Entrepreneurial Organizations and Human Capital

by

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ABSTRACT

This dissertation investigates how human capital shapes both the creation a nd performance of entrepreneurial organizations. In three essays, I study the intricate linkage between startups and the individuals that embody them – which include not only the founders, but also the non-founding joiners. In the first essay, my co-authors and I empirically assess the popular view that the most successful entrepreneurs tend to be young. Second, I investigate the types of individuals that choose to work for startups rather than established firms, and the resulting wage differential between the two employer types. Third, I study the effectiveness of high-tech startup acquisitions as a hiring strategy for incumbent firms – commonly known as "acqui-hiring."

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Chapter 1

Entrepreneurial Organizations and Human Capital: Introduction and Overview

In a seminal essay in 1965, Arthur Stinchcombe made a key observation: new firms exhibit higher rates of mortality compared to their older counterparts. This simple and yet remarkably consistent premise has galvanized generations of entrepreneurship research.

A central theme in this line of inquiry has been investigating why the vast majority of firms achieve little to no growth, while a handful of organizations go on to experience outsized success. While the extreme skewness in survival and growth outcomes among entrepreneurial firms is a well-documented phenomenon (e.g., Freeman, Carroll, and Hannan 1983; Decker et al 2014; Guzman and Stern, 2016), understanding the sources and causes of such dispersion is still in its infancy. How do entrepreneurial firms grow in the absence of organizational reputation and routines? More fundamentally in explaining this growth, who chooses to join these new firms – in spite of Stinchcombe's "liability of newness" – rather than established firms, and why? How does a startup attract top talent that enables the organization to execute its strategic goals and thereby rapidly scale?

This dissertation aims to advance our understanding of how human capital shapes both the creation and performance of entrepreneurial organizations. Through a collection of three essays, I study the intricate linkage between startups and the individuals that embody them – which include not only the founders, but also the non-founding joiners.

The academic literature on entrepreneurship has long recognized the importance of talent for new ventures. This stream of research has almost exclusively focused on the founders and their roles in starting and growing nascent firms. For instance, existing research demonstrates that founders are pivotal for identifying new technological opportunities, laying out a strategic vision, and successfully commercializing ideas into profitable businesses (Kirzner 1997; Ruef et al. 2003; Lazear 2005).

However, the prevailing theoretical and empirical focus on the founders leaves the human capital piece of entrepreneurship under-explored. Largely due to data constraints, very little is known regarding early employees – the first set of non-founder employees that join startup companies (Stuart and Sorenson 2005; Roach and Sauermann 2015). Although founders are undoubtedly important, highly skilled employees play a critical role in the growth and success of nascent firms. Therefore, efficiently acquiring human capital is a central challenge for young high-technology companies (Baron et al., 1996; Hsu, 2008; Wasserman, 2012). Consistent with this view, a practitioner survey of 869 founders indicates that their number one concern as entrepreneurs is hiring good talent.[1]

A comprehensive view of startup employment – one that includes but extends beyond the founders – is an important area of research because young firms account for a disproportionately high share of new jobs in the economy (Haltiwanger, Jarmin, and Miranda 2013). Given the outsized role of startup employment, many important questions remains open: Who are these early joiners and why do they choose to join a startup rather than an established company? Do startups create well-paying and stable jobs? How do early joiners strategically shape trajectory of their young employers?

Though conceptually motivated, a major empirical challenge in rigorously answering these questions is the lack of data that systematically captures these employees. To overcome this challenge, I have leveraged various empirical settings to offer a window of insights into these questions. These unique datasets and accompanying methodology are tightly linked to the questions that I seek to answer.

I address some of these questions in three essays. First, focusing on founders, my co-authors and I empirically assess the popular view that the most successful entrepreneurs tend to be young. This hypothesis of the youth advantage in entrepreneurship appears to deviate from standard economic theory in which work experience positively drives job performance. To

[1] See First Round Capital's annual survey results: http://stateofstartups.firstround.com/2016/.

resolve this puzzle, we leverage administrative data from the US Census and IRS to analyze the age of all business founders in the US in recent years. We find that the average age of entrepreneur at the time of founding is 42. Even when focusing on the most successful entrepreneurs – those who reach top 0.1% employment (or revenue) growth or achieving successful exits (e.g. IPO) – we find that successful entrepreneurs tend to be middle-aged, not young. To unpack the mechanism behind the age advantage, we document that prior work experience – especially in the same industry as the startup – is positively associated with entrepreneurial performance. As a result, we empirically illustrate that human capital, which accumulates with age in the form of industry experience, social ties, and financial resources, is a key driver behind successful venturing.

In the second essay, I focus on the non-founding employees of startups. In particular, I investigate the types of individuals who choose to work for startups rather than established firms, and the resulting wage differential between the two employer types. I tackle this question by leveraging MIT students who receive multiple job offers upon graduation. This empirical framework allows me to compare within-person wages of startups versus large firms, and identify the types of students who choose to join startups over other employers. I find that startups pay competitive wages relative to established companies, and that the students who express preferences for risky and challenging work are much more likely to join startup employers. More broadly, these results demonstrate that high-growth startups attract and frequently hire top-tier talent by outcompeting established firms in the labor market.

In the third essay, having documented startups as a promising source of human capital, I examine whether established firms can harness this entrepreneurial talent by acquiring their younger counterparts. More specifically, I study the effectiveness of high-tech startup acquisitions as a hiring strategy for incumbent firms – commonly known as "acqui-hiring". Unlike conventional hires who choose to join a new firm on their own volition, most acquired employees do not have a voice in the decision to be acquired, much less by whom to be acquired. Startup acquisitions therefore provide an empirical setting in which non-founding employees – from these individuals' perspective – are quasi-randomly assigned a new employer. I argue that the lack of worker choice lowers the average match quality between the acquired employees and the acquiring firm, leading to elevated rates of turnover. Using comprehensive employee-employer matched data from the US Census, I document that acquired workers are significantly more likely to leave compared to regular hires. Moreover, I demonstrate that these departures can be largely predicted ex-ante. Leveraging population data on career histories, I construct a measure of "startup affinity" for each target firm based on pre-acquisition employment patterns, and show that this strongly predicts post-acquisition worker retention. Lastly, these departures suggest a deeper strategic cost of competitive spawning: Upon leaving, acquired workers are more likely to found their own companies, many of which appear to later compete against the buyer. Overall, these findings show that while established firms attempt to bring in talent by acquiring high-tech startups, it is typically difficult to retain these entrepreneurial workers due to the stark organizational differences.

Taken together, these essays enhance our knowledge of the rich interaction between human capital and entrepreneurial organizations. In particular, this stream of work broadens our perspective on employment at startup firms by systematically studying the non-founding employees alongside the founders. A key lesson is that while startups are a source of top-tier talent, these workers are not necessarily transferable to other organizational types, as evidenced in the poor rates of employee retention following startup acquisitions.

I conclude by highlighting a set of open questions for future research. First, what are the long-term consequences of working for a startup? This is an important question because entrepreneurship is inherently a risky endeavor with high failure rates, meaning that a large share of workers who join startups will experience the demise of their employers. While joiners may not be able to directly steer the fate of their young employers, these organizational outcomes, ranging from an outright failure to a blockbuster IPO, may alter – albeit perhaps unfairly – the long-term trajectory of these individuals. These effects are likely at both the intensive (e.g., wages) and extensive (e.g., unemployment) margins. Relatedly, the joiners' experience of working for a startup may influence their own likelihood of becoming an entrepreneur in the future, as well as the resulting performance as a founder. These insights will undoubtedly inform the policy efforts designed to promote entrepreneurship by shedding light on the "spillover" consequences of startup employment. In addition, how can established firms effectively retain and manage workers from acquired startups? Insofar as many startup acquisitions are motivated by a desire to bring in talent, effectively managing the target firm's employees directly shapes the acquirer's financial and strategic returns from the acquisition.

More broadly, the interplay between talent and startups is a fertile area for future research – one where I hope to make a scholarly impact over the next several years.

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Chapter 2

Age and High-Growth Entrepreneurship

(with Pierre Azoulay, Benjamin Jones, and Javier Miranda)

Abstract: Many observers, and many investors, believe that young people are especially likely to produce the most successful new firms. Integrating administrative data on firms, workers, and owners, we study startups systematically in the U.S. and find that successful entrepreneurs are middle-aged, not young. The mean age at founding for the 1-in-1,000 fastest growing new ventures is 45.0. The findings are similar when considering high-technology sectors, entrepreneurial hubs, and successful firm exits. Prior experience in the specific industry predicts much greater rates of entrepreneurial success. These findings strongly reject common hypotheses that emphasize youth as a key trait of successful entrepreneurs.

"Young people are just smarter," Mark Zuckerberg, founder of Facebook

"The cutoff in investors' heads is 32...after 32, they start to be a little skeptical."

Paul Graham, venture capitalist and founder of Y Combinator

1 Introduction

Entrepreneurship has long been heralded as a key driver of rising living standards (Smith 1776, Schumpeter 1942, Lucas 1978), but successful entrepreneurship is rare, with the vast majority of entrepreneurs failing to provide the major innovations or creative destruction that can drive economic growth (Glaeser 2009; Haltiwanger et al. 2013; Guzman and Stern 2017; Levine and Rubenstein 2017). In understanding entrepreneurship, and the rarity of substantial success, a key set of questions surrounds the traits of the entrepreneurs themselves. In this paper, we provide wide-ranging evidence about one trait often thought to play a central role: the founders' age.

The view that young people are especially capable of producing big ideas – whether in scientific research, invention, or entrepreneurship – is common and longstanding (see, e.g., Jones et al. 2014). Among the advantages of youth in technology and innovation, young people are sometimes argued to be cognitively sharper, less distracted by family or other responsibilities, and more capable of transformative ideas – this last in line with "Planck's Principle", whereby younger people may be less beholden to existing paradigms of thought and practice (Planck 1949; Dietrich and Srinivasan 2007, Weinberg 2006, Jones 2010, Azoulay et al. 2018). Famous individual cases such as Bill Gates, Steve Jobs, and Mark Zuckerberg show that people in their early 20s can create eventually world-leading companies. Meanwhile, venture capital firms appear to emphasize youth as a key criteria in targeting their investments, which has led to charges of "ageism" in Silicon Valley.[2] At one extreme, Peter Thiel, the co-founder of PayPal, has created a prominent fellowship program that provides $100,000 grants to would-be entrepreneurs so long as they are below age 23 and drop out of school.

Despite these potential advantages, young entrepreneurs may also face substantial disadvantages. Older entrepreneurs might access greater human capital, social capital, or financial

[2] Vinod Khosla, the co-founder of Sun Microsystems and a prominent venture capitalist, has argued that "people under 35 are the people who make change happen," and "people over forty-five basically die in terms of new ideas." (source: Vivek Wadhwa, "The Case for Old Entrepreneurs," *Washington Post*, December 2, 2011). For public debate around venture capital activity and potential "ageism" see, for example "The Brutal Ageism of Tech" (Scheiber 2014).

capital. Theories of entrepreneurship often take human-capital orientations (e.g., Lucas 1978; Kihlstrom and Laffont 1979; Iyigun and Owen 1998; Lazear 2004, 2005; Amaral et al. 2011), and empirical studies have found that human capital, including the acquisition of relevant market and technical knowledge, can predict entrepreneurial success (e.g., Dunn and Holtz-Eakin 2000, Fairlie and Robb 2007, Gruber et al. 2008, Chatterji 2009, Lafontaine and Shaw 2014). In deeper technological areas, young people may not have sufficient scientific knowledge to produce or manage effective R&D (e.g., Jones 2010). Age and experience may also be relevant when accessing financial capital, where younger individuals will have less time to build up capital needed to start a business and may face difficulties borrowing it (e.g., Evans and Jovanovic 1989; Stiglitz and Weiss 1981).[3] Whether such issues impose important constraints in the entrepreneurial context is less clear, especially to the extent that young entrepreneurs can overcome personal limitations by assembling effective teams, accessing third-party financing, and tapping social networks.

The empirical literature on the characteristics of highly successful entrepreneurs is limited and mixed. Various studies suggest that mean age for starting companies of all kinds (i.e., including restaurants, dry cleaners, retail shops, etc.) is in the late 30s or 40s (e.g., Dahl and Sorensen 2012, Kautonen et al. 2014), but the data in these studies are dominated by small businesses without growth ambitions and do not focus on the relatively rare start-ups with the potential to drive innovation and economic growth. Other research suggests that growth-oriented firms and the people who start them have distinct characteristics (e.g., Guzman and Stern 2017, Levine and Rubinstein 2017). Meanwhile, studies of technology firms in the U.S. find contrasting results. Roberts (1991), looking across small samples of tech entrepreneurs, finds a median founder age of 37 among 270 new ventures, while Wadhwa et al. (2008) use a telephone survey of 502 technology and engineering firms with at least $1 million in sales and find that the mean founder age was 39. Ng and Stuart (2016) connect Angel List and CrunchBase data to individual LinkedIn profiles and find, in sharp contrast, that the founding of tech ventures comes most commonly only 5 years after college graduation. Frick (2014) studies a sample of 35 VC-backed firms from the Wall Street Journal's Billion Dollar Startup Club list and finds a mean founder age of 31, echoing

[3] In Evans and Jovanovic (1989) the entrepreneur's wealth limits the amounts of funds she can access. Empirical evidence for this mechanism continues to be debated (e.g., Holtz-Eakin et al. 1994a, 1994b; Hurst and Lusardi 2004; Andersen and Nielsen 2012; Fort et al. 2013; Adelino et al. 2015).

the popular view that the most successful and transformative new ventures come from young people (Table A1 in the online appendix further characterizes popular perceptions).

In this paper, we deploy U.S. administrative datasets to investigate the link between age and high-growth entrepreneurship in a systematic manner. By linking (a) newly available IRS K-1 data, which identifies the initial owners of pass-through firms, with (b) U.S. Census Bureau datasets regarding businesses, employees, and individuals throughout the economy as well as (c) USPTO patent databases and third-party venture-capital databases, we provide systematic new facts about founder age and entrepreneurship.

