110)	(0.096)	(0.186)	(0.042)
6+ years after hurricane	-0.031	-0.003	-0.255	-0.080	-0.022
	(0.049)	(0.096)	(0.189)	(0.147)	(0.043)
Observations	51,025	51,025	51,025	51,025	51,025
R-squared	0.992	0.902	0.901	0.895	0.990

Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Panel A shows the results for counties that are hit by Category 1 wind speeds. Panel B shows the results for counties that are hit by Category 2 wind speeds. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Table 3.4 shows the results for hurricane Category 1 and Category 2 winds separately. Although the estimates are quite noisy due to the low prevalence of counties that are hit by Category 2 winds in the sample, the findings indicate stronger effects for Category 2 winds quite similar to those shown before (cf. Figure 3.3). Two years after a county experienced hurricane Category 2 winds, startup employment is around 29% above its pre-hurricane levels, compared to 17% when a county experienced Category 1 winds. Similarly, firms of six years or older experience an estimated increase of nearly 16% in employment for Category 2 winds, compared to 3% for Category 1 winds, two years after a hurricane. Again, the findings indicate no significant changes in employment in firms aged 2-5 years-old in the initial years after a hurricane. The point estimates for these firms are even negative in the case of Category 2 winds, but due to the large standard errors the coefficients are not significantly different from zero.

Employment Shares

While these findings suggest that job creation through firm formation is more responsive to local demand shocks caused by hurricanes, the previous results do not tell much about the *share* of total excess job creation startups account for. This is because startups account for only a small fraction of total employment in a county (cf. Table 2.1), and, hence, a large absolute increase in startup employment may still be a relatively small increase compared to large firms.

To estimate the share of the total increase in employment firms of different ages account for, I construct the following outcome variable:

$$e_{ct}^{k} = \frac{Emps_{ct}^{k}}{Emps_{c2002}}$$

where e_{ct}^{k} is employment in county c in quarter t for firms in age category k relative to employment in county c in 2002 in sectors related to rebuilding and recovery. I use 2002 as baseline year as it is the last year in the sample without a hurricane. I lose a small fraction of counties because they did not report employment statistics for sectors associated with rebuilding in 2002, but this should not affect the results.[11] It is straightforward to see that:

$$\sum^{k} e_{ct}^{k} = e_{ct} = \frac{Emps_{ct}}{Emps_{c2002}}$$

By estimating Equation (3.1) with e_{ct} and e_{ct}^{k} as outcomes for the sectors associated with rebuilding, I can calculate the share of total employment increase accounted for by startups and established businesses respectively.

The results of these regressions are shown in Table 3.5. First consider Column (1), which reports overall employment changes relative to 2002. Similar to before, I find that overall employment increases significantly in the short-term after a hurricane: two years after a hurricane overall employment in sectors associated with rebuilding has risen by 11.5 percent since 2002 (and relative to controls). Overall, the coefficients are very similar to those reported in Column (1) of Table 3.3, further corroborating the findings that these sectors experience a significant rise in employment after a hurricane.

Employment through new firm formation increases with 2.6% of 2002 total employment two years after a hurricane. This implies that startups account for nearly 23% of the gain in total employment. Firms of six years or older experience an increase of 7.8% of 2002 total employment or 68% of the gain in total employment. Again, firms aged two to five year-old experience only small employment increases which are not statistically different from zero. In fact, the estimated share of employment growth these firms account for appears to be proportionate to their respective shares of total employment.

These magnitudes should be understood in light of the proportion that each firm age category makes up of total sector employment. In particular, the net employment creation by new entrants is striking given that these firms represent only 4% of total employment, while the oldest age category comprises over 85% of total employment in the average county (cf. Table 3.1). This means that startups disproportionally respond to economic shocks caused by hurricanes while we observe a relatively small proportional net response for the oldest category.

[11] I obtain similar results (available upon request) when using 2000 as base year.

Table 3.5: Impact of Hurricanes on Recovery and Rebuilding Employment Ratio by Firm Age

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
4+ years before hurricane	-0.032	0.004	-0.001	-0.002	-0.031
	(0.045)	(0.003)	(0.004)	(0.005)	(0.046)
3 years before hurricane	0.001	-0.004	-0.001	-0.005	0.012
	(0.030)	(0.004)	(0.005)	(0.003)	(0.030)
2 years before hurricane	-0.008	0.003	-0.002	0.001	-0.007
	(0.019)	(0.004)	(0.005)	(0.004)	(0.016)
0 years after hurricane	0.052	0.005	0.014*	-0.001	0.035
	(0.038)	(0.004)	(0.006)	(0.005)	(0.032)
1 year after hurricane	0.103*	0.014***	0.008	0.013	0.069*
	(0.040)	(0.004)	(0.005)	(0.009)	(0.033)
2 years after hurricane	0.115***	0.026**	0.004	0.008	0.078**
	(0.034)	(0.009)	(0.006)	(0.006)	(0.027)
3 years after hurricane	0.047	0.013*	0.007	0.001	0.025
	(0.028)	(0.006)	(0.005)	(0.005)	(0.028)
4 years after hurricane	0.044	0.001	0.005	0.002	0.037
	(0.045)	(0.004)	(0.003)	(0.004)	(0.045)
5 years after hurricane	0.016	-0.004	0.007	0.002	0.011
	(0.037)	(0.003)	(0.004)	(0.004)	(0.035)
6+ years after hurricane	-0.019	0.001	0.002	-0.000	-0.021
	(0.041)	(0.003)	(0.003)	(0.004)	(0.037)
Observations	48,378	48,378	48,378	48,378	48,378
R^2	0.618	0.372	0.373	0.300	0.592

Regressions of $\frac{In(Emps_t)}{In(Emps_{2002})}$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

3.5 Interpreting the Findings

In the previous sections, I find that employment in firms in sectors associated with rebuilding and recovery rises in the years following a hurricane. This increase is temporary, as job creation appears to respond for four years and eventually returns to the baseline. These results are consistent with a positive labor demand shock in these sectors. More novel is the finding that job creation through new firm creation accounts for a disproportionate share of employment growth, and that these jobs are not particularly short-lived. What accounts for this dissimilar responsiveness?

Standard models of firm dynamics with heterogeneous firms (Hopenhayn, 1992; Clementi and Palazzo, 2016) attribute a role to job creation through firm entry when economic shocks are not fully accommodated by existing firms due to decreasing returns to scale or adjustment costs. Hence, the share of job creation accounted for by startups will depend on the magnitude of these costs and firm entry conditions, in particular the level of entry barriers. Indeed, these models offer predictions consistent with empirical evidence on the contribution of startups to employment growth following demand shocks (e.g. Decker et al., 2018).

If firms cannot costlessly adjust their number of workers, this will give rise to imperfect competition, meaning that employers or workers or both get some rents from an existing employment relationship (Manning, 2011). Contrary to (standard) models of perfect competition, this implies that firm-specific heterogeneity will not only determine the number of workers hired, but also the salary a person receives. In particular, recent studies have found that wages closely track establishment-level productivity (Dunne et al., 2004; Faggio et al., 2010; Barth et al., 2016), and that shocks to productivity spill over to wages, indicating that workers capture some of the rents earned by their employers (see Card et al., 2018, for an overview of the evidence).

In a recent study, Kline et al. (2019) argue that a source of such positive rent-sharing elasticities is that new hires are imperfect substitutes for incumbent workers at a firm because they require costly on-the-job training. This allows incumbent workers to extract rents from the firm in the form of wage premia. In their model, new hires receive a competitive market wage, which can be decreasing in the value of non-monetary job amenities (Rosen, 1986). Incumbent workers, on the other hand, are paid the market wage plus a share of their marginal productivity, because retaining them is often cheaper than hiring and training new workers. The magnitude of the wage premium incumbent workers receive is proportional to their outside options, potential mobility frictions, and the hiring and training costs of new workers. Importantly, in this setting, a positive shock to labor demand will cause employers to raise the wages of incumbent workers, as long as employers possess some labor market power (cf. Manning, 2003). On the other hand, because firms compete for new hires in a competitive market, a labor demand shock will only affect the number of new hires but not their wages.

Translated to the setting of this paper, the above theory of rent-sharing offers different predictions regarding the employment and wage responses of startups and established firms. New ventures, simply because they are new, have no incumbent workers and only compete in the market for job seekers. Hence, I expect that only employment in startups increases but not the wages of their employees. On the other hand, established businesses will also increase employment but less than in a context where there would be perfect substitution between new hires and incumbent workers because part of the revenue increases following the demand shock will be passed through to incumbent workers. In other words, the employment response of established firms is partly dampened by the increase in wages of incumbent employees.