While we will include results for all new firms, our emphasis is on founders of "growth-oriented" firms that can have large economic impacts and are often associated with driving an increasing standard of living (Schumpeter 1942, Glaeser 2009). To delineate growth-oriented start-ups, we use both *ex ante* and *ex post* measures. The ex-ante measures include being a participant in a high tech sector, owning a patent, or receiving VC backing. The ex-post measures examine growth outcomes directly for each firm. Our datasets allow us to investigate multiple measures of firm growth and success at the firm level, including exceptionally high employment and sales growth, as well as exit by acquisition or initial public offering.

Our primary finding is that successful entrepreneurs are middle-aged, not young. We find no evidence to suggest that founders in their 20s are especially likely to succeed. Rather, all evidence points to founders being especially successful when starting businesses in middle age or beyond, while young founders appear disadvantaged. Across the 2.7 million founders in the U.S. between 2007-2014 who started companies that go on to hire at least one employee, the mean age for the entrepreneurs at founding is 41.9. The mean founder age for the 1 in 1,000 highest growth new ventures is 45.0. The most successful entrepreneurs in high technology sectors are of similar ages. So too are the most successful founders in entrepreneurial regions of the U.S. While the prevalence of the highest-growth companies having middle-aged founders is due in part to the prevalence of entry by the middle-aged, we further find that the "batting average" for creating successful firms is rising dramatically with age. Conditional on starting a firm, a 50-year-old founder is 1.8 times more likely to achieve upper-tail growth than a 30-year-old founder. Founders in their early 20s have the lowest likelihood of successful exit or creating a 1 in 1,000 top growth firm.

The rest of the paper is organized as follows. Section II details the newly-integrated administrative datasets that make this study possible. Section III presents our main results. Section IV presents extensions and discussion. Section V concludes.

2 Data and Measurement

Our study uses administrative data to identify the demographics of business founders in the U.S. and to track the performance of their businesses over time. Our primary datasets include administrative data from the U.S. Census Bureau's Longitudinal Business Database (LBD) and Schedule K-1 business owners data, while also integrating numerous other datasets. Detailed information about each data set is provided in the online appendix, with a summary displayed in Table A2. Below we describe how key measurement challenges can be overcome with the above databases, which enable us to analyze the demographics of business founders and track the performance of their firms over time.

2.1 Identifying New Firms

We rely on the LBD to identify startup firms. The LBD tracks both firms and their establishments over time. We follow Haltiwanger et al. (2013) and define a business's age as the age of the oldest establishment present at the first appearance of a new firm identifier. Startups are identified as de novo firms with no prior activity at any of its establishments. This approach ensures our definition of entrepreneurial firms does not include spinoffs from existing firms or new firms that are the result of the reorganization or recombination of existing businesses.[4] Note that the LBD identifies the startup year as the year when the business first hires an employee; as such the LBD startup date might differ from the legal founding date of a business. As a robustness check, we exclude businesses where the K-1 form founding date differs from the LBD age by more than two years. All results are consistent with the main findings from the full sample.

[4] We also drop age zero firms that have multiple establishments in their birth years. On average, their initial employment in year zero is unusually high relative to other new firms, suggesting that they are not de novo startups. Inspection of these startups suggest they are the result of multinational activity as well as newly created professional employer organizations.

2.2 Identifying Founders

Critical to our effort is the identification of founders. For S-corporations and partnerships, we use Form K-1 to define owners as individuals who own some portion of the firm at age zero in the LBD. We then use the W-2 data to define a founder as an owner who also works at the firm (as opposed to an investor who holds equity in the firm but does not work there). The identification of these "owner-workers" is, while traditionally very difficult in the U.S. data, straightforward in the linked administrative datasets we use.[5]

For C-corporations, we rely on two alternative approaches, as K-1 owner data is not available. For our primary analysis, we use the W-2 data to define the three highest paid workers in the first year of the firm's existence. This is the approach followed by Kerr and Kerr (2017), who argue that business owners are often among the top three initial earners in the firm.[6] Based on the S-corporation data, where ownership status can be determined with certainty, 90% of the owner-workers are in fact among the top three earners in the firm during the first year.[7] This "initial team" definition of founders can be applied to all firms. Secondarily, we will present results using the U.S. Census Annual Survey of Entrepreneurs (ASE), allowing us to look at a large subsample of C-corps for whom we can directly determine owner-workers.[8] In general, we have analyzed all of our results separately for S-Corporations (K-1 entities), partnerships (K-1 entities), and C-Corporations (non K-1 entities). Because the results are similar for each type, the main results emphasize the age findings pooled across all U.S. startups. In Section IV, we will demonstrate robustness across different ways of defining founders and different legal forms.

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[5] For about 20% of new S-corporations, none of the owners work at the firm, which we interpret as businesses where the equity holders are financing a new business and running it through hired management. These firms are not included in our analysis below; we will be considering these firms more closely in further work.

[6] Kerr and Kerr (2017) use LEHD data which currently excludes Massachusetts whereas we use more comprehensive W-2 earnings records. We have separately considered our analysis using LEHD records, including different definitions of founding team based on quarterly employment data, and find very similar results as in our W-2 sample.

[7] This approach is thus good at capturing owner-workers in the sense that few are missed. However, examining the S-Corporation data, the top three earners also typically include individuals who do not have ownership stakes in the firm. Thus this "initial team" definition of founders is best thought of as a related but distinct way of capturing the important individuals in the initial life of the firm, as opposed to an exact way of capturing owner-workers. We will consider distinctions between these approaches below.

[8] The Annual Survey of Entrepreneurs (ASE) is a representative survey of U.S. businesses with paid employees and receipts of $1,000 or more.

2.3 Identifying High-Growth Startups

We are especially interested in examining growth-oriented startups. We take two approaches. The first approach considers technology-orientation, which can suggest the potential for high growth. The second approach considers the actual outcome for the firm, based on the 3, 5, or 7 year time window after founding. We exclude from our analysis sole proprietors and businesses without employees.

Noting that there is no commonly accepted definition of "high tech" sectors or firms, we use three alternative definitions. First, following Hecker (2005), we define high tech sectors as industries (4-digit NAICS) with the highest share of technology-oriented workers according to the Bureau of Labor Statistics.[9] Second, we use a comprehensive match between the Census LBD and the businesses covered by the PCRI and VentureXpert databases to determine whether a given firm receives venture capital, suggesting that the firm is seen as having substantial growth potential. Third, we leverage prior research that matches the USPTO patent database with the LBD (Graham et al. forthcoming) to determine whether a firm has received a patent.

While the above measures attempt to delineate firms with substantial *potential* for growth, the LBD also allows us to quantify growth outcomes for each firm directly. Our primary outcome measures include (a) employment growth, and (b) sales growth, while we also consider (c) exit by acquisition and (d) initial public offerings. In the main text, we will emphasize employment growth, denoting a high-growth new venture as one that achieved a given threshold of employment 5 years after founding. We examine employment thresholds based on the Top 10, 5, 1, or 0.1 percentile. Analyses using sales growth are provided in the online appendix and show extremely similar results. Startups can grow and expand to become large multi-establishment corporations spanning multiple types of activities and locations. For these startups we calculate total firm employment by aggregating the establishment level records for each firm-year observation. From these firm-level measures it is straightforward to compute measures of employment growth by looking at the change in total employment over time.

[9] The list of Hecker (2005) includes 46 four-digit NAICS industries. An industry is considered high tech if the share of technology-oriented workers is at least twice the overall average of 4.9%. Defined by the Bureau of Labor Statistics, technology-oriented occupations are generally roles that require knowledge of science, engineering, mathematics, and/or technology typically acquired through specialized higher education.

Startups can also become targets for acquisition by existing firms. For example, the owner(s) of a successful venture might decide to exit by selling their idea and the assets embodied in their firm. In this case the original firm will cease to exist as such after the acquisition.[10] Some startups will simply fail and shutdown. We separately identify acquisitions of startups by existing firms as well as shutdowns and classify these events as distinct types of firm outcomes.[11] Lastly, we use the Compustat-Business Register Bridge to identify firms that enter public equity markets through an IPO. Our measure of "successful exit" below is an indicator for acquisition or IPO ever occurring within the scope of our databases.

3 Results

We now turn to the analysis of founder age in the universe of U.S. startups delineated above. Table 1 presents the results. Focusing on the first row and first column, which shows all new ventures in the U.S., we see that the mean age at founding is 41.9. This finding is broadly consistent with other population surveys of general types of new firms. Of course, while the word "startup" may conjure the image of technology entrepreneurs in their proverbial Silicon Valley garage, the great bulk of the new ventures that constitute our universe do not match this archetype. Though our data do not include sole proprietor businesses, it is still the case that most U.S. firms do not have the ambition and/or the business model to grow and scale their business (Hurst and Pugsley, 2011).[12]

To focus on growth-oriented entrepreneurs within our universe of U.S. startups, we take several approaches. Our first set of approaches examines the nature of the startup at founding, based on technology-related criteria. Our second set of approaches examines the growth performance of the startups themselves. Given the scale of the administrative data, we can further look at intersections of these criteria to focus on narrow subgroups of firms that both grow quickly and are in high-technology areas.

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[10] In the LBD these firms' establishments will take on the acquiring firms' identifiers.

[11] To distinguish successful acquisitions (i.e., those that generate positive returns for investors) from fire sale acquisitions, we drop observations for which total employment after the acquisition is lower than initial employment.

[12] While excluded from the analysis, our data show that the average age of new sole proprietors in 2010 was 44.8, significantly older than the rest of the population.

3.1 Ex-Ante Growth-Orientation

The results for different measures of growth-orientation are found in columns (2)-(4) of Table 1. We see that focusing on "high-tech" does not substantively affect mean founder age compared to the overall U.S. sample. Depending on the definition of high-technology, mean founder age now ranges from 41.9 to 44.6, with founders in high-tech sectors (43.2) and founders of patenting firms (44.6) appearing somewhat older on average than founders in the U.S. overall.

We can further partition the data geographically and consider California, Massachusetts, and New York separately given that these three states account for significant portions of high-growth startup activity in the U.S. (see Chen et al. 2010 with respect to VC-backed startups). In addition, we can examine regions with the most entrepreneurial activity at the zip code level. Using the Entrepreneurial Quality Index developed by Guzman and Stern (2017), we define entrepreneurial hubs as the 50 zip codes with the highest entrepreneurial quality. We also look specifically at Silicon Valley, considering all new ventures in the zip codes of Santa Clara and San Mateo counties.

Taking the overall population of new ventures (column 1), we see little variation with geography. Even when looking at the zip codes with the most growth-oriented new ventures, the mean founder age is 40.8, or approximately 1 year younger than the U.S. population average. One interpretation of this result may be that, even in entrepreneurial regions, most new firms are not in technology or growth-oriented sectors. However, reading across columns and rows in the table, we can further examine the intersection of geography with technology or growth-orientation. Remarkably, we see only modest differences in age. Mean founder ages rarely dip much below age 40, let alone ages 35, 30, or 25. The only category where the mean ages appear (modestly) below age 40 is when the firm has VC-backing. The youngest category is VC-backed firms in New York, where the mean founder age was 38.7. More generally, across the various narrow cuts in Table 2, the mean age ranges from 38.7 to 45.3. Put another way, even when reducing the set of 2.7 million founders to the 1,900 associated with firms that are both in entrepreneurial hubs and receive VC backing, the mean age at founding is 39.5. Meanwhile, founders in high-tech employment sectors tend to be slightly older than the U.S.-wide average, and founders of patenting firms are the oldest of all, with an average age of 44.3 in Silicon Valley and 43.8 in the entrepreneurial hubs.

3.2 Ex-Post High Performance Firms

It may still be that younger founders produce the highest performance new firms. Our second approach considers firm-level outcomes. The capacity to examine firm performance draws on the strengths of the LBD, which provides employees and sales for each firm, as well as indicating exit by acquisition and, via the Compustat Bridge, initial public offerings. A potential limitation in the intersection of our databases is that we have a limited time-period in which we can examine firm performance. Here we will focus on growth outcomes five years after the hiring of the first employee.[13]

To delineate "successful" entrepreneurs within the population of new ventures, we focus on the upper tail of the new ventures' employment growth. Specifically, we examine firms alternatively in the Top 10%, Top 5%, Top 1%, and Top 0.1% of growth. We complement these employment-based growth measures with a metric tracking whether these ventures ever exited by acquisition or IPO within our sample period.

Table 2 presents founder age across a range of upper-tail performance definitions. We see that more successful startups have, if anything, slightly older founders on average. For example, the 1,700 founders of the fastest growing new ventures (the top 0.1%) in our universe of U.S. firms had an average age at founding of 45.0 (compared to 43.7 for the top 1% and 42.1 for the top 5%). Regardless of the measure of technology-intensiveness chosen, we see older founders as we move toward upper-tail performance, especially for the top 1 in 100 or top 1 in 1,000 firms, as well as for founders with successful exits. This evidence is at odds with the conventional wisdom that successful founders skew younger.

3.3 Founder Age Distributions

One limitation of the foregoing results is that they only shed light on mean founder age. While mean age provides a standard summary statistic, and one that we can compare across technology-intensity, regions, and outcome measures, investigating the entire age distribution may reveal bands of age where founder activity is especially intense or founders are especially successful.

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[13] Using 3-year windows and 7-year windows shows broadly similar results.

Figure 1 presents the full founder age distributions, for the founders of all U.S. firms (blue line) and for Top 1% firms by employee growth after five years (red line).[14] Studying all founders, the age distribution is single peaked, with a relatively flat plateau at ages 37-43. Studying founders of high-growth firms, the founder age distribution shifts systematically to the right. Thus, the highest-growth new firms not only appear to come from those in middle-age, but also tend to come at even older ages than the background age distribution for founders would imply. Prior to the late 30s, the frequency of successful founders is well below the frequency of these founders in the population. Starting in the late 30s, and especially by the mid-to-late 40s, the frequency of successful founders is substantially greater than the frequency of these founders in the population. A similar peak in middle age appears when comparing the founder age distribution against the underlying workforce age distribution as opposed to the population as a whole (Figure A1).