I test for these varying predictions regarding the wage responses of startups and established firms by estimating Equation (3.1) but now using average monthly earnings as dependent variable. The results are shown in Table 3.6. On average, wages increase between zero and three years after a hurricane, with an estimated peak of 4.3% after one year. However, and in line with the predictions, I observe no increase in wages in new firms, while workers in established firms aged two years or older experience a pay rise in the first twelve months after a hurricane. The wages of workers in firms aged 4 years or older also remain significantly elevated for several years, up to five years after a hurricane. This may suggest incumbent workers in established firms receive part of the revenue increases that stem from the rise in demand for recovery and rebuilding work.

Table 3.6: Impact of Hurricanes on Recovery and Rebuilding Earnings by Firm Age

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
4+ years before hurricane	-0.031**	-0.058	-0.054	-0.040	-0.025
	(0.011)	(0.042)	(0.032)	(0.030)	(0.013)
3 years before hurricane	-0.007	-0.023	-0.012	-0.026	-0.007
	(0.006)	(0.018)	(0.021)	(0.022)	(0.008)
2 years before hurricane	0.005	-0.002	-0.016	0.013	0.004
	(0.005)	(0.021)	(0.016)	(0.013)	(0.007)
0 years after hurricane	0.027***	0.016	0.051**	0.035*	0.028**
	(0.008)	(0.015)	(0.018)	(0.017)	(0.010)
1 year after hurricane	0.043***	0.024	0.033	0.057**	0.047***
	(0.007)	(0.020)	(0.022)	(0.020)	(0.010)
2 years after hurricane	0.037***	0.005	0.030	0.059*	0.047***
	(0.009)	(0.025)	(0.018)	(0.026)	(0.010)
3 years after hurricane	0.023**	-0.002	0.007	0.027	0.039***
	(0.008)	(0.023)	(0.016)	(0.025)	(0.010)
4 years after hurricane	0.007	-0.006	0.016	0.052*	0.023**
	(0.007)	(0.026)	(0.023)	(0.024)	(0.009)
5 years after huricane	0.009	0.021	-0.004	0.031	0.024*
	(0.009)	(0.026)	(0.024)	(0.022)	(0.010)
6+ years after hurricane	-0.006	-0.025	-0.011	0.003	-0.009
	(0.014)	(0.031)	(0.017)	(0.030)	(0.014)
Observations	51,025	51,025	51,025	51,025	51,025
R^2	0.949	0.636	0.662	0.665	0.921

Regressions of $ln(Earns)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

3.6 Robustness Tests

3.6.1 Restricting the Sample to Coastal Watershed Counties

In the preceding analysis the sample consisted of all counties in the states close to the Atlantic Ocean, as described in Section 3.2. Now, I restrict the sample to coastal watershed counties in these states. The National Oceanic and Atmospheric Administration (NOAA) considers a county to be a coastal watershed county if, at a minimum, 15 percent of the county's total land area is located within a coastal watershed or it comprises at least 15 percent of a coastal cataloging unit. Hence, these are the regions closest to the ocean and that should be very similar in terms of protection and preparation for possible hurricane strikes. In total, these are 426 counties over 19 states.

Tables C1 and C2 in the Appendix show the findings for the regressions of employment in firms of different age categories in sectors associated with rebuilding, for the restricted sample of coastal watershed counties. The results are very similar in sign and magnitude to those for the broader sample (cf. Tables 3.3 and 3.4. This seems to suggest that the findings are due to (unobserved) characteristics of counties further away from the shore.

3.6.2 Changes in House Prices

One assumption that runs throughout this study is that the used measure of wind speed is a good proxy for whether buildings in a county incur substantial damage or not. While the findings of more pronounced effects for Category 2 winds support this claim, I further substantiate this by looking at the changes in housing prices. Liao and Panassié (2019) find evidence that hurricane winds destroy or cause sufficient damages to buildings leading to a negative housing supply shock in the short run. Hence, if wind speed is a good measure for hurricane destruction, I will observe rising house prices in the initial years after a hurricane. To test for this, I estimate Equation (3.1) using a county's annual house price index (HPI) as the dependent variable.

The results are shown in Figure C1. As expected, house prices rise in the year of a hurricane and remain elevated until three years after a hurricane. In the first year after a hurricane, the HPI increases with 15 index points compared to pre-hurricane levels. The duration of these elevated housing prices is also consistent with the findings that employment in sectors associated with rebuilding increases in the first three years after a hurricane. This seems to suggest it takes about three years to fully restore damaged property. In line with the previous findings, the effects are stronger for Category 2 winds: one year after a county experienced Category 1 winds the HPI increases by 10 points, compared to 25 points for Category 2 winds. Overall, these results support the notion that the rise in employment in sectors related to rebuilding is because of a shock in demand for labor to restore storm damage.

3.6.3 Varying the Controls

I have also probed the robustness of the results for the regressions on employment by firm age to varying the econometric specifications of Equation 3.1. In particular, I omit the county characteristics variables (Table C3), and include county linear trends (Table C4). Overall, the point estimates and statistical significance levels are very similar across the various specifications.

3.6.4 Concise Event Study

Because of its flexibility, Equation 3.1 is inefficient if some coefficients are not substantially different from each other. To increase the power of the estimates, I use another specification that combines post-hurricane years 0–1, 2-3, 4-5, and 6+ and and assumes no differences between affected and unaffected counties in the prior to the hurricane. Given that I have not found significant pre-trends in the regressions on employment in the sectors associated with rebuilding, these assumptions appear to be suitable. The exact specification is:

$$\begin{aligned} Y_{ct} = {} & \beta_1 H_{ct,0\,to\,1} + \beta_2 H_{ct,2\,to\,3} + \beta_3 H_{ct,4\,to\,5} + \beta_4 H_{ct,6+} \\ & + \alpha_c + \alpha_t + \boldsymbol{X}_{c,2000}\alpha_t + \beta_5 H_{c,pre\,00} + \epsilon_{ct}, \end{aligned} \tag{3.2}$$

The variable $H_{ct,0\,to\,1}$ is equal to one in the quarter of a hurricane strike and the seven following quarters, and zero otherwise. β_1 will thus reflect the mean effect on outcome Y_{ct} in years 0-1 after the hurricane, relative to the years prior to the hurricane. $H_{ct,2\,to\,3}$, $H_{ct,4\,to\,5}$, and $H_{ct,6+}$ are defined in the same way.

The results are shown in Table C5. The findings for startups are very similar to before. However, I now also find a significant increase in employment for firms aged 2-3 years-old for the period between two and three years after a hurricane, although the size of the coefficient is still smaller than for startups. This seems to suggest that the insignificant outcomes for this group using the flexible event study framework were partly due to the low power of the estimates. Overall, the results from the concise event study suggest that the responsiveness to hurricanes in terms of employment decreases monotonically by firm age.

3.7 Conclusion

The rate at which economies recover from the impact of natural hazards depends on the ability of local firms in sectors associated with rebuilding and recovery to quickly respond to the surge in demand for reconstruction of damaged property. Which types of firms are more likely to address the rising demand? Recent evidence tells us that startups account for a large share of net job creation in general and that employment growth through new venture creation is more responsive to aggregate economic shocks than that through the expansion or contraction of established businesses. Here, I explore whether new ventures play an important role in responding to the demand for reconstruction and recovery labor in the wake of a natural hazard.

I examine this question by estimating changes in employment across firms in different age categories when a region is hit by a hurricane. I find strong evidence that new firms are responsible for a disproportionate share of net job creation in sectors associated with rebuilding and recovery. On the other hand, old firms contribute substantially less than expected. Existing firms aged between two and five years old account for only a small share of net job creation that is roughly proportional to their share in the economy. These findings lend strong support to models of firm dynamics that attribute an important role to job creation through firm entry because economic shocks are not fully accommodated by existing firms due to decreasing returns to scale or adjustment costs.

What can explain this 'muted' response by established businesses that opens up space for new firms to attract new hires? Perhaps, the bureaucratic nature of old firms makes them less willing or less suitable to respond to the opportunities that accompany the rise in demand (Özcan and Reichstein, 2009; Sørensen, 2007). Or, unobserved characteristics tied to the entrepreneur make them more suitable to detect and respond to the changing demand (Kirzner, 1979; McMullen and Shepherd, 2006). While a complete answer to this question is beyond the scope of this paper, I provide additional evidence supportive of the idea that existing firms grow less than expected because they share part of the rents that sprout from the positive demand shock with their incumbent workers instead of hiring new workers.

In this sense, this paper speaks to the nascent literature documenting that declining firm entry is related to the slowing down of employment and productivity growth, and sectoral reallocation of workers (Alon et al., 2018; Dent et al., 2016; Pugsley and Sahin, 2019). In fact, the results presented here suggest that the same factors that may constrain firm entry can lead to a slower recovery from the impact of natural hazards. Therefore, better understanding the reasons why job creation through firm formation is more responsive to demand fluctuations as well as the specific barriers to entrepreneurship is an important and ongoing research agenda with implications regarding firm dynamics, organizational behavior, and business strategy.

General Conclusion

Motivated by the observation that entrepreneurial activity fosters innovation, job creation, and economic growth, researchers and policy makers have dedicated considerable attention to better understand why individuals become entrepreneurs. Labor economists have approached this question by modeling the decision as a choice between entrepreneurship or working for an established employer. Hence, a substantial amount of studies within this literature has focused on comparing the private returns to these different choices. Theories of firm dynamics, on the other hand, have offered a framework that explains the important role firm creation plays in understanding aggregate employment patterns. Hence, these theories focus more on the social returns to entrepreneurship.

This dissertation revisits prior work on the returns to entrepreneurship by focusing on two important but overlooked questions. First, while it has already been noted by Carroll and Mosakowski (1987) that entrepreneurship is often a 'transient' rather than a 'stable' state in a person's career, scholars have mostly focused on mobility of individuals into entrepreneurship. I shift the focus to mobility out of entrepreneurship and ask how entrepreneurial experience shapes the earnings trajectories of entrepreneurship returning to wage work. Second, while a nascent literature documents that job creation in startups is more responsive to local economic shocks than job creation through the expansion of established business, little is known about the underlying mechanisms. Here, I examine how and why natural hazards affect job creation among firms of different ages. The ultimate goal of the dissertation is to provide a more comprehensive picture of the costs and benefits of the choice to become entrepreneur, in order to improve policies focused on fostering entrepreneurship among the workforce.

In the next sections, I will summarize the main findings, and discuss some of their implications for theory and practice. Finally, I will consider the main limitations of the different chapters, and offer suggestions for future research.

Main Findings

Chapter 1 starts by replicating the findings of previous studies of a significant wage penalty for former entrepreneurs at the time they return to wage work. We show that full-time employees who become self-employed and move back to a full-time job at some point on average earn roughly 6 percent less than equivalent employees without an entrepreneurial background. More importantly, we find that this wage penalty varies according to a person's rank in the wage distribution before entering entrepreneurship: the top 10 percent earners – the 'stars' – are penalized most, while we find no penalty for entrepreneurs coming from the bottom two deciles of the wage distribution. Furthermore, only entrepreneurs who exit self-employment relatively quickly are penalized; those who survive 5 or more years incur no wage loss. Also, entrepreneurs who return to their past employer incur no penalty, and the penalty is smaller for entrepreneurs who find a job in a large firm. These results are consistent with the proposed theory that a spell of entrepreneurship in a person's career signals uncertainty about his/her productivity in future jobs in wage employment.

Chapter 2 shifts the focus to the years after an entrepreneur has returned to wage work and shows that the observed initial wage losses are persistent over time, contrary to assumptions made in earlier studies. On average, former entrepreneurs earn 30 percent less per quarter and earn a full-time-equivalent daily wage that is 14 percent lower than their matched counterparts five years after exiting entrepreneurship, and there is no evidence of a catching up effect within the sample period. This indicates the losses are in part due to a reduction in hours worked, and a lower real wage. While non-full-time job contracts and above average job changing behavior of former entrepreneurs post-entrepreneurship can explain most of the reduction in hours worked, they have little explanatory power for the daily wage loss. To identify the potential mechanisms behind the daily wage penalty, we explore variation in the sample. In particular, we examine to what extent the losses differ depending on hybrid entrepreneurship status, reasons for entering self-employment, industry switching, age, employer size, and earnings in entrepreneurship. For most entrepreneurs, they can explain a part but not all of the observed wage gap.

Chapter 3 finds that that new firms are responsible for a disproportionate share of net job creation in sectors associated with rebuilding and recovery: I estimate that employment through new firm formation rises with nearly 24% in the first year after a hurricane, compared to a rise of 6.6% in old firms. Existing firms aged between two and five years old account for only a small share of net job creation that is roughly proportional to their share in the economy. This implies that startups account for nearly 23% of the gain in total employment. Firms of six years or older experience contribute to 68% of the gain in total employment. These magnitudes should be understood in light of the proportion that each firm age category makes up of total sector employment. In particular, the net employment creation by new entrants is striking given that these firms represent only 4% of total employment, while the oldest age category comprises over 85% of total employment in the average county. Furthermore, I observe no increase in wages in new firms, while workers in established firms aged two years or older experience a pay rise in the first twelve months after a hurricane. This may suggest incumbent workers in established businesses obtain part of the rise in profits due to the heightened demand for labor.

Implications

One important implication from the findings of Chapters 1 and 2 is that they caution against policies promoting entrepreneurship within certain groups of the workforce, solely based on their expected returns *during* the entrepreneurial spell. For example, although high-ability workers with high earnings in wage work typically also perform well as entrepreneurs (Levine and Rubinstein, 2018), we show that they suffer significant wage losses in case they decide to exit entrepreneurship quickly. In fact, it appears that when considering the decision the become entrepreneur, many individuals mainly take into account how much they are likely to earn as entrepreneur, disregarding how a spell of entrepreneurship will affect their future career in case the business fails. In Chapter , I argued that most theories of entrepreneurship start from a standard framework of expected utility theory. The findings presented here suggest that individuals optimize short-term utility, potentially to detriment of their long-term outcomes.

The finding that former entrepreneurs suffer significant persistent wage losses in the labor market also goes against the assumption made in several recent studies on the dynamics of self-employment that there are no costs involved in the move from entrepreneur to employee, and if there are they are only temporary in nature. Furthermore, our interpretation that this is in part because employers are uncertain about the qualities of entrepreneurs is not in line with contemporary policies in countries with little entrepreneurial culture, like Belgium, that tend to focus heavily on reducing potential stigmas around entrepreneurial failure or entrepreneurship in general.[12] The findings of this dissertation suggest that these policies might not be as beneficial as expected or even be counterproductive, because they ignore the potentially high costs involved in returning to paid employment after entrepreneurship.

We argue that labor market policies should focus more on reducing frictions in the search-and-hiring process when entrepreneurs return to paid employment. Our findings indicate that former entrepreneurs have difficulties in finding a suitable job, and that the increased likelihood of selecting into an inferior job can have long-lasting consequences. Chapters 1 and 2 highlight the benefits of offering former entrepreneurs a job with a trial period to reduce the downside risk in case they turn out to be a bad hire, but at the same time provides employers with a (relatively cheap) opportunity to tap into a pool of potentially valuable human capital. These recommendations echo complaints by employers after the abolishment of the possibility of a trial period by the Belgian federal government in 2014, which has now been partly reversed.

Second, technological change in the last decade has dramatically lowered the costs of experimenting with entrepreneurial ideas, particularly in industries that have benefited from the rise of the Internet, due to trends like open-source software and cloud computing. Because of this, temporary methodolo-

[12] One example is the *Failing Forward* campaign sponsored by the Flemish government: https://metfalenenopstaan.be/

gies to build companies, like the Lean Startup methodology, emphasize testing the viability of a product in the most cost-effective way. Importantly, these methodologies often recommend to abolish the experiment in case customer response is below expectations. However, and somehow paradoxically, the findings presented here that trying to minimize costs of entrepreneurial experiments by abolishing them early might involve significant earnings losses for the individuals that conduct them. Our results suggest that entrepreneurs should actively consider to adjust their course by pivoting from the original agenda in case of poor results, instead of immediately shutting down the business.