3.4 The Likelihood of Success

Our previous results have demonstrated that growth-oriented start-up founders in the US economy tend to be middle-aged, not young. Thus, when asking where most high-growth or technology-intensive firms in the U.S. come from, the answer is "middle aged people." However, an equally important question is to ask how the probability of entrepreneurial success changes with founder age, conditional on starting a new firm. This statistic may be more informative for an individual considering founding a company or for investors deciding where to place their bets. For example, if two founders (of two distinct firms) come to pitch their idea to a venture capitalist, and all the venture capitalist knows is these founders' ages, which founder would be more likely to produce an upper-tail growth outcome?

To examine the relationship between the likelihood of success and age, we run linear probability models where an indicator for "success" is regressed on a full set of founder age fixed effects (age 20 and below is the omitted category). We graph each age coefficient and the associated 95% confidence interval in Figure 2.[15] Our success indicators are (a) exit by acquisition or IPO and (b) employment in the top 0.1% measured here at 5 years from founding.

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[14] Appendix Figure A4 presents analyses using upper tail sales growth instead of employment growth and shows similar results.

[15] The regressions calculate robust standard errors, clustered at the new venture level.

Figure 2A considers successful exits, which occurs for roughly 4,000 (or 0.15%) of the founders in our universe. We see that the relationship between age and successful exit is monotonically increasing up until about age 60 and declining slightly thereafter. A founder at age 50 is approximately twice as likely to experience a successful exit compared to a founder at age 30. Figure 2B replicates this analysis using Top 0.1% employment growth as the success metric. Here again, success probabilities are increasing with age, though the individual age coefficients are estimated less precisely. Similar to the exit results, a founder at age 50 is approximately twice as likely to achieve upper-tail employment growth compared to a founder at age 30.[16]

Overall, we see that younger founders appear strongly *disadvantaged* in their tendency to produce the highest-growth companies. That said, there is a hint of some interesting age thresholds and plateaus in the data. Below age 25, founders appear to do badly (or rather, do well extremely rarely), but there is a sharp increase in performance at age 25. Between ages 25 and 35, performance seems fairly flat. However, starting after age 35 we see increased success probabilities, now outpacing the 25-year-olds. Another large surge in performance comes at age 46 and is sustained toward age 60.

4 Extensions and Discussion

In this section, we provide secondary results and discussion to further characterize and help interpret the main findings of Section III.

4.1 Robustness across Sectors, Years, Legal Form, and Founding Team Definition

The data can be cut several additional ways to further establish robustness of the main results. First, we explore heterogeneity across industries. Table A3 documents some substantial differences across sectors in the mean age of founders. Yet there is no sector, including in computing, where the mean founder ages are below 38, and only 3 of the 315 NAICS-4 digit sectors show a mean founder age below 40. Second, we consider founder age by calendar year, in part to see if the findings are robust outside the Great Recession, which occurs in our sample period. Using ex-ante or ex-post growth orientation, we find similar age results looking at calendar years

[16] Results (not shown) controlling for industry are virtually unchanged.

individually from 2007-2014 (Table A4). Third, we disaggregate the results by legal form and across definitions of the founding team (Figure A2). We see that the highest-growth firms are started by individuals in middle age and beyond regardless of legal form or founding team definition.

4.2 Age Differences within Founding Teams

We further examine age variation within founding teams. To the extent that different members of a founding team play different roles, it is theoretically possible that the youngest members play outsized roles. Further, successful firms might feature founding teams with heterogeneous ages, possibly leveraging advantages of both youth and experience. However, looking at the *youngest* member of successful founding teams, a pre-middle-age tendency does not emerge (Table A5). For the Top 0.1% of new ventures, the youngest members center in the late 30s and early 40s (while the oldest members center in the late 40s and early 50s).

4.3 Entrepreneurial Outliers

Although we have looked at the Top 0.1% of firms and the rare outcome of successful acquisition or IPO, one might still wonder if even more extreme upper-tail outliers are the province of the very young. More precisely, several cases of extreme entrepreneurial success in the software and IT sectors have prominently featured very young founders (e.g., Steve Jobs, Bill Gates, and Mark Zuckerberg). One response to this observation is to balance the ledger by noting cases of extraordinary successes featuring older founders. For example, Herbert Boyer was age 40 when, based on his genetic engineering breakthroughs, he founded Genentech (which would eventually be acquired for $47 billion), and David Duffield was 64 when he founded Workday (which currently has a market capitalization of $43 billion).

At the same time, a subtler but perhaps more important response may lie among the greatest young founders themselves. Namely, the claim that young people are especially good at starting companies is a *within person* claim. That is, a given individual is thought to be "better" when s/he is young (e.g., when s/he may have greater energy, deductive abilities, originality, etc.). If so, then we would expect great young entrepreneurs to become "worse" when they age. At a cursory level, this seems doubtful. Elon Musk's Tesla and SpaceX seem no less visionary than his earlier ventures, Zip2 and X.com. Steve Jobs and Apple computer appeared to find their blockbuster innovation with the iPhone, introduced when Jobs was 52. Jeff Bezos and Amazon have moved far beyond selling books online. These examples suggest that these prominent founders themselves may not have peaked when very young.

To examine this idea quantitatively, we studied the forward 5-year stock price multiple as a function of founder age for each of Microsoft, Apple, Amazon, and Google.[17] This analysis allows us to examine whether the additional growth in market valuation tends to decline as these individuals age. We see no such tendency (Figure A3). In fact, the five-year multiples tend to rise toward middle age. The peaks come at age 48 (Steve Jobs), age 39 (Bill Gates), age 45 (Jeff Bezos), and age 36 (Sergei Brin and Larry Page).

Because many forces influence the stock prices of firms, interpreting these results requires substantial caution. With this important caveat in mind, however, the patterns may suggest a potential reconciliation between the existence of great young entrepreneurs and the advantages of middle age. Namely, extremely talented people may also be extremely talented when young. These individuals may succeed at very young ages, even when people (including these young successes) get better with age. Thus there is no fundamental tension between the existence of great young entrepreneurs and a general tendency for founders to reach their peak entrepreneurial potential later in life.

4.4 Industry Experience

Among successful entrepreneurs more broadly, we further consider the idea that capabilities may increase with experience by consulting prior employment histories. Using the LEHD to link 2.5 million founders to their prior work experience, we examine, for every founder, whether the individual has prior work experience in the specific sector of the start-up. Overall, the results (Table 3) indicate that founders with both closer and longer experience in the specific industrial sector of the start-up see substantially greater success rates. For achieving a 1 in 1,000 highest-growth firm, having no experience in the 2-digit level industry leads to a

[17] The stock price multiple is the ratio of the closing stock price five years in the future to the January 1st closing stock price in the current year. The stock price series are post IPO and account for dividends and splits. While Facebook would be a natural addition to this quartet of firms, the stock price series is too short as yet to allow such analysis.

success rate of 0.11%, while having at least three years of experience in the start-up's industry shows success rates rising to 0.22% (NAICS2 experience), 0.24% (NAICS4 experience), and 0.26% (NAICS6 experience). These findings are the opposite of stories that emphasize an outsider advantage for founders – which is a primary rationale underlying the broader belief that young people will produce the highest-growth firms.

4.5 Prior Wages

We can further incorporate individuals' prior W-2 wages into the decision to start new firms. Net of wage controls, we find that entry still peaks in middle age (Figure A5). At the same time, wages positively predict success. Individuals who start the highest-growth firms typically have very high prior wages (Figure A6), so that these individuals have outsized success both in the labor market and in founding firms. This finding is consistent with upper-tail founders having high skill; it is also consistent with the idea that high-growth founders set a high bar for entry into entrepreneurship, given a high opportunity cost of leaving the ordinary labor market behind.

4.6 Venture Capital Behavior

We also see that venture capitalists tend to bet on relatively young founders. Given that younger founders have substantially lower batting averages (e.g., see Figure 2), the founder-age tendency in VC investments may be surprising. VCs may thus be seen as making bad bets, which may be consistent with empirical findings suggesting that VCs have trouble predicting success and have earned low returns (Kaplan and Lerner 2010, Kerr et al. 2014). However, young founders may also be more in need of early-stage external finance, thus leading to this relationship. More subtly, and noting that VCs are seeking high returns, which is not identical to high growth, it may be that younger founders tend to sell their equity at lower prices, and thus VCs are making optimal return decisions. Teasing apart why VCs bet young is an interesting area for further work. We can say now however that venture capital, a major source of early-stage financing that can help drive creative destruction and economy-wide growth, does not currently appear allocated to the firms with the highest growth potential.

5 Conclusion

Researchers, policymakers, investors, and entrepreneurs themselves all strive to understand entrepreneurial traits that predict the creation of successful new firms. This paper has focused on founder age, which is often thought to be a key predictor of entrepreneurial success. We find that age indeed predicts success, and sharply, but in the opposite way that many propose. The highest success rates in entrepreneurship come from founders in middle age and beyond.

These findings are consistent with theories in which key entrepreneurial resources (such as human capital, financial capital, and social capital) accumulate with age. Mechanisms by which young people are proposed to have advantages (such as energy or originality) may still be operating, but if so they appear to be overwhelmed by other forces. Future work can explore how variation in specific founder traits predict entrepreneurial entry and success, further informing underlying theories for the life cycle of entrepreneurs and provide additional capacity to predict entrepreneurial success. More broadly, new administrative datasets linking founder traits and business outcomes promise to further reveal core facts about the high-growth new ventures that can drive economic growth and the advance of socioeconomic prosperity.

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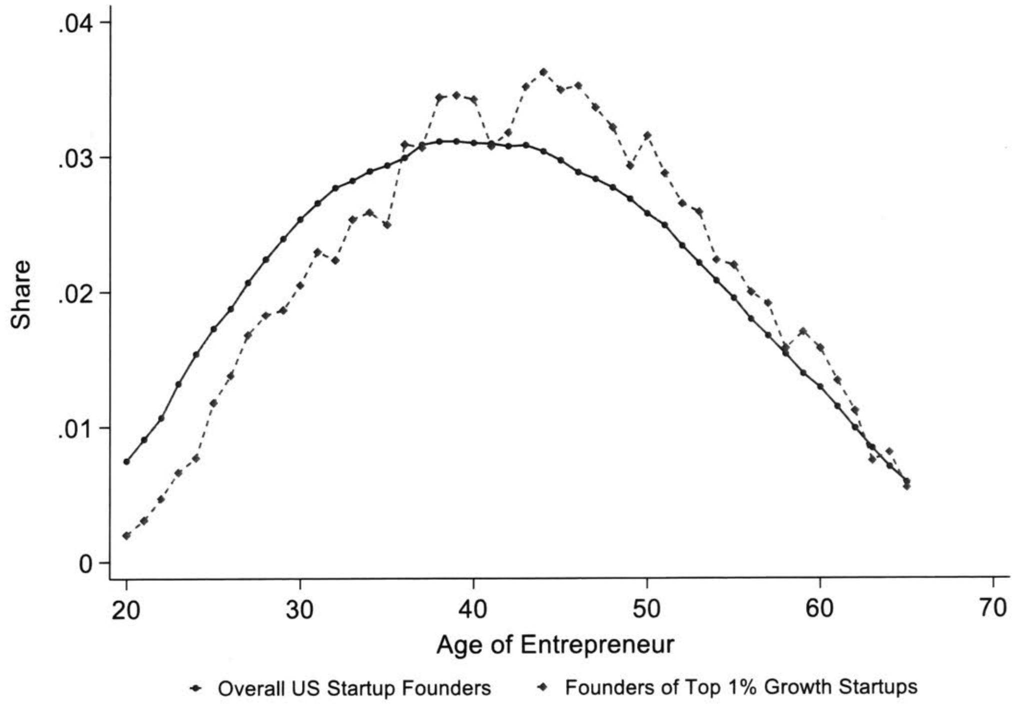
Figures and Tables

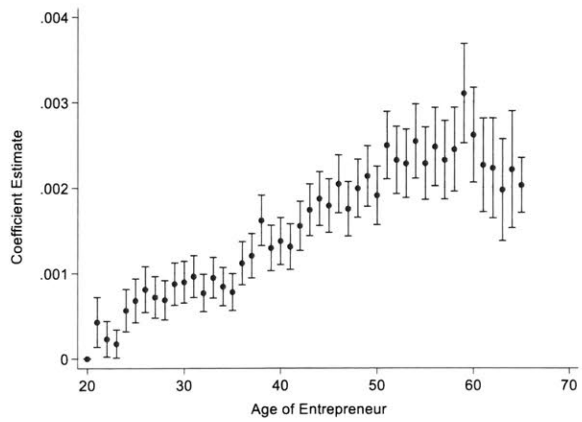
Figure 1: Founder Age Distribution: All Startups and High Growth Startups



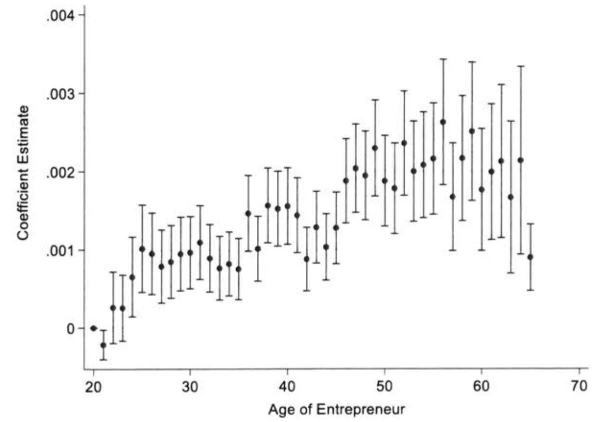
Source: Authors calculations based on W-2 earnings records, form K-1 and Longitudinal Business Database.
Notes: This set of kernel density plots shows the age distribution of startup founders (at year of founding) in the US. Each bin represents an age cohort. Ages between 20 and 65 are incorporated in the plots. The blue (left) plot incorporates all founders of new C-corporations, S-corporations, and Partnerships with employees founded between 2007 and 2014 as identified in the Longitudinal Business Database (LBD). The red (right) plot represents founders of the top 1% growth firms founded over the 2007-2009 period. The top 1% employment growth threshold value is calculated for each yearly cohort based on the raw employment figures from the LBD in the five years after the birth of the firm.