Theoretically, the findings also hold implications regarding theories of occupational choice. Given the magnitude of the estimated wage losses our findings question models that explain the entrepreneurial risk-return puzzle (Moskowitz and Vissing-Jørgensen, 2002; Hamilton, 2000) by stating that greater variance in the returns to entrepreneurship offers option value once entrepreneurs can abandon their costs at low cost (Vereshchagina and Hopenhayn, 2009). On the contrary, it seems that one reason why a substantial fraction of individuals persist in self-employment despite earning less than observably equivalent workers in paid employment, is because their spell of entrepreneurship negatively affects the value of their outside options. This resembles to some extent the findings of Landier (2005) who shows that in case entrepreneurial exit is costly this can lead to an equilibrium with a large fraction of underperforming entrepreneurial projects, which in turns justifies the high costs of abandoning and refinancing a new project. Although Landier (2005) focuses on the refinancing of new projects, it is quite straightforward to extend his model to a framework of occupational choice.

The findings of Chapter 3 highlight that economies with a high rate of entrepreneurial entry, and/or with low entry barriers might be able to better address the adverse outcomes of natural disasters and local economic shocks. Yet, accumulating evidence from multiple data sources and countries indicates that entrepreneurship and the growth of new ventures have been declining in recent decades, and at an accelerating speed since 2000: the post-2000 period has seen a decline in firm formation rates, high-growth ventures, and in particular high-growth young ventures (Bijnens and Konings, 2018; Davis et al., 2006; Haltiwanger et al., 2011; Reedy and Litan, 2011). This decline has led to a substantial reallocation of economic activity from new ventures and young firms to older incumbents, with potentially negative effects on job creation and recovery from recessions (Pugsley and Sahin, 2019). Better understanding the nature of these trends is therefore important as it is likely that the rate and intensity of such shocks will increase in the coming years.

Limitations and Avenues for Future Research

Chapters 1 and 2 examine the wage trajectories of former entrepreneurs to draw conclusions about the returns to entrepreneurial experience outside the entrepreneurial context. Of course, these returns are pecuniary and non-pecuniary, and the observed negative wage returns may mask positive non-pecuniary benefits. A better understanding of the whole of the advantages and disadvantages of self-employment can shed light on the question whether they continually tend toward equality compared to other occupational choices, as theories of equalizing differences (Rosen, 1986) would suggest, or whether market frictions distort this equilibrium.

Furthermore, some of the theoretical predictions within these chapters can only be directly tested with detailed information about person's occupation and job rank within a firm. For example, the persistency of the losses observed in Chapter 2 could be interpreted from a task-specific human capital perspective where temporary distortions to job assignment can lead to long-term differences (Gibbons and Waldman, 2006). To do so, the researcher needs data about a person's rank in his organization's hierarchy, as well as information about the correlation structure between tasks performed at different jobs within a firm. Unfortunately, this kind of information is absent in the Belgian social security data used in the empirical analysis. While data on occupations and tasks is typically absent from administrative employer-employee matched data, studies using this kind of data could for example proxy job rank according to a person's rank within a firm's wage distribution. Another possibility is to use survey data with detailed information about tasks performed on the job, like, for example, the Scientists and Engineers Statistical Data System (SESTAT) that collects longitudinal information of the college-educated U.S.science and engineering workforce.

An almost unexplored territory in the literature is how employees' entrepreneurial background can help firms in achieving or sustaining a competitive advantage in the market. In particular, a spell of entrepreneurship can be an opportunity for workers to acquire skills and knowledge that are difficult to obtain in more traditional employment settings. For example, Lazear's (2005) notion that entrepreneurs are 'jacks-of-all-trades', unlike employees, could also imply that firms engaging in R&D could benefit from hiring entrepreneurs, as they need employees with diversified knowledge in order to generate impactful or breakthough discoveries (Nagle and Teodoridis, 2020; Verhoeven, 2020). Furthermore, it is not well understood how firms can fully leverage former entrepreneurs' set of skills, although a substantial literature argues that (former) entrepreneurs differ in many ways from other workers. I believe this to be a particularly fruitful avenue for future research in the strategic human capital literature.

An important limitation of Chapter 3 is that it relies on aggregated data at the county-level. This means that the conclusions are based on averages across firms within the same age group in a county. However, changes in mean employment may mask important changes in higher moments of the distribution. Also, a more detailed picture of which firms and entrepreneurs create jobs following local economic shocks will help in understanding the mechanisms behind the observed patterns. For the US, novel matched employer-employee datasets like the Longitudinal Employer-Household Dynamics (LEHD) that combine firm and worker characteristics will likely be instrumental for this challenge.

Of course, no observational study in the social sciences is context independent, and this dissertation is no exemption. When interpreting the results of the different chapters, one should therefore be well aware of the characteristics of the respective empirical contexts in order not to extrapolate the findings too quickly to other labor markets. The analyses in Chapters 1 and 2 focus on social security data from Belgium, which is known to have a relatively rigid labor market[13], low entrepreneurial entry, but high rates of entrepreneurial

[13]Belgium ranks 48^{th}) on the labor market flexibility ranking of the 2019 Global Competitiveness Index, well behind Denmarkt (4^{th}) or the U.S. (3^{rd}), and neighboring

survival. While we argue that this is a good context to test the proposed mechanis, it remains an open question whether the findings from these chapters can be replicated in more flexible labor markets with high rates of entrepreneurship like the U.S. or Denmark. Similarly, Chapter 3 examines sectors that are relatively labor intensive. It is an open question whether the results hold true in capital intensive industries, such as manufacturing, where imperfect substitution between labor and capital and potential high fixed investment costs allow new firms to be equally responsive to economic shocks.

countries like the Netherlands (12^{th}) or Germany (18^{th}) , but similar to France (35^{th})

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Appendices

A Chapter 1

Table A1: Balance of full and matched samples

	Full sample			Matched sample		
	Entrepreneurs	Wage employees	Std. % bias	Entrepreneurs	Wage employees	Std. % bias
Age						
22 - 24	0.0445	0.1030	-22.5	0.0863	0.0863	0.0
25 - 29	0.2931	0.1525	34.3	0.3208	0.3208	0.0
30 - 34	0.2567	0.1857	17.2	0.2668	0.2668	0.0
35 - 39	0.1759	0.2125	-9.2	0.1734	0.1734	0.0
40 - 44	0.1115	0.2319	-32.4	0.1034	0.1034	0.0
45 - 49	0.0597	0.1729	-35.9	0.0492	0.0492	0.0
Female	0.2501	0.3329	-18.3	0.2072	0.2072	0.0
Region						
Flanders	0.6654	0.6516	2.9	0.7861	0.7861	0.0
Wallonia	0.2416	0.2850	-9.9	0.1943	0.1943	0.0
Brussels	0.0929	0.0633	11.0	0.0195	0.0195	0.0
Household position						
Living with parents	0.1614	0.1231	11.0	0.1911	0.1928	0.4
Single	0.1445	0.1184	7.7	0.1216	0.1198	0.6
Cohabiting - 0 children	0.1991	0.1647	8.9	0.2014	0.2062	-1.2
Cohabiting - 1 child	0.1715	.1923	-5.4	0.1798	0.1792	0.2
Cohabiting - 2 children	0.1933	0.2480	-13.2	0.1991	0.1983	0.2
Cohabiting - 3> children	0.0756	0.0929	-6.2	0.0635	0.06402	-0.2
Head 1 parent family - 1 child	0.0134	0.0201	-5.2	0.0085	0.0087	-0.2
Head 1 parent family - 2> children	0.0107	0.0174	-5.7	0.0067	0.0068	-0.1
Other	0.0301	0.0227	4.6	0.0263	0.0257	0.4
Occupation						
Blue-collar	0.3492	0.3399	2.0	0.4714	0.4752	0.8
White-collar/Govt. official	0.6508	0.6600	-2.0	0.5248	0.5285	-0.8
Public sector	0.0800	0.2605	-49.5	0.0878	0.0885	-0.3
Employer Industry						
Manufacturing	0.1812	0.2372	-13.8	0.2529	0.2529	0.0
Electricity, gas, water	0.0023	0.0088	-8.6	0.0004	0.0004	0.0
Construction	0.1445	0.0643	26.5	0.1906	0.1906	0.0
Wholesale and retail trade	0.2065	0.1313	20.2	0.2068	0.2068	0.0
Hotels and restaurants	0.0338	0.0105	12.9	0.0099	0.0099	0.0
Transport, storage, communication	0.0699	0.0989	-10.4	0.0638	0.0638	0.0
Financial institutions	0.0460	0.0541	-3.7	0.0329	0.0329	0.0
Real estate and professional services	0.1735	0.0892	22.3	0.1312	0.1312	0.0
Public administration, defence	0.0229	0.0998	-32.5	0.0285	0.0285	0.0
Education	0.0237	0.0939	-30.2	0.0257	0.0257	0.0
Healthcare and support services	0.0543	0.0849	-12.1	0.0455	0.0455	0.0
Social and cultural services	0.0392	0.0254	7.8	0.0112	0.0112	0.0
Households as employers	0.0003	0.0005	-0.9	0.00006	0.00006	0.0
Employer size						
< 5	0.1625	0.0449	39.3	0.1014	0.0981	1.1
5 - 9	0.1154	0.0461	25.6	0.1009	0.0977	1.1
10 - 19	0.1196	0.0628	19.8	0.1136	0.1118	0.6
20 - 49	0.1551	0.1122	12.6	0.1653	0.1652	0.0
50 - 99	0.0821	0.0753	2.5	0.0845	0.0866	-0.8
100 - 199	0.0700	0.0774	-2.8	0.0749	0.0787	-1.5
200 - 499	0.0819	0.1053	-8.0	0.0934	0.0983	-1.7
500 - 999	0.0518	0.0743	-9.3	0.0617	0.0611	0.2
⩾ 1000	0.1612	0.4012	-55.4	0.2041	0.2020	0.5
Nr. of employers	2.0411	1.6561	35.2	1.7019	1.7146	-1.3
Nr. of quarters unemployed	2.3338	0.9669	40.5	1.2817	1.3079	-0.9
Employer tenure	10.25	11.964	-29.3	12.263	12.149	2.0
Industry tenure	12.224	14.544	-46.4	14.032	13.938	2.0
Daily wage	124.88	118.51	7.3	109.19	109.19	0.0
Wage growth	0.0699	0.0686	0.2	0.0632	0.0489	2.3
Partner works	0.2973	0.2371	13.6	0.2835	0.2860	-0.6
Partner works#partner daily wage	31.572	24.17	9.5	27.355	26.727	1.3
Partner entrepreneurship experience	0.0526	0.0306	11.0	0.0296	0.0314	-1.0
Observations	87614	835969		32473	32473	

Comparison of means between entrepreneurs and matched employees in the full and matched samples in the first quarter of 2004. The standardized % bias is the % difference of the sample means between the entrepreneurs and matched employees as a percentage of the square root of the average of the sample variances of both groups

Table A2: Balance of a k2k Coarsened exact matched sample

	Entrepreneurs	Wage employees	Standardized %bias
Age			
22-24	0.108	0.108	0.0
25-29	0.319	0.319	0.0
30-34	0.253	0.253	0.0
35-39	0.170	0.170	0.0
40-44	0.098	0.098	0.0
45-49	0.052	0.052	0.0
Female	0.261	0.261	0.0
Flanders	0.640	0.640	0.0
Wallonia	0.260	0.260	0.0
Brussels	0.100	0.100	0.0
Household position			
Living with parents	0.151	0.151	0.0
Single	0.160	0.160	0.0
Cohabiting - 0 children	0.219	0.219	0.0
Cohabiting – 1 child	0.172	0.172	0.0
Cohabiting – 2 children	0.167	0.167	0.0
Cohabiting - 3> children	0.071	0.071	0.0
Head 1 parent family - 1 child	0.013	0.013	0.0
Head 1 parent family – 2> children	0.012	0.012	0.0
Other	0.035	0.035	0.0
Occupation			
Blue-collar	0.330	0.330	0.0
White-collar/Govt. official	0.668	0.668	0.0
Public sector	0.053	0.053	0.2
Employer Industry			
Manufacturing	0.155	0.155	0.0
Electricity, gas, water	0.002	0.002	0.0
Construction	0.138	0.138	0.0
Wholesale and retail trade	0.223	0.223	0.0
Hotels and restaurants	0.034	0.034	0.0
Transport, storage, communication	0.075	0.075	0.0
Financial institutions	0.042	0.042	0.0
Real estate and professional services	0.205	0.205	0.0
Public administration, defence	0.013	0.013	0.0
Education	0.014	0.014	0.0
Healthcare and support services	0.052	0.052	0.0
Social and cultural services	0.048	0.048	0.0
Households as employers	0.000	0.000	0.0
Employer size			
< 5	0.165	0.165	0.0
5-9	0.119	0.119	0.0
10-19	0.132	0.132	0.0
20-49	0.163	0.163	0.0
50-99	0.091	0.091	0.0
100-199	0.079	0.079	0.0
200 - 499	0.082	0.082	0.0
500 - 999	0.054	0.054	0.0
≥ 1000	0.113	0.113	0.0
Nr. of employers	2.203	2.206	-0.2
Nr. of quarters unemployed	2.422	2.291	3.4
Employer tenure	9.084	9.047	0.6
Industry tenure	11.360	11.373	-0.2
Daily wage	12.126	11.944	3.0
Wage growth	0.059	0.054	0.8
Partner works	0.285	0.284	0.1
Partner works#partner daily wage	2.987	2.822	3.2
Partner entrepreneurship experience	0.050	0.051	-0.7
Observations	9835	9835	

Comparison of means between entrepreneurs and matched employees in the first quarter of 2004 for a k2k Coarsened Exact Matched sample. Mean values for most of the variables of this sample are highly similar to those obtained after the propensity score matching. Because we imposed to use the existing categories of the categorical variables as bins to create the strata, there is perfect balance on all these variables, but at the cost of much more observations being discarded to obtain balance and an equal number of observations in both the entrepreneur and control groups than in the propensity score matching. To increase this number would require further coarsening of both the continuous and categorical variables, leading to non-zero imbalance on most of the (categorical) variables.

Table A3: Balance of retained and dropped samples

	Retained	**Dropped**	Std. % bias
Age			
22-24	0.0841	0.0865	-0.9
25-29	0.3092	0.3219	-2.7
30-34	0.2767	0.2659	2.4
35-39	0.1923	0.1717	5.3
40-44	0.1078	0.1031	1.5
45-49	0.0300	0.0511	-10.7
Female	0.1056	0.2166	-30.5
Region			
Flanders	0.7665	0.7879	-5.2
Wallonia	0.2156	0.1924	5.8
Brussels	0.0179	0.0197	-1.3
Household position			
Living with parents	0.1926	0.1919	0.2
Single	0.1171	0.1211	-1.2
Cohabiting - no children	0.1941	.2047	-2.7
Cohabiting - 1 child	0.1742	0.1800	-1.5
Cohabiting - 2 children	0.2067	0.1980	2.2
Cohabiting - 3 or more children	0.0691	0.0633	2.3
Head 1 parent family - 1 child	0.0082	0.0087	-0.5
Head 1 parent family - 2 or more children	0.0068	0.0068	0.0
Other	0.0316	0.0253	3.5
Occupation			
Blue-collar	0.5207	0.4690	10.4
White-collar	0.4355	0.4840	-9.7
Govt. official	0.0439	0.0471	-1.5
Employer sector			
Public	0.0764	.0893	-4.7
Employer Industry			
Manufacturing	0.2423	0.2540	-2.7
Electricity, gas, water	0.0000	.0005	-3.1
Construction	0.2317	0.1869	11.0
Wholesale and retail trade	0.2153	0.2061	2.2
Hotels and restaurants	0.0102	0.0099	0.3
Transport, storage, communication	0.0826	0.0621	7.9
Financial institutions	0.0296	0.0333	-2.1
Real estate and professional services	0.1203	0.1323	-3.6
Public administration, defence	0.0311	0.0279	1.9
Education	0.0190	0.0266	-5.1
Healthcare and support services	0.0113	0.0487	-22.0
Social and cultural services	0.0066	0.0117	-5.4
Households as employers	0.0000	0.0003	-0.8
Employer size			
< 5	0.0917	0.1005	-3.0
5 - 9	0.0996	0.0993	0.1
10 - 19	0.1083	0.1131	-1.5
20 - 49	0.1888	0.1631	6.7
50 - 99	0.0928	0.0849	2.8
100 - 199	0.0802	0.0765	1.4
200 - 499	0.1014	0.0954	2.0
500 - 999	0.0556	0.0610	-2.7
$\geqslant 1000$	0.1815	0.2051	-6.0
Nr. of jobs	1.8957	1.7339	11.8
Employer tenure (quarters)	11.712	12.257	-9.7
Daily wage	108.98	109.25	-0.8
Observations	5,470	59,476	

Comparison of means of selected variables between the retained and dropped subsamples in the 1st quarter of 2004.