Figure 2: Likelihood of Extreme Success, Conditional on Starting a Firm

Fig. 2A: Probability of Successful Exit (IPO or acquisition), by Age



2B: Probability of Top 0.1% Employment at 5 Years, by Age



Source: Authors calculations based on W-2 earnings records, form K-1, Longitudinal Business Database and Compustat for firms founded over the 2007-2009 period.

Notes: OLS regression coefficients from estimating the likelihood of extreme firm success on a series of age indicators are shown. Ages 19 and below are grouped as 19 while ages 66 and above at grouped as 66. IPO data are sourced from Compustat. Acquisitions are based on firm ownership changes in the Longitudinal Business Database (LBD). Top 0.1% employment outcomes are calculated based on five-year employment growth in the LBD.

Table 1: Founder Age – Averages across U.S. and by Technology Definition

	All Startups	High Tech Employment	VC-Backed Firms	Patenting Firms
US (entire)	41.9	43.2	41.9	44.6
	(12)	(11.5)	(10.6)	(11.3)
	2,658,000	334,000	11,000	10,000
California	41.7	42.1	39.6	43.9
	(12)	(11.3)	(10)	(11)
	374,000	61,700	4,000	3,000
Massachusetts	41.7	43.2	42.3	45.3
	(11.8)	(11.2)	(9.8)	(10.6)
	52,000	8,100	900	400
New York	41.4	41.8	38.7	42.7
	(11.6)	(11.6)	(10.1)	(11.4)
	276,000	22,600	800	600
Silicon Valley	41.6	41.5	40.2	44.3
	(11.4)	(10.3)	(9.7)	(9.8)
	32,000	11,700	1,700	900
Entrepreneurial hubs	40.8	40.5	39.5	43.8
	(11.3)	(10.6)	(9.8)	(10.2)
	23,000	9,300	1,900	700

Notes: Mean founder age is shown in the first row, standard deviation in parentheses, and observation count in the third row. Data incorporates all C-corporations, S-corporations, and Partnerships founded over 2007-2014. Based on the Longitudinal Business Database (LBD), only new firms from each year are included. High tech sectors in column 2 are defined at the 4-digit NAICS level (see text). Column 3 represents firms that ever receive venture capital. Column 4 represents firms that are ever granted a patent, which is derived from the Longitudinal Linked Patent-Business Database. Silicon Valley is defined as zip codes in Santa Clara and San Mateo counties. Entrepreneurial hubs are defined as zip codes with the highest entrepreneurial quality as defined by Guzman and Stern (2017). Counts are rounded to comply with disclosure rules.

Table 2: Founder Age and Success — Upper Tail Growth or Acquisition

	All Startups	Top 10%	Top 5%	Top 1%	Top 0.1%	Successfully Exited Startups
US (entire)	41.8	41.6	42.1	43.7	45.0	46.7
	(11.9)	(11.5)	(11.5)	(11.1)	(10.7)	(10.6)
	1,079,000	126,000	62,000	13,000	1,700	4,000
Tech Employment	43.2	42.1	42.3	43.6	45.9	48.4
	(11.3)	(10.5)	(10.5)	(10)	(9.6)	(9.8)
	132,000	13,000	7,800	2,200	400	1,100
VC-Backed Firms	42.4	42.3	42.5	43.3	43.4	47.9
	(10.3)	(10.1)	(10.1)	(10)	(10.1)	(9.5)
	6,600	2,500	2,000	800	140	180
Patenting Firms	44.4	44.4	44.6	45.0	46.2	49.3
	(11.1)	(10.4)	(9.9)	(9.2)	(9.7)	(10.1)
	7,000	1,900	1,300	500	90	200

Notes: Mean founder age is shown in the first row, standard deviation in parentheses, and observation count in the third row. Data incorporates all C-corporations, S-corporations, and Partnerships founded over 2007-2009 in the Longitudinal Business Database (LBD), for which we can observe 5 years of performance data after founding. Only new firms from each year are included. Employment growth is measured using the 5-year window. Tech Employment consists of NAICS-4 sectors with high shares of STEM-trained workers. Counts are rounded to comply with disclosure rules.

Table 3: Industry-Specific Experience and Growth Outcomes

Panel A: Founders with Work Experience in Startup's 2-Digit Industry Classification

	Top 10%	Top 5%	Top 1%	Top 0.1%	Successful Exit
NAICS-2 Experience					
Never	8.6%	4.1%	0.9%	0.11%	0.13%
1-2 years	10.1%	4.8%	1.0%	0.11%	0.10%
>= 3 years	15.0%	7.7%	1.7%	0.22%	0.20%

Panel B: Founders with Work Experience in Startup's 4-Digit Industry Classification

	Top 10%	Top 5%	Top 1%	Top 0.1%	Successful Exit
NAICS-4 Experience					
Never	9.1%	4.5%	1.0%	0.12%	0.14%
1-2 years	11.6%	5.6%	1.1%	0.14%	0.12%
>= 3 years	16.8%	8.5%	1.7%	0.24%	0.20%

Panel C: Founders with Work Experience in Startup's 6-Digit Industry Classification

	Top 10%	Top 5%	Top 1%	Top 0.1%	Successful Exit
NAICS-6 Experience					
Never	9.4%	4.6%	1.0%	0.12%	0.13%
1-2 years	12.6%	6.0%	1.2%	0.15%	0.13%
>= 3 years	17.7%	9.0%	1.8%	0.26%	0.21%

Notes: Data incorporates all C-corporations, S-corporations, and Partnerships founded over 2007-2009 in the Longitudinal Business Database (LBD), for which we can observe 5 years of performance data after founding. Growth outcomes are determined by employment growth, using the 5-year window after founding.

Chapter 3

Is There a Startup Wage Premium? Evidence from MIT Graduates

Abstract: While startups are the center of extensive policy discussion given their outsized role in job creation, it is not clear whether they create high quality jobs relative to incumbent firms. This paper investigates the wage differential between venture capital-backed startups and established firms, given that the two firm types compete for talent. Using data on MIT graduates, I find that non-founder employees at VC-backed startups earn roughly 10% higher wages than their counterparts at established firms. To account for unobserved heterogeneity across workers, I exploit the fact that many MIT graduates receive multiple job offers. I find that wage differentials are statistically insignificant from zero when individual fixed effects are included. This implies that much of the startup wage premium in the cross-section can be attributed to selection, and that VC-backed startups pay competitive wages for talent. To unpack the selection mechanism, I show that individual preferences for risk as well as challenging work strongly predict entry into VC-backed startups.

1 Introduction

Politicians and pundits routinely tout that startups are the engine of job creation in the US economy. True to popular belief, young businesses account for roughly 70% of gross job creation in the US (Haltiwanger et al., 2012). While startup companies play a vital role in creating jobs, it is not clear whether startups — relative to established firms — create high quality jobs. In light of the fact that startups employ a disproportionately high share of young workers (Ouimet and Zarutskie, 2014), a central question remains: do startups or large established firms create better paying jobs for young workers?

Although prior studies extensively document that large established firms generally pay higher wages than their smaller (Brown and Medoff, 1989; Oi and Idson, 1999) and younger counterparts (Davis and Haltiwanger, 1991; Brown and Medoff, 2003; Haltiwanger et al., 2012), the existing set of evidence is difficult to interpret for two reasons. First, the potential sorting of workers across employers limits the interpretation of cross-sectional wage comparisons. For instance, if large firms possess superior managerial talent as shown in the (Lucas, 1978) span of control theory, then high-ability workers may sort into large firms and thus command higher wages. Exploiting the fact that many graduates from Massachusetts Institute of Technology (MIT) receive multiple job offers, this study seeks to uncover the counterfactual wages that the first set of non-founder employees at startups ("early employees") would have earned if these young workers had instead joined large established companies.

Second, prior studies do not clearly distinguish high-growth startups from small businesses. While many policymakers broadly use the term entrepreneurship to refer to all new enterprises, small businesses and high-growth startups are fundamentally different types of firms (Schoar, 2010). High-growth startups are a small subset of new firms that grow rapidly and account for a disproportionately high share of wealth and job creation (Shane, 2009; Decker et al., 2014). In contrast, most small businesses (e.g. local restaurants) tend to remain small because they typically do not intend to grow large or innovate in a meaningful way (Hurst and Pugsley, 2011). Given their distinct growth intentions, high-growth startups — unlike small businesses — compete against incumbent firms for talent. Therefore, a suitable setting to compare wages between startups and established firms is one in which workers who join startups are much more likely to do so in the high-growth rather than the small business sector.

MIT is a particularly appropriate setting to study the allocation of top technical talent between high-growth startups and established corporations. While MIT selectively draws highly talented individuals that may not represent the average worker, the right tail of the talent distribution is precisely where the rich interplay between high-growth startups and established firms can be studied. This is because entrepreneurial growth is itself an extremely skewed outcome; a very small fraction of startups at the right tail of the quality distribution are responsible for much of the job creation and impactful innovation (Guzman and Stern, 2016). To quantify the skewness, Puri and Zarutskie (2012) estimate that only 0.10% of the US firms born between 1981 and 2005 ever receive venture capital financing. Given that a large portion of MIT graduates are prolific inventors, entrepreneurs, and early employees of high-growth ventures, MIT graduates are much more likely to select into both established firms and high-growth startups — rather than small businesses — where their skills are directly used.

This paper explores the wage differential between venture capital-financed startups and large established firms, and the role of selection as the channel through which these differences persist. Using data on graduating college students from MIT, I find that VC-backed startups on average pay 8% to 13% higher wages than their more established counterparts holding all observable individual-level covariates constant. Given that VC-backed firms are — by construction — young and small, this finding stands in contrast to the literature's well-documented wage premium associated with large and old firms. However, the observed startup wage premium for MIT graduates is consistent with the recent evidence that the relationship between firm age and wages becomes negative when controlling for employee age (Ouimet and Zarutskie, 2014) or focusing on rapidly growing startups (Sorenson et al., 2016). Nonetheless, relatively high wages associated with VC-backed startups are robust across several regression specifications. Given that venture capital investors typically concentrate their deals in a few select industries, I restrict the sample to the high-tech sector and find that the startup wage premium remains statistically significant albeit slightly attenuated in magnitude.

Next, I test for selection as the source of wage differentials between startups and established firms. Even with a rich set of control variables, cross-sectional wage comparisons can be biased due to selection based on unobservable characteristics such as ability. The two groups of workers appear to be systematically different along several observable dimensions, suggesting that there may also be unobserved differences that lead to non-random sorting of workers. For instance, early employees receive more job offers and less strongly prefer job security and firm reputation relative to workers at established firms. To account for unobserved heterogeneity across workers, I focus on MIT graduates who receive multiple job offers from both firm types. Originally employed by Stern (2004), this identification strategy allows for within-person comparison of wages.

Based on empirical specifications that use individual fixed effects, I find that the effect of startup employment on wages becomes negative and statistically indistinguishable from zero. At a minimum, these results reject the large, positive wage premium associated with entrepreneurial employment in the cross-section. More broadly, these findings suggest a positive selection of high-ability workers into startups; counterfactually, they would also command relatively high wages at established firms. Overall, much of the startup wage premium can be attributed to selection. This result highlights the substantial role that endogenous sorting of heterogeneous workers plays in determining key labor market outcomes such as wages. In addition, though they face more credit constraints than large firms, VC-backed startups appear to pay competitive wages for talent.

Empirical exploration of the dynamics of high-growth startups vis-à-vis established firms is important to both policymakers and researchers for several reasons. First, in terms of startup entry, the allocation of productive workers has significant implications for economic growth (Baumol, 1990; Murphy et al., 1991; Philippon, 2010). Given the recent surge in venture capital activity, hiring at venture capital-backed firms has risen.[18] As a result, talented young workers have increasingly joined early-stage companies financed by venture capital. For instance, the share of MIT graduates joining VC-backed startups rapidly grew from less than 2% to 14% between 2006 and 2014. In tandem with this rise, the portion joining the financial sector sharply fell from 30% to 5% in the same period. If workers' career paths are endogenous to the set of sector-specific skills and social ties developed during initial employment (Gompers et al., 2005; Elfenbein et al., 2010; Campbell, 2013), then this phenomenon has larger implications for the future supply of innovators and entrepreneurs.

[18] Venture Capital Activity at 13-Year High" Ernst & Young Global Limited. 5 February 2015 <http://www.ey.com/GL/en/Newsroom/News-releases/News-EY-venture-capital-activity-at-13-yearhigh>

Second, from a policy perspective, it is important to understand whether startups create high-paying jobs relative to those in other sectors of the economy. There are numerous policy efforts aimed to encourage entrepreneurship typically through tax breaks and funding (e.g. SBA loans). Burgeoning evidence shows that tax breaks and financing aid are effective levers in enhancing entrepreneurial activity (Gentry and Hubbard, 2000; Howell, 2017). However, Shane (2009) argues that simply encouraging more entrepreneurship is a flawed policy approach because the vast majority of new firms generate little economic impact. For instance, it is not clear whether the new jobs stemming from policy-induced entrepreneurial entries are low quality jobs. Since wages are a key indicator of job quality, wage determination between startups and established firms is an insightful empirical analysis.

Third, scholars in the fields of labor economics and entrepreneurship have not sufficiently unpacked the importance and the role of early employees. While founders are undoubtedly important, high-skilled employees play a critical role in the growth and success of nascent firms. Attracting and retaining high quality workers is a challenge for early-stage companies because they compete against established firms for talent. Yet, very little is known regarding the first set of non-founder employees that join startup companies (Stuart and Sorenson, 2005; Roach and Sauermann, 2015). Therefore, the lack of empirical and theoretical attention on early employees leaves the human capital piece of entrepreneurship under-explored. This study offers one of the first set of empirical evidence on the characteristics of high-skilled young workers who join VC-backed startups and the wages that they earn relative to their counterfactual wages at established companies.

The remainder of this paper is structured as follows: Section II reviews the relevant prior literature and the conceptual framework. Section III explains the identification strategy exploiting multiple job offers and the empirical setting. Section IV describes the data and systematic differences between early employees and workers who join established firms. It also presents the results on the startup wage differential with and without accounting for selection effects. Finally, Section V concludes with this study's main insights, limitations, and implications for future research.