Table A4: Comparisons at pre-entry time of entrepreneurs with and without a mover matched pair

Matched employees changes jobs	Mover at entry in entrepreneurship				Mover at re-entry in wage work			
	Mean		t-test		Mean		t-test	
Age	Yes	No	t	p>t	Yes	No	t	p>t
22-24	0.02664	0.0178	1.28	0.199	0.0241	0.01811	0.93	0.353
25-29	0.25	0.18781	3.12	0.002	0.19449	0.20009	-0.30	0.764
30-34	0.36066	0.27948	3.57	0.000	0.30293	0.29155	0.53	0.593
35-39	0.18648	0.24344	-2.70	0.007	0.22719	0.23491	-0.39	0.696
40-44	0.1127	0.17134	-3.20	0.001	0.16179	0.16063	0.07	0.946
45-49	0.05123	0.08189	-2.31	0.021	0.07229	0.07753	-0.42	0.673
50-54	0.01025	0.0178	-1.19	0.234	0.01549	0.01671	-0.21	0.837
55-59	0.00205	0.00045	1.19	0.235	0.00172	0.00046	0.99	0.320
Female	0.10246	0.10636	-0.25	0.799	0.08434	0.11142	-1.89	0.060
Region								
Flanders	0.84016	0.74944	4.30	0.000	0.76592	0.76555	0.02	0.985
Wallonia	0.14959	0.23186	-4.01	0.000	0.21687	0.21727	-0.02	0.983
Brussels	0.01025	0.01869	-1.30	0.193	0.01721	0.01718	0.01	0.995
Household position								
Living with parents	0.10861	0.10903	-0.03	0.978	0.11188	0.10817	0.25	0.799
Single	0.13115	0.12328	0.48	0.633	0.12909	0.12349	0.36	0.717
Cohabiting - 0 children	0.18648	0.17713	0.49	0.625	0.16179	0.18338	-1.21	0.228
Cohabiting - 1 child	0.21107	0.1927	0.93	0.354	0.2117	0.19174	1.08	0.282
Cohabiting - 2 children	0.25	0.25501	-0.23	0.818	0.26334	0.25162	0.58	0.565
Cohabiting - 3 or more children	0.07172	0.09613	-1.69	0.091	0.08606	0.09331	-0.54	0.591
Head 1 parent family - 1 child	0.0082	0.00801	0.04	0.967	0.00516	0.00882	-0.88	0.381
Head 1 parent family - 2 or more children	0.01025	0.00757	0.60	0.548	0.00861	0.01161	-0.62	0.538
Other	0.01844	0.01869	-0.04	0.971	0.01549	0.0195	-0.63	0.526
Occupation								
Blue-collar	0.54303	0.49666	1.86	0.063	0.52496	0.49954	1.09	0.277
White collar	0.43443	0.45972	-1.02	0.309	0.46127	0.45357	0.33	0.741
Govt. Official	0.02254	0.04361	-2.16	0.031	0.01377	0.04689	-3.63	0.000
Job regime								
Full-time	1	1	.	.	1	1	.	.
Public sector	0.04918	0.07788	-2.21	0.027	0.04131	0.08124	-3.29	0.001
Employer Industry								
Manufacturing	0.15984	0.21451	-2.72	0.007	0.16695	0.21495	-2.55	0.011
Electricity, gas, water	0.00205	0.00089	0.70	0.483	0	0.00139	-0.90	0.368
Construction	0.28893	0.23097	2.71	0.007	0.25473	0.2377	0.85	0.395
Wholesale and retail trade	0.20697	0.21451	-0.37	0.712	0.22031	0.21123	0.47	0.636
Hotels and restaurants	0.01025	0.01202	-0.33	0.742	0.01893	0.00975	1.83	0.068
Transport, storage, communication	0.09221	0.08767	0.32	0.749	0.09639	0.08635	0.76	0.450
Financial institutions	0.03279	0.03204	0.08	0.933	0.0327	0.03203	0.08	0.935
Real estate and professional services	0.15574	0.12461	1.85	0.064	0.16351	0.12117	2.69	0.007
Public administration, defence	0.03074	0.03204	-0.15	0.882	0.01377	0.03668	-2.80	0.005
Education	0.0082	0.02136	-1.93	0.054	0.01377	0.02043	-1.04	0.297
Healthcare and support services	0.00615	0.01424	-1.44	0.149	0.00688	0.01439	-1.43	0.153
Social and cultural services	0.00205	0.01335	-2.14	0.033	0.00861	0.01207	-0.70	0.484
Households as employers	0	0.00045	-0.47	0.641	0	0.00046	-0.52	0.604
Employer Size								
< 5	0.18033	0.12639	3.16	0.002	0.15491	0.13092	1.50	0.135
5-9	0.13934	0.11259	1.66	0.096	0.13425	0.11281	1.42	0.154
10-19	0.13115	0.1166	0.90	0.369	0.13597	0.11467	1.41	0.160
20 - 49	0.17418	0.18069	-0.34	0.734	0.18761	0.17734	0.57	0.567
50 - 99	0.09426	0.081	0.96	0.337	0.08262	0.08357	-0.07	0.941
100 - 199	0.04918	0.06631	-1.41	0.159	0.06196	0.0636	-0.14	0.885
200 - 499	0.07992	0.085	-0.37	0.714	0.09466	0.08124	1.03	0.301
500 - 999	0.03893	0.05251	-1.25	0.213	0.03787	0.05339	-1.52	0.128
≤ 1000	0.1127	0.17891	-3.56	0.000	0.11015	0.18245	-4.16	0.000
Nr. of employers	2.9857	2.7512	2.07	0.038	3.0052	2.7358	2.55	0.011
Employer tenure	15.209	16.509	-2.21	0.027	16.413	16.24	0.31	0.755
Daily wage	122.62	128.72	-2.33	0.020	133.7	125.99	3.15	0.002
Observations	488	2,274			581	2,154		

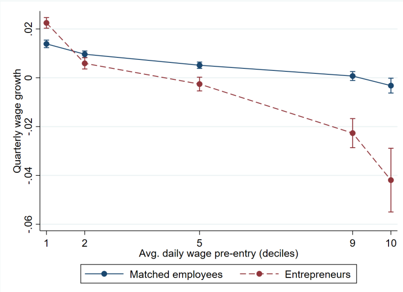


Figure A1: Full sample

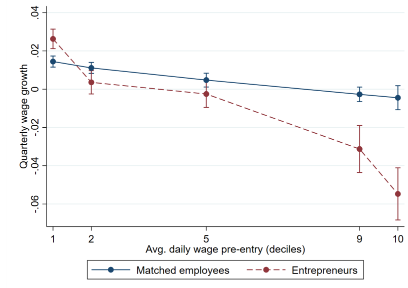


Figure A2: Part-time workers

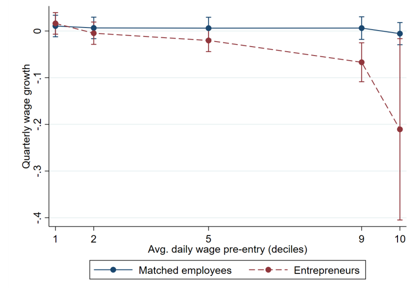


Figure A3: Temporary workers

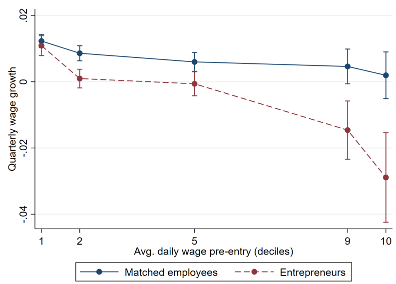


Figure A4: Necessity entry into entrepreneurship

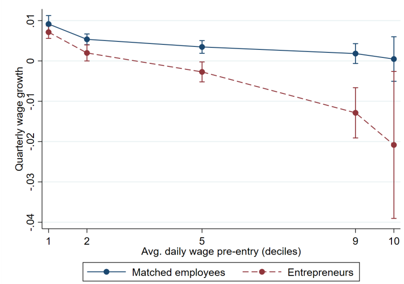


Figure A5: Necessity exit out of entrepreneurship

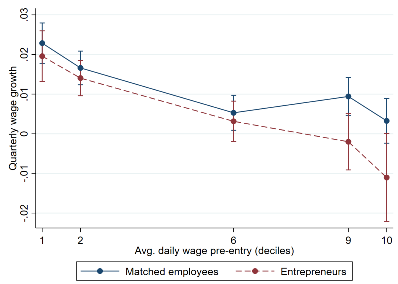


Figure A6: Predictive margins of entrepreneurs and matched employees who change jobs at the time entrepreneurs enter into entrepreneurship