2 Literature Review and Conceptual Framework

2.1 Existing Evidence

In theory, should startup salaries be meaningfully different from those at large established companies? If so, what is the equilibrium wage that a startup must pay in order to induce a worker into the young company who would otherwise sort into an established firm? As a useful starting point, the literature on the returns to entrepreneurship may offer relevant insights because in a sense, early employees are an extension of the founding team. Unfortunately, the financial returns to entrepreneurship appear to be a puzzle. While many studies show that entrepreneurs earn less than their salaried counterparts (Borjas and Bronars, 1989; Evans and Leighton, 1989; Hamilton, 2000; Hall and Woodward, 2010), more recent studies argue that the pecuniary returns to entrepreneurship are relatively high (Levine and Rubinstein, 2017; Kartashova, 2014; Sarada, 2014; Manso, 2016).

Results are seemingly inconsistent largely due to the broad definition of entrepreneurship. While many scholars and policy-makers generalize all small or young firms as startups, entrepreneurial firms are extremely heterogeneous in their growth outcomes (Decker et al., 2014). Broadly, there are two types of entrepreneurship that fundamentally differ in their economic intentions, skill composition, and rates of job creation (Schoar, 2010). On the one hand, small businesses typically do not intend to grow large or innovate in a meaningful way (Hurst and Pugsley, 2011). As a result, Hurst and Pugsley (2011) document that more than 85% of mature US firms (in operation for at least ten years) remain small. On the other hand, high-growth startups aim to grow large and thus make strategic decisions — such as incorporating in Delaware or applying for a patent — that are related to substantial growth outcomes (Guzman and Stern, 2016).

Naturally, the two types of entrepreneurship also exhibit different wage patterns. Studies that conflate small business owners and high-growth entrepreneurs generally find a wage penalty for entrepreneurs relative to employees of large firms. However, when selecting on entrepreneurial firms that intend to expand, Levine and Rubinstein (2017) find that entrepreneurs earn higher hourly wages than their salaried counterparts. Therefore, the results on entrepreneurial earnings are muddled by the inconsistent measurement of entrepreneurship, lending unclear guidance to the wage comparison between early employees at high growth ventures and workers at established firms.

Furthermore, the literature on the financial returns to entrepreneurship may be inapplicable to the wage differences between high-growth startups and established firms because joiners are considerably different from founders. In many ways, early employees resemble salaried workers in large firms (Chen, 2013; Roach and Sauermann, 2015). The main similarity is that early employees are hired workers who receive competitive salaries. In contrast, compared to joiners, founders of VC-backed startups typically take on lower cash compensation and greater equity ownership (Wasserman, 2006; Bengtsson and Hand, 2013). As a result, joiners and founders experience substantially different economic incentives and rewards. Therefore, the literature on the returns to entrepreneurship appears to bear little pertinence to the wages that startup joiners earn.

Another relevant set of insights comes from the rich literature in labor economics around wage differentials across firms. In particular, employer size and age appear to be salient drivers of a persistent gap in earnings. Extensive evidence documents that large firms tend to pay higher wages than their smaller counterparts (Brown and Medoff, 1989; Oi and Idson, 1999). Similarly, old firms generally pay higher wages relative to young firms (Davis and Haltiwanger, 1991; Brown and Medoff, 2003; Haltiwanger et al., 2012). Since high-growth startups are both young and small, the existing evidence appears to lend support to the hypothesis that startups pay lower wages compared to large established firms. However, the positive firm age-wage relationship becomes questionable after accounting for worker characteristics (Brown and Medoff, 2003), raising the concern for selection bias.

The literature on wage differentials by firm size and age does not adequately address the potential sorting of heterogeneous workers. Workers may endogenously sort into startups or established firms based on unobservable worker characteristics that are also related to wages. For instance, prior studies provide evidence of non-random sorting of workers between incumbent and new firms (Nystrom and Elvung, 2015), as well as between academic spin-offs and other technology-based startups (Dorner et al., 2017). Simple wage comparisons would be biased if workers who join established companies are systematically different from early employees at startups.

Prior literature show that early employees are intrinsically different from established firm employees along several important observable characteristics. With respect to age, Ouimet and

Zarutskie (2014) document that young firms tend to hire younger workers. The authors also show evidence suggesting that, relative to young workers at older firms, young workers at young firms are more risk tolerant and technically skilled. In addition, Sauermann (2017) finds that academic scientists who join small firms place a lower value on job security but prioritize independence and challenging work. Therefore, the two groups of workers appear to be different not only in their demographic characteristics, but also in their technical capacity and individual preferences.

It is also likely that the two groups are dissimilar along unobservable dimensions. In early empirical examination of compensating differentials, Brown (1980) contends that cross-sectional evidence of wage differentials does not necessarily substantiate the theory because several key variables are omitted — most importantly, worker ability. Omission of worker ability is problematic because ability is typically positively correlated with the individual's earnings capacity. In addition, ability may be related to the worker's entry into startups. For instance, Dahl and Klepper (2015) theorize that high quality workers are matched to large — presumably more productive — firms, leaving low quality workers to be matched to new firms. Potential sorting of workers between entrepreneurial firms and established companies weakens the interpretation of the widely documented wage penalty associated with small and young firms.

2.2 Wage Differentials

As a starting point, the well-documented employer-age wage premium informs the basic relationship between VC-financed startups and wages which can be organized into a simple econometric framework with worker *i*, firm *j*, and a vector of individual-level traits X_i:

$$\log(WAGES_{ij}) = \beta_0 + \beta_1 STARTUP_j + \boldsymbol{X}_i{}'\Theta + \varepsilon_{ij} \tag{1}$$

Equation (1) is a cross-sectional relationship between startup employment and wages in which the unit of observation is the individual. Only the accepted job offer is observed for each individual. Previous literature provides a prior on the magnitude and direction of β_1. In particular, Haltiwanger et al. (2012) compute the real monthly earnings of US workers at both new and established firms.[19] The authors show that, in 2011, workers at young firms earned

[19] New firms are defined to be younger than two years old while established older than ten years old.

roughly 70% as much as their counterparts at mature firms. Therefore, prior evidence from the literature estimates β_1 at roughly -0.30. Since VC-backed startups are — by construction — young, the existing prior on the negative relationship between firm age and wages leads to the first hypothesis: VC-backed startups on average pay lower wages than do established companies.

2.3 Selection Bias

Selection may explain the wage gap between entrepreneurial and established firms. As discussed, simple wage comparisons would be biased if workers who join established companies are systematically different from early employees at startups. Selection bias can be eliminated through conditional independence if such differences across workers are perfectly observable to the econometrician and thus included in the conditional expectation function (Angrist and Pischke, 2009). In this case, observable differences between startup joiners and established firm employees — such as worker age — can be included as control variables.

However, the key omitted variable in the wage comparison is worker ability. Omission of ability is problematic because it is typically positively correlated with the individual's earnings capacity. At the same time, worker quality may be associated with firm maturity (Dahl and Klepper, 2015). A possible explanation for the positive assortative matching is that since larger firms have better managerial talent and a greater span of control (Lucas, 1978), high quality workers are matched to large firms. The relationship between wages and startups conditional on worker ability is the following:

$$\log(WAGES_{ij}) = \pi_1 STARTUP_j + \pi_2 ABILITY_i + \boldsymbol{X}_i{}'\Theta + \eta_{ij} \tag{2}$$

The model in Dahl and Klepper (2015) predicts that startups are matched to lower quality workers, who generally command lower wages. In this case, β_1 in (1) would be downward biased because ability is negatively correlated with startups while positively linked to wages. Sorting of low quality workers into new firms would then be the mechanism through which startups appear to pay lower wages than established firms. In such scenario, entrepreneurial employment is expected to be unrelated to wages after accounting for individual ability. This leads to the second hypothesis: Holding worker ability constant, VC-backed startups and established firms pay statistically equal wages.

3 Methodology and Data

3.1 Identification Strategy

The true startup-wage relationship in (2) cannot be directly tested because $ABILITY_i$ is unobserved. In order to estimate the startup-wage relationship while accounting for selection, I exploit bundles of job offers — both accepted and rejected — that MIT graduates receive before entering the labor market. This framework allows for the comparison of wage offers across firms while holding the individual constant. Since multiple price points are observed for the same labor service, the demand curve for startup employment can be traced out while holding the supply curve fixed (Hsu, 2004). As a result, the effect of startup employment on wages can be cleanly identified. Econometrically, individual fixed effects are employed to essentially difference out the unobservable individual-level factors that may be systematically correlated with wages:

$$\log(WAGES_{ij}) = \beta_0 + \beta_1 STARTUP_j + \delta_i + \boldsymbol{X}_i{}'\Theta + \varepsilon_{ij} \tag{3}$$

Contrary to the previous empirical relationship, the unit of observation in Equation (3) is the job offer such that the individual is separately observed for each of her job offer. As a result, individual fixed effects account for the effects of unobserved factors that are individual-specific but fixed over time — most notably, worker ability or attractiveness to employers. The β_1 in Equation (3) is the estimated effect of entrepreneurial employment on wages. If the second hypothesis is true, meaning that VC-backed startups and established firms pay similar wages conditional on worker ability, then β_1 will be statistically insignificant from zero.

A key identification assumption behind the multiple job offers methodology is that those who receive one job offer are not fundamentally different from workers with multiple offers. This methodology requires narrowing the sample to only the individuals with multiple offers in order to employ individual fixed effects. Selection issues may weaken the internal validity of the following analysis if, for instance, multiple offers are systematically drawn from a different part of the worker ability distribution. It is possible that workers with higher ability attain more job offers because they are presumably more attractive to employers. However, many top MIT graduates have a single job offer because they receive and accept a full-time job offer from their summer internship prior to their senior year and thus do not participate in the ensuing full-time job recruiting. I revisit this assumption in Section IV by testing for differences in observable individual traits between the two groups.

3.2 Empirical Setting

MIT serves as the empirical setting in which I study wage differentials between VC-backed startups and established firms. Although MIT is a highly selected sample of talented workers and therefore may not be representative of the broader labor market, it serves as a favorable setting for three reasons.

First, as noted earlier, MIT is a major technology-based university whose alumni include productive inventors responsible for nearly 25,000 patents (Shu, 2012) as well as entrepreneurs estimated to have founded more than 30,000 actively operating companies as of 2015 (Roberts et al., 2015). Given the roots of a research university, MIT alumni-founded companies are largely technology-based (Hsu et al., 2007). Such active participation in innovative activities among MIT graduates is important for this study because there are fundamental differences between high-growth ventures and small businesses (Schoar, 2010; Levine and Rubinstein, 2017); the latter type of entrepreneurship does not provide an appropriate basis for wage comparisons since small businesses do not directly compete against established firms for talent. Since MIT attracts highly skilled individuals, its graduates are much more likely to select into high-growth startups and established companies rather than small businesses.

Second, a significant portion of graduating students from MIT receive job offers from both established firms and high-growth startups, generating rich variation in the comparable job offers that these graduates receive. While roughly 550 of the 1,100 graduating class seek full-time employment in a typical year, more than 400 companies actively recruit at MIT.[20] As a result, the average student on the job market receives two competing job offers. This is an important feature not only for the interpretation of the wage differential, but also for the multiple offers methodology's identifying assumption that some workers receive offers from both VC-

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[20] Data from the MIT Global Education and Career Development Office show that, between 2006 and 2014, approximately 50% of MIT undergraduates enter into full-time employ upon graduation, 40% into graduate school, and 10% into other plans including fellowships, continuing education, traveling, volunteering, and part-time work.

backed startups and established firms. This study's empirical strategy rests on the fact that the average MIT undergraduate on the job market receives two competing offers.

Third, while job offers from startups are relatively rare and often difficult to observe, many MIT graduates join early-stage firms whose salary offers are observable. In fact, the portion of MIT graduates joining startups as non-founder employees has substantially increased especially following the financial crisis in 2008. In 2014, roughly 14% of the graduating class chose employment at VC-backed startups compared to less than 2% in 2006 (see Figure 1). Interestingly, the share of MIT graduates joining the financial sector fell from 30% to 5% during the same period (see Figure 2). Thus, MIT provides a setting to study and compare offers from entrepreneurial companies and established firms distributed among a pool of highly talented labor market entrants.

3.3 Data

The data come from the two following surveys on full-time recruiting outcomes for graduating college students at MIT: (1) Graduating Student Survey and (2) MIT Early Careers Survey. The Graduating Student Survey, which is annually administered by MIT Career Services, collects information regarding each student's post-graduation plans, job offers that the individual receives, and motivations for accepting a particular offer. The survey data coverage extends from 2006 to 2014 with response rates consistently around 80% and includes 18,789 total respondents from undergraduate, and master's, and doctoral programs.[21] The sample is reduced to undergraduate seniors who indicate plans to be employed full-time during the year following graduation; immediately following graduation, approximately half of MIT college graduates enter graduate school. Furthermore, those entering into non-private sector employment are removed from the sample. The final sample includes 2,064 individuals. Table 1 shows the summary statistics.

In addition, the MIT Early Careers Survey, launched in 2014, is an online follow-up survey of recent MIT alumni and the set of offers they received upon graduation. Respondents were asked to provide information on various job characteristics (e.g. salary, title, industry) and

[21] When this study was initially launched, the MIT Graduating Student Survey covered from 2006 to 2014. Summarized results from future waves of this survey are available here: https://gecd.mit.edu/resources/survey-data

motives for choosing the accepted offer. Respondents with job offers from startups were additionally asked about stock options (e.g. number and percentage of shares, then-current company valuation, vesting schedule). Since the survey was motivated by the initial results from the Graduating Student Survey, it was designed to cover the exact same time frame and population (i.e. college graduates who select into full-time employment). Given the administrative concern that MIT graduates are too frequently solicited to fill out surveys, the MIT Early Careers Survey's outreach was limited to 2,500 people. Consequently, the random sampling of 2,500 potential respondents was slightly weighted towards (1) the Engineering school and (2) graduation years closer to the implementation year to reduce recall bias. The final sample contains 1,014 private sector job offers among 626 individuals.

The MIT Graduating Student Survey measures compensation from the three following variables: (1) yearly salary in US dollars; (2) sign-on bonus; and (3) additional compensation (e.g. allowance for moving expenses). All of the analyses in this study are based on the first component, the yearly salary, as the main dependent variable. Nonetheless, as shown in Appendix Tables A3 and A4, the main results are consistent with using the total compensation package. Ex-post compensation (e.g. performance bonus) are not observed because individuals are surveyed before they begin their jobs.

Moreover, equity compensation is not included in this study. Although the MIT Early Careers Survey collects some information regarding stock options, the data are difficult to interpret. The real value of a share in an early-stage company is almost impossible to assess ex-ante considering the uncertainty around the company's underlying idea or business model (Kerr et al., 2014); even with information on the most recent company valuation, the actual value of the employee's shares is not realized until the company eventually exits via an acquisition or initial public offering. Therefore, it is not clear how the job candidates perceive and value the proposed stock options at young private firms during the time of the job offer. Due to issues around both measurement and interpretation, equity compensation is not captured in this study.[22]

[22] Typically, college graduates entering into entry-level positions are not offered significant stock options at large established companies. In contrast, VC-backed startups typically offer equity to their early employees to attract talent without offering more cash (Booth, 2006). Given the assumption that VC-backed startups tend to pay equity more frequently than large established companies, it is likely that the startup wages estimated in this study are downward biased since equity compensation is omitted in the analysis.

A potential concern for the MIT Early Careers Survey is the non-response bias. The MIT Early Careers Survey has a response rate of 25%. The low response rate is problematic if the 25% who responded to the survey are qualitatively different from those who did not. In this case, the multiple offers analysis based on this survey data may not be generalizable to the full labor market of MIT graduates. For instance, MIT alumni with "less successful" early careers may be less inclined to participate in the survey which would upward bias the observed earnings distribution.

Fortunately, non-response bias can be rigorously assessed since MIT contains administrative data on both the survey respondents and non-respondents. Table A1 in the Appendix shows difference in means tests of observable individual characteristics between respondents and non-respondents. By design, the respondents are more likely to be from the Engineering school and recent graduation years relative to the non-respondents. Consistent with the sectoral trends in Figure 2, and given their more recent graduation years, respondents are much more likely to have chosen jobs in the high-tech sector (e.g. software) and less so in the financial services sector. Therefore, these industry differences are rather expected and are controlled for in the inclusion of year fixed effects. Overall, the two groups appear to be similar in their individual traits (e.g. gender, number of offers received, citizenship status). More importantly, respondents and non-respondents are similar in terms of their job outcomes (e.g. number of offers, accepted salary), suggesting that non-response bias is not a credit alternative explanation to this study's results on wages. As a result, the interpretation of the results from the MIT Early Careers Survey does not seem to be threatened by non-response bias.

Firms are categorized as one of the three following types based on firm age and venture capital financing: Established Firm, VC-Backed Startup, or Non-VC-backed Startup. Firm categorization is based on firm age – not size – in light of the fact that young firms play a salient role in job creation (Haltiwanger et al., 2013). Mechanically, I define a VC-backed startup as any for-profit company that receives early-stage institutional capital — either venture capital or angel financing — within five years of the employee's join date. All results are robust to narrowing the venture capital financing window to three years. Venture-backed companies that successfully exit via an IPO or M&A before the student's graduation year are categorized as established firms. Moreover, non-VC startups are companies that are five years old or younger and that do not receive VC-financing prior to the student's join date. Lastly, established firms are companies that are older than five years old and do not receive venture capital financing within the narrow window prior preceding the worker's graduation year.

It is important to discuss why venture capital financing is salient to this study's categorization of firm types. While many studies in the entrepreneurship literature generalize all small or young firms as startups, many small businesses are not viable employment alternatives to large established corporations. Most small businesses never intend to grow large or innovate in a meaningful way (Hurst and Pugsley, 2011), implying that they do not typically recruit for the type of human capital that large corporations seek. Since firm intentions are unobservable, VC financing is used to distinguish lifestyle businesses from young high-growth firms, which presumably compete against established firms for talent.[23]

In addition, VC financing is relevant to high-growth entrepreneurship because venture investors commonly professionalize their portfolio companies by implementing formal human resource policies (Hellmann and Puri, 2002). This allows the nascent companies to appropriately compensate their employees. Also, venture capital financing enables early-stage companies to attract new talent as evidenced by the hiring spree that typically follows each additional round of venture financing (Davila et al., 2003). Therefore, VC activity forms an important dimension to how firms are categorized in this study.

4 Empirical Results

4.1 Simple Wage Comparisons

This section examines the cross-sectional relationship between offered salaries and startup employment for MIT graduates from 2006 to 2014. The analysis is at the individual-level and wages are those of the accepted job offer. The following regression specifications in Table 2

[23] Aulet and Murray (2013) similarly categorize young firms into two distinct types: small and medium-sized enterprises (SMEs) and innovation-driven enterprises. They explain that firms in the latter category are typically supported by external financing because they require investment capital in order to develop novel products and scale their businesses.

closely follow Equation (1). All specifications include graduation year fixed effects to account for idiosyncratic time trends in the labor market.

In the simple case shown in Specification (2-1), the association between VC-backed startup employment and log wages is positive and statistically significant at the 1% level. The economic significance is also large; relative to established firms, VC-backed startups on average pay 13% higher salaries. This is a surprising finding because while VC-backed startups are typically young and small, the labor economics literature widely supports firm size- and age-wage premium as an empirical regularity. This suggests that, contrary to the general population, workers selected from the right tail of the human capital distribution experience a fundamentally different dynamic between firm age and wages when choosing among job offers from startups and established firms in the US.

Specifications (2-2), (2-3), and (2-4) control for individual characteristics that are potentially linked to the worker's earnings capacity. These characteristics include gender, US citizenship, number of offers received, and the MIT school in which the graduate was academically trained. The estimated effect of startup employment on wages is attenuated after accounting for individual traits related to earnings. This is consistent with Brown and Medoff (2003) who find that the empirical relationship between firm age and wage is highly sensitive to controlling for worker characteristics. More importantly, controlling for the number of offers received in Specification (2-4) noticeably attenuates the wage premium attributed to VC-backed startups. Given that the number of job offers can be a proxy for the individual's unobserved ability, it is reasonable that the estimated wages shrink after indirectly accounting for ability. Overall, even after controlling for worker characteristics that are related to wages, specifications (2)-(4) indicate a robust effect of a VC-backed startup wage premium. Therefore, I reject Hypothesis 1 at the 1% statistical significance level and find that VC-backed startups on average pay 8-13% higher wages than their mature counterparts.

It is worth noting that non-VC startups generally pay lower wages than both established firms and VC-backed startups. While not always statistically significant, non-VC startups are generally associated with a 10% wage discount relative to established firms. In light of the wage premium consistently linked to VC-backed startups, these results corroborate the fundamental role that venture capital plays in financially enabling young firms to offer attractive compensation.

A key concern is that the observed wage differential may be driven by firm location. Given the venture-backed startups tend to be clustered in entrepreneurial regions (e.g. California, Massachusetts, and New York) that are also expensive, these firms may pay relatively high wages to simply offset the high cost of living. In other words, geographic differences are a plausible alternative explanation to the main result shown above.

Accordingly, Specification (2-5) tests whether and how the estimated wage differential changes after including location (state) fixed effects. For job offers outside the US, the locations are grouped by the continent. For example, jobs in Japan and South Korea are categorized as "Asia". Over and above the location of the job offer, VC-backed startups are consistently associated with a wage premium relative to established firms. It is worth highlighting that the estimated effect is slightly smaller in magnitude. The attenuation is consistent with the intuition that the cost of living in the employer's area is positively associated with regional wages. Nonetheless, geographic differences can be ruled out as the main mechanism that explains the higher wages at VC-backed startups.

As an additional robustness check, I split the analysis into subsamples to assess whether the effect is driven by a peculiar sector or industry. One potential concern is that since venture capital investments tend to be concentrated in a few select industries such as computer software, the estimated wage effect may be driven by wage differences in industry composition rather than those between startups and established firms. Specification (2-6) subsets on the high-tech sector which represents 73% of the VC-backed startups in the labor market for MIT graduates. This regression does not include state fixed effects because VC-backed firms are both concentrated in terms of industry and geography; compared to 65% of high-tech established firms in the sample, 95% of high-tech VC-backed startups are located in California, Massachusetts, or New York. Nonetheless, the effect of startup employment is attenuated to a wage premium of roughly 6%, which is statistically significant at the 10% level. This is not surprising given that inter-industry wage differentials tend to be large and persistent (Katz and Summers, 1989). Overall, even after conditioning on only the high-sector in which VC-backed startups are heavily concentrated, the startup wage premium is positive and significant.

Similarly, specification (2-7) explores the startup-wage relationship across only non-finance jobs. Finance jobs are an important aspect of the labor market outcomes for MIT graduates because it is a lucrative early career track that draws a large share of talent each year. Although Shu (2013) claims that MIT graduates who become financiers versus those enter into the innovation sector are not substitutable in their skill sets, Figure 2 suggests that the allocation of talent has qualitatively shifted from the financial sector to entrepreneurial firms in the recent decade. Therefore, a comparison of the magnitude and sign of the startup wage premium from the full sample against those drawn from only the non-finance jobs is informative. Specification (2-7) shows that the estimated startup wage premium, which remains statistically significant at the 1% level, is larger than documented in the full sample (Specification 2-5). This is expected because finance jobs are generally the most lucrative early career tracks. Overall, these tests on subsamples show that the startup wage premium is not primarily driven by sectoral differences.

Another key concern is the 2008-09 Financial Crisis, which occurs in the middle of the sample. Business cycles may influence not only the graduating students' initial career selection but also the wage dynamics within sectors. For instance, it is possible that the crisis more sharply affected jobs in the financial services sector, experiencing greater declines in wages relative to those in other sectors. Therefore, the startup wage "premium" effect could be an artifact of financial services firms (more) steeply reducing compensation during the financial crisis. In order to address this issue, I split the sample into three periods: before, during, and after the financial crisis. Given MIT undergraduates typically search for full-time jobs almost a year before their graduation, I categorize the three time periods as graduation years 2006-2008 (pre), 2009-2010 (during), and 2011-2014 (after).

Overall, the results in Table 3 reflect the underlying business cycles. Venture-backed startup wage estimates noticeably vary across the three time periods centered on the financial crisis. In the years before and after the financial crisis, the VC-backed startups are consistently associated with a wage premium relative to established firms. These results are consistent with the original finding that VC-backed startups are associated with higher salary offers than established firms.

However, the wage relationship is strikingly different during the financial crisis. Although statistically insignificant, VC-startup wages are qualitatively lower than those at established firms. This suggests that the financial crisis exerted a more severe liquidity shock on small and young firms, limiting their ability to pay attractive salaries. Soon after, startup wages appear to recover back to their pre-recession levels by the early 2010s.

While VC-startup wage effects are quite different during the financial crisis, the results clearly confirm that the documented VC-startup wage premium in the cross-section is not driven by the financial crisis. During the financial crisis, there is no wage premium associated with VC-backed startups. In this regard, the inclusion of the financial crisis period in the sample only attenuates the VC-startup wage estimates towards zero. Therefore, business cycles do not appear to be a credible alternative explanation to the startup wage premium documented in the cross-section.

Lastly, I explore the startup-wage relationship at other points in the distribution in order to check that the mean effect is not predominantly driven by outliers. Appendix Table A5 presents the quantile regression points estimates at each decile of the conditional wage distribution. I find that the startup wage premium is highest for workers at middle to high range of the conditional earnings distributions while much lower for those at either tail of the distribution. Overall, the effect of entrepreneurial employment on wages is qualitatively similar given the positive, albeit not always statistically significant, startup-wage relationship at every point in the conditional earnings distribution.

4.2 Testing for Selection

I assess selection as the main channel that may explain the startup wage premium. First, I compare MIT graduates who join VC-backed startups with those who work at established companies with respect to observable characteristics. Large observable differences would suggest that the two types of workers are systematically different and that there may also be unobserved heterogeneity that results in the sorting of workers across employers.

Table 4 shows a series of t-tests of equality of means comparing MIT graduates who join VC-backed startups and those who work at established companies.[24] In terms of academic training, MIT graduates who join VC-backed startups appear to be based more in the

[24] Formula for normalized differences is adopted from Imbens and Wooldridge (2009).

Engineering school and less in the Management school. To extent that the choice of major at MIT directly shapes the development of skills relevant to future employment, this suggests that the type of human capital that sort into venture-backed startups is qualitatively different.

Moreover, individual characteristics widely vary between employees at established firms and those at VC-backed startups. VC startup joiners are much more likely to be male. More importantly, they tend to receive more job offers. This difference is statistically significant and hints that the students who join VC-backed startups exhibit higher ability or other qualities that are valued by employers.

Taken together, Table 4 illustrates that the two types of workers are systematically different along many observables characteristics including MIT school, gender, and number of offers received. Given these considerable differences, it is plausible that there also exist unobservable qualities by which employees at venture-backed startups and workers at established firms differ. Estimated wage premia in Table 2 are especially concerning if these unobservable differences are linked to wages. As explained in Section II, the primary concern for selection is found in Dahl and Klepper (2015). The model shows that high quality workers are matched to large productive firms, leaving low quality workers to be allocated to startups.

Although Equation (2) cannot be directly tested because worker ability is unobservable, I use bundles of offers for each individual to account for both observed and unobserved individual characteristics. Before the analysis of wages at the offer-level rather than at the individual level, I revisit the key identification assumption that individuals who receive a single job offer are not systematically different from those who receive multiple. Table 5 presents a series of t-tests of equality of means for both individual and employer characteristics associated with the job offer.

Administratively observable individual traits such as gender, citizenship, graduation year, MIT school of affiliation are statically equal between the two groups. Moreover, individuals with a single offer relative to those with multiple offers express similar preferences for the three job attributes (firm reputation, job security, and impactful work) that are related to startup entry. Overall, single and multiple offers appear to be drawn from two demographically similar groups of workers.