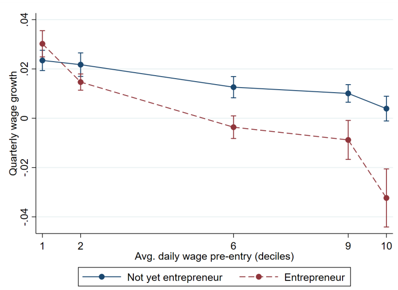


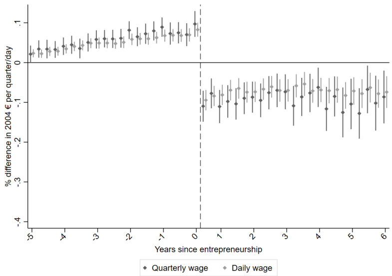
Figure A7: Predictive margins of entrepreneurs who return to wage work before or at the 1st quarter of 2011 and of "Not yet" entrepreneurs who did not enter entrepreneurship until the 2nd quarter of 2011

B Chapter 2

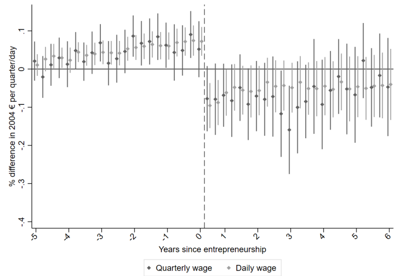
Table B1: Selection into small firms after entrepreneurship

Small firm	(1) Linear probability	(2) Probit
Ln(mean entrepreneurial earnings)	-0.032	-0.090
	(0.024)	(0.065)
Duration of entrepreneurial spell	0.002	0.004
	(0.001)	(0.003)
Female	-0.094**	-0.260**
	(0.032)	(0.087)
Electricity, gas, water	-0.505*	-1.377
	(0.248)	(0.761)
Construction	-0.072	-0.202
	(0.058)	(0.162)
Wholesale and retail trade	-0.127*	-0.346*
	(0.058)	(0.161)
Hotels and restaurants	-0.306***	-0.814***
	(0.065)	(0.179)
Transport, storage, communication	-0.192**	-0.515**
	(0.073)	(0.197)
Financial institutions	0.047	0.139
	(0.090)	(0.258)
Real estate and professional services	-0.247***	-0.656***
	(0.057)	(0.158)
Education	-0.399***	-1.114**
	(0.113)	(0.369)
Healthcare and support services	-0.424***	-1.236***
	(0.076)	(0.247)
Social and cultural services	-0.224**	-0.592**
	(0.072)	(0.195)
Constant	1.875***	1.384*
	(0.334)	(0.699)
Observations	2,108	2,104
(Pseudo) R^2	0.087	0.065

Linear probability and probit regressions on the likelihood to work for a small firm (< 100 employees) right after entrepreneurship. Omitted categories: Male, Manufacturing industry. Robust standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

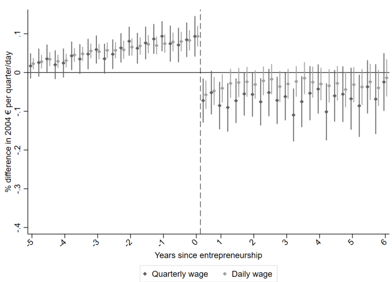


(a) Industry Stayers

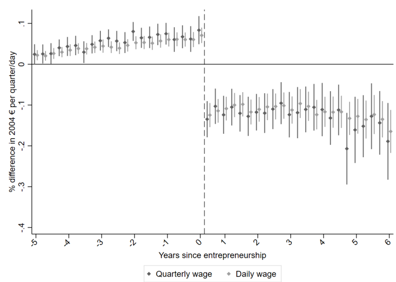


(b) Industry Switchers

Figure B1: Quarterly (dark) and daily (light) wage losses of entrepreneurs returning to wage work. Figure B1a reports coefficient estimates for entrepreneurs who start working in the same NACE industry after entrepreneurship as they worked in right before entering entrepreneurship (at the 2 digit level). Figure B1b reports coefficient estimates for entrepreneurs who move to a different industry after entrepreneurship. Vertical bars represent 95% confidence intervals.



(a) Young



(b) Old

Figure B2: Quarterly (dark) and daily (light) wage losses of entrepreneurs returning to wage work. Figure B2a reports coefficient estimates for entrepreneurs who are younger than 40 at the time they re-enter wage work after entrepreneurship. Figure B2b reports coefficient estimates for entrepreneurs who are 40 or older at the time of re-entry. Vertical bars represent 95% confidence intervals.

C Chapter 3

Table C1: Impact of hurricanes on Recovery and Rebuilding Employment by Firm Age in Coastal Watershed Counties

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
4+ years before hurricane	-0.033	-0.011	-0.083	-0.000	-0.030
	(0.030)	(0.056)	(0.060)	(0.083)	(0.037)
3 years before hurricane	0.002	-0.063	-0.025	-0.085	0.016
	(0.029)	(0.055)	(0.066)	(0.050)	(0.035)
2 years before hurricane	-0.007	0.041	-0.055	0.034	-0.007
	(0.016)	(0.058)	(0.063)	(0.056)	(0.016)
0 years after hurricane	0.009	0.032	0.077	-0.064	0.008
	(0.035)	(0.070)	(0.074)	(0.065)	(0.033)
1 year after hurricane	0.060*	0.218***	0.034	0.105	0.048
	(0.030)	(0.062)	(0.062)	(0.073)	(0.031)
2 years after hurricane	0.079**	0.203**	0.026	0.086	0.072**
	(0.025)	(0.063)	(0.078)	(0.065)	(0.025)
3 years after hurricane	0.043*	0.213**	0.076	0.018	0.033
	(0.021)	(0.066)	(0.071)	(0.071)	(0.023)
4 years after hurricane	0.044	0.156*	0.099*	0.091	0.041
	(0.025)	(0.060)	(0.045)	(0.064)	(0.026)
5 years after hurricane	0.037	0.049	0.167**	0.120*	0.034
	(0.025)	(0.055)	(0.055)	(0.054)	(0.026)
6+ years after hurricane	0.001	0.147**	0.088	0.075	-0.006
	(0.032)	(0.050)	(0.068)	(0.072)	(0.032)
Observations	21,575	21,575	21,575	21,575	21,575
R^2	0.992	0.927	0.927	0.919	0.991

Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. The sample is restricted to coastal watershed counties. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Table C2: Impact of hurricanes on Recovery and Rebuilding Employment by Firm Age in Coastal Watershed Counties: Category 1 vs. Category 2 hurricanes

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
Panel A. Category 1 hurricanes					
4+ years before hurricane	-0.007	-0.019	-0.078	0.003	0.001
	(0.026)	(0.063)	(0.068)	(0.078)	(0.029)
3 years before hurricane	0.018	-0.022	-0.048	-0.083	0.036
	(0.031)	(0.052)	(0.071)	(0.062)	(0.039)
2 years before hurricane	-0.016	0.039	-0.036	0.023	-0.021
	(0.017)	(0.062)	(0.067)	(0.058)	(0.019)
0 years after hurricane	-0.016	0.036	0.081	-0.056	-0.018
	(0.023)	(0.069)	(0.062)	(0.055)	(0.022)
1 year after hurricane	0.024	0.183**	0.063	0.097	0.013
	(0.023)	(0.067)	(0.052)	(0.066)	(0.023)
2 years after hurricane	0.041	0.133*	0.056	0.106	0.038
	(0.022)	(0.066)	(0.081)	(0.065)	(0.023)
3 years after hurricane	0.021	0.163*	0.078	0.071	0.012
	(0.018)	(0.076)	(0.073)	(0.068)	(0.018)
4 years after hurricane	0.025	0.141*	0.097	0.123	0.021
	(0.019)	(0.059)	(0.061)	(0.067)	(0.019)
5 years after hurricane	0.026	0.008	0.163*	0.128	0.023
	(0.024)	(0.061)	(0.065)	(0.072)	(0.024)
6+ years after hurricane	0.012	0.143*	0.155**	0.104	0.003
	(0.038)	(0.062)	(0.050)	(0.081)	(0.037)
Panel B. Category 2 hurricanes					
4+ years before hurricane	-0.139	0.036	-0.022	-0.008	-0.166
	(0.086)	(0.077)	(0.096)	(0.173)	(0.116)
3 years before hurricane	-0.078	-0.193	0.109	-0.004	-0.096
	(0.058)	(0.147)	(0.146)	(0.070)	(0.068)
2 years before hurricane	0.039	0.018	-0.088	0.050	0.061
	(0.032)	(0.094)	(0.099)	(0.074)	(0.045)
0 years after hurricane	0.115	-0.021	-0.014	-0.034	0.117
	(0.098)	(0.141)	(0.180)	(0.176)	(0.109)
1 year after hurricane	0.165	0.167*	-0.131	0.037	0.156
	(0.084)	(0.074)	(0.174)	(0.196)	(0.106)
2 years after hurricane	0.164**	0.310*	-0.129	-0.089	0.145*
	(0.061)	(0.133)	(0.141)	(0.172)	(0.057)
3 years after hurricane	0.092*	0.218	-0.011	-0.227	0.086*
	(0.036)	(0.133)	(0.103)	(0.176)	(0.043)
4 years after hurricane	0.075	0.066	0.009	-0.132	0.084
	(0.056)	(0.123)	(0.107)	(0.165)	(0.056)
5 years after hurricane	0.042	0.173	0.021	-0.028	0.040
	(0.049)	(0.112)	(0.097)	(0.206)	(0.046)
6+ years after hurricane	-0.042	0.021	-0.268	-0.113	-0.034
	(0.053)	(0.095)	(0.194)	(0.148)	(0.048)
Observations	21,575	21,575	21,575	21,575	21,575
R^2	0.992	0.927	0.927	0.919	0.991

Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Panel A shows the results for counties that are hit by Category 1 wind speeds. Panel B shows the results for counties that are hit by Category 2 wind speeds. The sample is restricted to coastal watershed counties. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Table C3: Impact of hurricanes on Recovery and Rebuilding Employment by Firm Age: No Controls

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
4+ years before hurricane	-0.027	-0.027	-0.119*	-0.061	-0.006
	(0.032)	(0.045)	(0.050)	(0.083)	(0.041)
3 years before hurricane	0.012	-0.085	-0.061	-0.081	0.034
	(0.030)	(0.055)	(0.065)	(0.042)	(0.037)
2 years before hurricane	-0.008	0.035	-0.093	0.020	-0.004
	(0.015)	(0.059)	(0.057)	(0.046)	(0.015)
0 years after hurricane	0.004	0.045	0.032	-0.074	0.003
	(0.035)	(0.071)	(0.083)	(0.060)	(0.033)
1 year after hurricane	0.055	0.209***	0.046	0.062	0.042
	(0.032)	(0.056)	(0.062)	(0.067)	(0.032)
2 years after hurricane	0.068**	0.226***	0.048	0.038	0.056*
	(0.025)	(0.054)	(0.073)	(0.062)	(0.023)
3 years after hurricane	0.031	0.210***	0.094	0.004	0.016
	(0.020)	(0.058)	(0.060)	(0.055)	(0.021)
4 years after hurricane	0.030	0.135*	0.134***	0.074	0.021
	(0.024)	(0.053)	(0.039)	(0.050)	(0.024)
5 years after hurricane	0.024	0.062	0.171***	0.105*	0.014
	(0.025)	(0.055)	(0.051)	(0.048)	(0.025)
6+ years after hurricane	-0.025	0.139*	0.097	0.074	-0.040
	(0.033)	(0.056)	(0.069)	(0.070)	(0.032)
Observations	51,025	51,025	51,025	51,025	51,025
R^2	0.991	0.899	0.898	0.893	0.989

Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects and quarter fixed effects.

Table C4: Impact of hurricanes on Recovery and Rebuilding Employment by Firm Age: County Linear Trends

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
4+ years before hurricane	0.067***	0.104	-0.019	0.089	0.085***
	(0.017)	(0.073)	(0.047)	(0.075)	(0.020)
3 years before hurricane	-0.027	0.016	0.007	0.031	
	(0.020)	(0.057)	(0.063)	(0.048)	(0.026)
2 years before hurricane	0.003	0.053	-0.047	0.082	-0.001
	(0.015)	(0.054)	(0.055)	(0.054)	(0.013)
0 years after hurricane	0.005	0.034	0.044	-0.057	0.001
	(0.038)	(0.074)	(0.086)	(0.064)	(0.035)
1 year after hurricane	0.051	0.182**	0.034	0.058	0.038
	(0.033)	(0.061)	(0.063)	(0.068)	(0.033)
2 years after hurricane	0.059*	0.186**	0.024	0.016	0.046*
	(0.026)	(0.057)	(0.077)	(0.061)	(0.023)
3 years after hurricane	0.022	0.165**	0.059	-0.037	0.010
	(0.015)	(0.062)	(0.061)	(0.052)	(0.016)
4 years after hurricane	0.016	0.077	0.082	0.003	0.013
	(0.015)	(0.055)	(0.046)	(0.043)	(0.014)
5 years after hurricane	0.005	-0.015	0.097*	0.005	0.004
	(0.011)	(0.052)	(0.048)	(0.040)	(0.010)
6+ years after hurricane	-0.002	0.028	-0.047	-0.079	0.008
	(0.021)	(0.069)	(0.054)	(0.054)	(0.020)
Observations	51,025	51,025	51,025	51,025	51,025
R^2	0.995	0.916	0.914	0.908	0.994

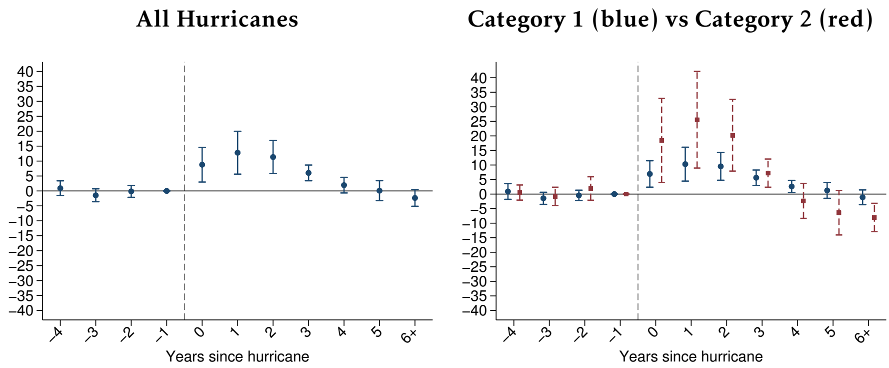
Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, county linear trends, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Table C5: Impact of Hurricanes on Recovery and Rebuilding Employment by Firm Age: Concise Event Study

	(1) All Firms	(2) Startups	(3) 2-3 year-olds	(4) 4-5 year-olds	(5) 6+ year-olds
0-1 years after hurricane	0.043	0.139*	0.120	0.063	0.030
	(0.039)	(0.058)	(0.062)	(0.069)	(0.040)
2-3 years after hurricane	0.061	0.209***	0.148*	0.080	0.045
	(0.032)	(0.057)	(0.069)	(0.057)	(0.036)
4-5 years after hurricane	0.026	0.049	0.185***	0.126*	0.020
	(0.040)	(0.051)	(0.050)	(0.058)	(0.044)
6+ years after hurricane	-0.013	0.072	0.112	0.084	-0.020
	(0.042)	(0.062)	(0.079)	(0.071)	(0.043)
Observations	51,025	51,025	51,025	51,025	51,025
R-squared	0.992	0.902	0.901	0.895	0.990

Regressions of $ln(Emps)$ on years since a hurricane strike for firms in different age categories in the sectors related to recovery and rebuilding are shown. Observations are at the county-firm age level. Standard errors clustered at the commuting zone level are shown in parentheses. Controls include county fixed effects, quarter fixed effects, quarter fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

Figure C1: The Effect of Hurricanes on House Prices



Notes: Point estimates and 95 percent confidence intervals from regressions of *HPI* on years since a hurricane strike for All Hurricanes, and for Category 1 (blue) and Category 2 (red) winds separately. The HPI in 2015 = 100. Standard errors are clustered at the commuting zone level. Controls include county fixed effects, year fixed effects, year fixed effects linear in 2000 county characteristics, and an indicator for whether a county experienced a hurricane in the ten years before 2000.

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Faculty of Economics and Business

Department of Management, Strategy and Innovation

Naamsestraat 69
3000 LEUVEN, België
tel. +32 16 37 79 03
jeroen.mahieu@kuleuven.be
www.kuleuven.be