For employer characteristics, the share of offers from VC-backed startups is statistically similar although the percentage is slightly higher for multiple offers (8% vs. 13%). However, offered salary and firm age show a sharp contrast between the two groups. It may simply be the case that individuals who are more attractive to employers receiver higher salaries and more job offers. While it is possible that the individuals with multiple offers are drawn from higher points in the ability distribution compared to single-offer individuals, offer salaries are likely inflated due to ex-post bargaining process between the workers and firms. Furthermore, many top MIT graduates have a single job offer because they receive and accept a full-time job offer from their summer internship prior to their senior year and thus do not participate in the ensuing full-time job recruiting. Nonetheless, while selection is a potential issue, the balanced individual-level covariates as well as the subsequent results consistent with the startup wage premium documented in Table 2 are reassuring.

Table 6 presents the offer-level relationship between entrepreneurial employment and wages. Consistent with the findings in Table 2, specification (6-1) shows that job offers from VC-backed startups are roughly 9% higher in compensation than those from established firms. This effect is positive and statistically significant at the 1% level. Results are consistent for job offers from both high-tech (6-3) and non-finance sectors (6-5). As similarly documented in Table 2, non-VC startups generally appear to relatively low wages when compared to both established and VC-backed startup firms.

Finally, I introduce individual fixed effects to account for both observed and unobserved individual traits including ability. All controls are omitted because they are time-invariant individual-level covariates whose effects are absorbed by the individual fixed effects. Specification (6-2) shows that the effect of startup employment on wages is statistically insignificant from zero. The sign flips to negative to roughly -6% although the point estimate is not statistically significant. Nonetheless, it is clear that accounting for heterogeneity across workers erases the relatively high wages associated with startup employment. In other words, conditional on worker quality, startups and established employers pay similar wages. This indicates that the cross-sectionally observed startup wage premium is primarily driven by selection. The results are consistent for both high-tech (6-4) and non-finance (6-6) job offers. Therefore, I accept the second hypothesis.

It is worth emphasizing that many high ability workers appear to select into entrepreneurial firms, which then pay high wages for superior talent. In a counterfactual world in which these workers are assigned to large corporations, these workers would also earn similarly high salaries. This implies that VC-backed startups pay competitive wages for talent. While not much is known regarding the personnel economics inside early-stage VC-financed companies, it is surprising that VC-backed startups tend to pay competitive salaries in spite of credit constraints.

4.3 Unpacking the Selection Mechanism

The main finding of this study is that while VC-backed startups seemingly pay higher wages compared to established firms, the "startup wage premium" is explained by high-ability workers sorting into VC-backed startups. Given that the allocation of talent among top graduates poses an important phenomenon, the natural follow-up question is: Why do better graduates end up in VC-backed startups? The following discussion offers some evidence regarding the mechanism that drives many high-ability students to select into entrepreneurial firms vis-à-vis established companies. Motivated by the existing literature, I test a few key predictions surrounding the individuals' ex-ante preferences for particular job attributes. The first half discusses the role of risk-appetite and impatience, and the second half highlights the importance of the job content.

Although ability is not directly measured, there are other proxies that can further clarify the relationship between ability and job selection among MIT graduates. Dohmen et al. (2010) offer experimental evidence that an individual's cognitive ability is systematically related to his preferences for risk and immediate satisfaction. In particular, the authors show that lower cognitive ability is linked to higher levels of risk-aversion and impatience. This leads to the following hypothesis that risk-averse and impatient MIT graduates are more likely to select into established firms than into startups.

To test this key prediction, I leverage a set of questions from the MIT Graduating Student Survey on the students' preferences for certain job attributes. On a 4-point Likert scale of "Not important" to "Essential", each student is asked to evaluate a set of factors (e.g. employer reputation) and their importance to her decision to accept the ultimate job offer. Among the twenty different job attributes included in the survey, the three following job attributes are most closely connected to Dohmen et al. (2010)'s results on risk aversion and impatience: "Job Security", "Employer Reputation", and "First Job Offered".

Table 7A shows a series of multinomial logit regressions estimating likelihood of students' job selection among established firms, VC-startups, and non-VC startups. The main independent variables are a set of preferences for a series of job attributes. Job security and employer reputation, as shown in Specifications 1 and 2, are proxies for the individual's risk appetite given that risk-averse are likely to prioritize job security and organizational reputation. Similarly, preferences for the first job offered is a proxy for the student's level of patience for labor market outcomes.

The results account for the students' initial selection of their area of study (MIT School) as well as for demographic characteristics and year-specific effects; only the specifications with the full set of controls are shown for brevity. First, results from Specifications 1 and 2 reflect a negative and statistically significant relationship between selecting into VC-startups and risk-aversion. A standard deviation increase in the preference for job security or firm reputation is each associated with a roughly 55% lower likelihood of joining a VC-startup compared to an established firm. This is consistent with Roach and Sauermann (2015) who find that startup joiners and founders express a stronger preference for risk than do joiners at established companies.

In addition, Specification 3 indicates a negative and statistically significant association between a worker's ex-ante level of impatience and subsequent selection into VC-startups. A standard deviation increase in the preference for the first job offer is linked to a 29% decline in the likelihood of joining a VC-startup.

These findings confirm the hypothesis that risk-averse and impatient students are more likely to select into established firms. Given the previous implication that high-ability MIT students tend to select into VC-startups and therefore command relatively high wages, these results parallel Dohmen et al. (2010)'s finding that cognitive ability is positively related to risk-taking attitude and patience.

Beyond innate individual characteristics such as ability and risk-appetite, the nature of the job itself may play a vital role in attracting particular types of workers to startups versus established firms. Baron et al. (1996) highlight that employees of high-tech startups are fundamentally motivated by "a desire to work at the technological frontier." Moreover, entrepreneurial workers express a strong desire for autonomy (Roach and Sauermann, 2015). In response, Baron et al. (1996) explain that startup founders intentionally craft and offer "interesting and challenging work" to hire, motivate, and retain skilled employees.

Table 7B illustrates how the content of the job affects the type of firm that MIT graduates choose. More broadly, Specification 1 demonstrates that MIT graduates who express a strong preference for the job content are much more likely to join a VC-backed startup rather than an established firm. The next two specifications indicate a strong and statistically significant relationship between joining a VC-backed startup and preferences for "creative and challenging work" and "opportunity for impact." In other words, MIT students who seek meaningful and challenging work are much more likely to choose jobs at VC-backed startups rather than at established firms. Consistent with prior studies on startup employees (Baron et al., 1996; Sauermann, 2017), workers at VC-backed startups are appear to be distinctly influenced by the content of their job.

A limitation to this analysis is that there may be "ex-post rationalization" by the respondents; since the MIT Graduating Student Survey is administered during the spring of the student's final year – presumably *after* their job search – the students may be inclined to justify their job selection choices. However, albeit only suggestive, these results point to a strong link between individual preferences and job selection. Overall, innate individuals traits such as risk-appetite and patience – which are traits tightly linked to cognitive ability – play an important role in motivating certain types of MIT graduates and talent into entrepreneurial firms vs. established companies. Moreover, the nature of the job itself strongly influences the hiring process in which heterogeneous workers are matched to different firm types; MIT graduates who prefer challenging, creative, and impactful work tend to join VC-backed startups.

5 Conclusion

Human capital is undoubtedly a central component of entrepreneurship. In addition to the company founders, early employees are an indispensable force behind the growth and success of nascent companies. However, little is known regarding the type of workers who self-select into startups as well as the wages early employees earn relative to employees at large established firms. This study offers an empirical treatment of wage differentials between VC-backed startups and established firms — two firm types that compete for high talent.

Using data from graduating college students from MIT, I show that early employees earn roughly 10% higher salaries than their counterparts at established firms. It appears that selection is the primary channel through which startups appear to pay a wage premium in the cross-section. Holding worker ability constant in a framework of multiple job offers, I show that early employees earn statistically equal wages as employees at large established firms. In sum, these findings suggest that high-ability workers, who command high wages in both employment settings, tend to select into VC-backed startups, thereby creating an illusion of a cross-sectional wage premium associated with startups.

Wage parity between VC-backed startups and established firms stands in contrast to the existing evidence that small and young businesses tend to pay lower wages. This set of seemingly inconsistent results is likely driven by the fact small businesses and high-growth startups are systematically different (Schoar, 2010; Hurst and Pugsley, 2011; Guzman and Stern, 2016). From a policy perspective, this finding lends insight to Shane's (2009) claim that it is "bad policy" to simply encourage more people to become entrepreneurs since the vast majority of new businesses create very small economic impact. In line with Shane (2009), this study documents that entry-level jobs created by high-growth startups are as well-paying as their counterparts at established companies. Therefore, policy efforts aimed to create "good jobs" should pay special attention to high-growth entrepreneurship rather than all young and new businesses.

Another interpretation of this study is that VC-backed startups pay competitive wages for talent. The wage parity between startups and established is surprising considering that nascent companies are typically credit constrained; it is commonly believed that startups offer equity compensation in order to justify a below-market salary (Booth, 2006). A complementary insight is that non-VC backed startups systematically pay relatively low wages compared to both established firms and venture-backed startups. Taken together, these findings clarify a fundamental role of venture capital: financially equipping young firms to be able to pay attractive salaries.

Moreover, this study concludes by offering some evidence regarding the selection mechanism. Consistent with prior evidence that lower cognitive ability is linked to greater risk-aversion and impatience (Dohmen et al., 2010), MIT graduates who strongly prefer job security (i.e. risk-averse) and first job offered (i.e. impatient) are significantly more likely to join established firms rather than startups. Moreover, job content strongly predicts the type of firm that the individual chooses to join; MIT graduates who desire creative, challenging, and impactful work are much more likely to join startups. These findings imply that managers at older companies can build and reinforce a culture of autonomy and impact – which startups generally embody (Baron et al., 1996) – to appeal to the "entrepreneurial talent" that would otherwise sort into their younger competitors.

A limitation of this study is that its findings may not be generalizable to the broader labor market since MIT represents a highly selected sample of workers at the right-tail of the ability distribution. However, the narrow nature of the sample is advantageous in many ways. MIT's distinctly high level of human capital generates a local labor market in which various types of firms vigorously compete for talent. Therefore, unlike many other labor markets, MIT students typically receive numerous job offers — some of which are from established firms and others from VC-backed startups. In addition, while many studies on the financial returns to entrepreneurship include both lifestyle businesses and high-growth startups in their comparison to large employers, the former is not an appropriate basis for comparison because small businesses often employ low-skilled workers who are not fit for high-productivity roles at large established firms. In contrast, MIT graduates generally possess the type of human capital that is sought after by both high-growth startups and mature firms. Lastly, this paper's multiple offers methodology turns on the fact that a sufficient number of workers receive offers from both firm types, making MIT an empirically advantageous setting to compare wages between VC-backed startups and established firms. Nevertheless, the insights drawn from MIT graduates and their labor market outcomes are generally limited to high-skilled young workers.

This study motivates numerous questions for future research. I discuss three promising follow-on questions. The first concerns the gender effect on VC-backed startup entry and wages. Although not explicitly addressed given the limited scope of this study, males are positively and significantly associated with a wage premium in almost all of the regression analyses in this study. Such a systematic gender inequity in pay is surprising: this sample is selected from a single elite research university, meaning that the men and women in this setting exhibit similar ex-ante characteristics (e.g. age) and experience identical training and career opportunities. While a vast literature surrounds the topic of gender discrimination, more research is needed to better understand the complex interplay between gender, endogenous job selection, and wages among the elite STEM-educated workforce. It would be promising to leverage multiple job offers – as done in this study – to empirically pin down the demand- vs. supply-side factors that govern gender pay inequity among STEM-educated workers.

Second, how does homphily (i.e. MIT graduates being drawn to MIT alumni-founded companies) impact the job offer that the worker ultimately chooses and the resulting wages? On the one hand, social ties may mitigate the "liability of newness" (Stinchcombe, 1965) that hampers new organizations' ability to attract top talent. On the other hand, homophily can positively bias employers' evaluation of socially connected job candidates (c.f., Gompers et al. (2016) for homophily in hiring among venture capital investors), leading firms to make poor hiring decisions. Given the high rates of entrepreneurship among MIT alumni, it would be insightful to better understand the real economic impact of homophily on hiring and wage outcomes.

Third, entry-level salaries for college graduates provide a setting for meaningful comparisons because these individuals possess almost identical pre-entry levels of education, social capital, and work experience. However, there are several open empirical questions regarding the real effects of entrepreneurial employment in the long-run. Do early employees develop a different set of skills as well as social ties that directly shape their follow-on productivity and earnings? If entrepreneurship is a skill that can be learned, does experience as an early employee directly affect the individual's future entry into business ownership and conditional on entry, the individual's performance (changes at the extensive and intensive margin, respectively)? Given that entrepreneurial success is extremely difficult to predict ex-ante (Kerr et al., 2014) and that most startups fail, how much of the real effects of entrepreneurial employment vary around the performance of the startup employer? It is vital that scholars at the intersection of labor economics and entrepreneurship examine more deeply the role and impact of early employees.

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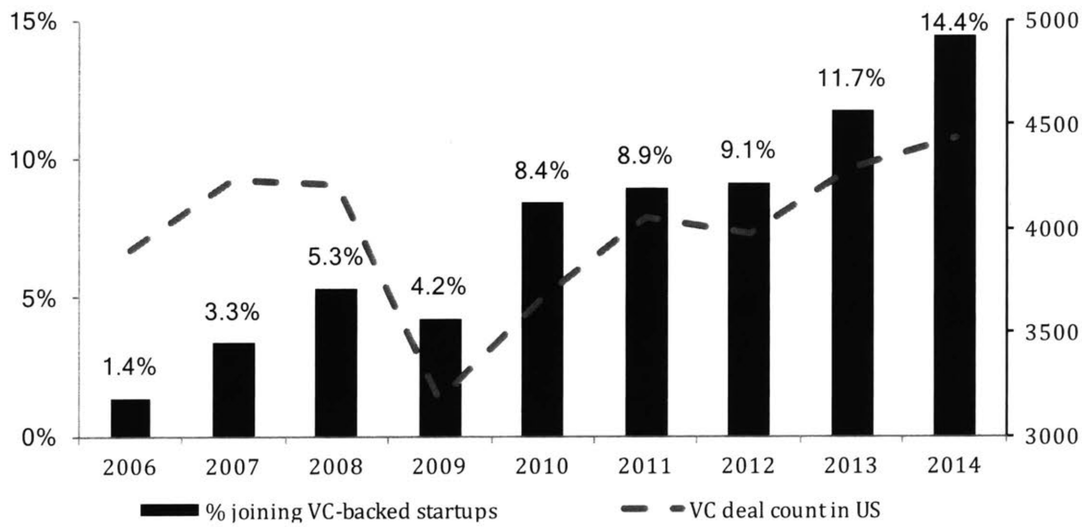
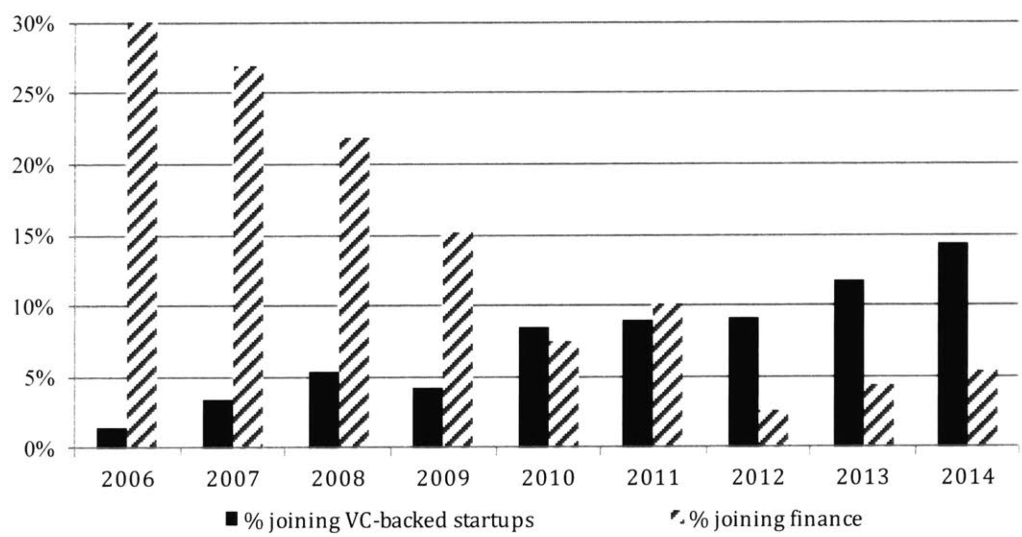
Figures and Tables

Figure 1: Allocation of MIT Graduates into VC-Backed Startups Relative to Total VC Investments



Notes: Join percentage is calculated based on the subset of graduating seniors at MIT who select into full-time employment.
Source: MIT Graduating Student Survey; PricewaterhouseCoopers and National Venture Capital Association

Figure 2: Allocation of MIT Graduates into VC-Backed Startups vs. Finance, 2006-2014



Notes: Join percentages are calculated based on the subset of graduating seniors at MIT who select into full-time employment. Finance includes financial services (commercial banking and insurance), investment banking, and money management. *Source*: MIT Graduating Student Survey

Table 1: Summary Statistics (N=2064 workers)

Individual Characteristics	**Mean**	**Median**	**SD**	**Min**	**Max**
Male	0.43	0	0.49	0	1
US Citizen	0.87	1	0.33	0	1
Graduation year	2010.27	2011.00	2.63	2006	2014
Number of offers received	1.94	1	1.34	1	12
MIT School					
Architecture and Planning	0.01	0	0.08	0	1
Engineering	0.63	1	0.48	0	1
Humanities, Arts, & Social Sciences	0.06	0	0.25	0	1
Management	0.10	0	0.30	0	1
Science	0.20	0	0.40	0	1
Employer Characteristics (Accepted Offer)	**Mean**	**Median**	**SD**	**Min**	**Max**
Firm Type					
Established Firm	0.89	1	0.31	0	1
VC-Backed Startup	0.08	0	0.27	0	1
Non-VC backed Startup	0.03	0	0.16	0	1
Salary ($2006)	61,614	60,094	19,033	10,537	187,271
Firm age (at graduation year)	54.06	34	52.94	1	348
Industry					
Aerospace and Defense	0.07	0	0.25	0	1
Automotive and Transportation	0.01	0	0.12	0	1
Business Services (Advertising, Real Estate, Retail)	0.02	0	0.15	0	1
Chemicals and Materials	0.02	0	0.14	0	1
Computer Hardware/ Electrical Engineering	0.03	0	0.18	0	1
Computer Software	0.17	0	0.37	0	1
Consulting	0.15	0	0.36	0	1
Education	0.02	0	0.16	0	1
Energy and Utilities	0.04	0	0.19	0	1
Engineering	0.09	0	0.28	0	1
Financial Services	0.06	0	0.23	0	1
Health/Medicine	0.04	0	0.19	0	1
Industrial and Consumer Manufacturing	0.02	0	0.13	0	1
Money Management	0.13	0	0.34	0	1
Pharmaceutics (Biotech, Medical Device)	0.03	0	0.17	0	1
Other	0.06	0	0.23	0	1

Table 2: OLS Cross-Sectional Wage Regression

	Dependent Variable: Log Salary of Accepted Offer						
	All	All	All	All	All	High-Tech only	Non-Finance
Omitted: Established Firm	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VC-Backed Startup	0.130***	0.110***	0.0935***	0.0791***	0.0693***	0.0550*	0.106***
	(0.0261)	(0.0259)	(0.0249)	(0.0258)	(0.0235)	(0.0313)	(0.0273)
Non-VC Startup	-0.0723	-0.0833	-0.0931*	-0.0642	-0.0250	-0.116*	-0.0963**
	(0.0557)	(0.0550)	(0.0498)	(0.0512)	(0.0537)	(0.0607)	(0.0473)
Male		0.122***	0.123***	0.114***	0.0919***	0.0633***	0.0875***
		(0.0170)	(0.0166)	(0.0163)	(0.0152)	(0.0240)	(0.0177)
US Citizen		-0.0696***	-0.0665***	-0.0585***	-0.0685***	-0.0626	-0.0507*
		(0.0227)	(0.0220)	(0.0222)	(0.0198)	(0.0399)	(0.0263)
Number of offers received			0.0552***	0.0555***	0.0464***	0.0374***	0.0469***
			(0.00603)	(0.00604)	(0.00554)	(0.00794)	(0.00624)
Constant	10.96***	11.02***	10.96***	10.76***	10.75***	10.95***	10.72***
	(0.0209)	(0.0291)	(0.0296)	(0.105)	(0.106)	(0.0814)	(0.109)
Location (State) Fixed Effects	No	No	No	No	Yes	No	No
MIT School Fixed Effects	No	No	No	Yes	Yes	Yes	Yes
Observations	2064	2064	2064	2052	2024	718	1667

Notes: This table reports OLS wage regressions on a sample of 2,064 graduating seniors from MIT. The unit of observation is the individual with employment information based on his or her accepted job offer. All specifications include graduation year fixed effects. Robust standard errors are shown in parentheses. * p < 0.10; ** p < 0.05; *** p < .01

Table 3: Wage Regressions Before, During, and After the Financial Crisis

	Dependent Variable: Log Salary of Accepted Offer				
	All	All	All	High-Tech	Non-Finance
	(1)	(2)	(3)	(4)	(5)
Before the Financial Crisis					
VC-Backed Startup	0.118**	0.120**	0.119**	0.165*	0.154**
	(0.0506)	(0.0567)	(0.0577)	(0.0890)	(0.0658)
Observations	636	626	626	189	442
During the Financial Crisis					
VC-Backed Startup	-0.00156	-0.00137	-0.0100	-0.0974	-0.0254
	(0.0784)	(0.0737)	(0.0748)	(0.0969)	(0.0836)
Observations	391	379	379	121	317

After the Financial Crisis					
VC-Backed Startup	0.161***	0.0953***	0.0745***	0.0626*	0.122***
	(0.0304)	(0.0284)	(0.0275)	(0.0334)	(0.0311)
Observations	1037	1019	1019	408	908
Control Variables	No	Yes	Yes	Yes	Yes
Graduation Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Location (State) Fixed Effects	No	Yes	Yes	No	No
MIT School Fixed Effects	No	Yes	Yes	Yes	Yes

Notes: This table reports OLS wage regressions on a sample of 2,064 graduating seniors from MIT. The unit of observation is the individual with employment information based on his or her accepted job offer. From the three-categorization of firm types, Established Firms are the omitted category. Control Variables include binary indicators for whether the accepted offer was associated with Non-VC startup, Male, and US Citizenship. Robust standard errors are shown in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < .01$

Table 4: Univariate Difference in Means Test: Workers at Established Firms vs. Early Employees

	Est. Firm Employees (n=1,851)	VC-Backed Startup Emps. (n=160)	t-Stat: Equal Means	Norm Diff.
Individual Characteristics				
MIT School				
Architecture and Planning	0.01	0.01	0.04	0.00
Engineering	0.62	0.81	4.68	-0.18
Humanities, Arts, & Social Sciences	0.07	0.02	2.44	0.05
Management	0.10	0.01	3.79	0.09
Science	0.20	0.16	1.32	0.04
Male	0.40	0.64	5.92	-0.24
US Citizen	0.87	0.87	0.14	0.00
Number of Offers Received	1.91	2.28	3.38	-0.37

Notes: This table reports a series of t-tests of equality of means between MIT graduates who join established companies and those who join VC-backed startups; students who join non-VC startups are omitted for brevity. Differences are normalized based on Imbens et al. (2009). * $p < 0.10$; ** $p < 0.05$; *** $p < .01$

Table 5: Univariate Difference in Means Test: Single vs. Multiple Job Offers

	Single offers (n=328)	Multiple offers (n=658)	*t*-Stat: Equal Means	Norm. Diff.
Individual Characteristics				
Male	0.42	0.42	0.11	0.01
US Citizen	0.89	0.86	1.06	0.05
Graduation Year	2011.41	2011.59	0.88	0.04
MIT School				
Architecture and Planning	0.01	0.00	1.29	0.06
Engineering	0.66	0.72	1.97	0.10
Humanities, Arts, & Social Sciences	0.06	0.05	0.82	0.04
Management	0.07	0.06	0.74	0.04
Science	0.20	0.17	1.16	0.06
Job Preferences				
Firm Reputation	0.65	0.64	0.24	0.01
Job Security	0.40	0.38	0.32	0.02
Opportunity for Impactful Work	0.70	0.72	0.61	0.03
Employer Characteristics				
Offer Salary ($2006)	58,752	67,482	6.70	0.35
VC-Backed Startup	0.08	0.13	1.90	0.10
Firm Age	56.79	47.56	2.67	0.13
Industry				
Aerospace and Defense	0.09	0.08	0.85	0.04
Automotive and Transportation	0.03	0.02	1.64	0.08
Business Services (Advertising, Real Estate, Retail)	0.01	0.01	0.24	0.01
Chemicals and Materials	0.01	0.02	0.17	0.01
Computer Hardware/ Electrical Engineering	0.02	0.03	0.12	0.01
Computer Software	0.15	0.23	2.80	0.15
Consulting	0.16	0.18	0.79	0.04
Education	0.03	0.01	2.25	0.10
Energy and Utilities	0.06	0.10	1.95	0.10
Engineering	0.06	0.05	0.59	0.03
Financial Services	0.07	0.09	0.72	0.04
Health/Medicine	0.06	0.02	3.00	0.14
Industrial and Consumer Manufacturing	0.04	0.02	1.45	0.07
Money Management	0.07	0.08	0.40	0.02
Pharmaceutical (Biotech, Medical Device)	0.05	0.03	1.86	0.09

Notes: This table reports a series of t-tests of equality of means between job offers among MIT graduates who receive one offer and job offers among MIT graduates who receive multiple offers. Differences are normalized based on Imbens et al. (2009). Variables of comparison are observable individual characteristics and preferences for job attributes. Each job preference is a dummy equaling one if the respondent indicated that the particular job attribute was considered "essential" or "very important" in choosing the ultimate job offer.

Table 6: OLS Offer-Level Wage Regression with Individual Fixed Effects

	Dependent Variable: Log Offer Salary					
	All		High-Tech only		Non-Finance	
Omitted: Established Firm	(1)	(2)	(3)	(4)	(5)	(6)
VC-Backed Startup	0.0864***	-0.0652	0.0981**	-0.0693	0.0913**	-0.0613
	(0.0328)	(0.0554)	(0.0419)	(0.0868)	(0.0366)	(0.0508)
Non-VC Startup	-0.108	-0.0713	-0.0771	0.0671	-0.163**	-0.0979
	(0.0673)	(0.0565)	(0.0911)	(0.163)	(0.0702)	(0.0708)
Male	0.0907***		0.0502		0.0812***	
	(0.0240)		(0.0351)		(0.0257)	
US Citizen	-0.131***		-0.0904*		-0.118***	
	(0.0261)		(0.0473)		(0.0266)	
Number of Offers Received	0.0342***		0.0601***		0.0304***	
	(0.0106)		(0.0130)		(0.0108)	
Constant	11.45***	11.29***	11.17***	11.26***	11.39***	11.3***
	(0.197)	(0.0709)	(0.142)	(0.168)	(0.206)	(0.092)
Individual fixed Effects	No	Yes	No	Yes	No	Yes
Observations (offers)	658	658	319	319	556	556

Notes: This table shows the result of individual fixed-effects OLS regressions on a sample of 658 job offers. The unit of observation is a job offer made to a graduating senior at MIT. All specifications include graduation year and MIT School fixed effects. Robust standard errors are shown in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < .01$

Table 7A: Risk Appetite, Impatience, and Job Selection

Multinomial Logit DV: Type of Firm Chosen Omitted: Established Firm	**Main Independent Variable: Job Attributes** **Job Security**	 **Employer Reputation**	 **First Job Offered**
VC-Backed Startup	0.525	0.503	0.765
	-0.645***	-0.686***	-0.268**
	(0.120)	(0.114)	(0.117)
Non-VC Startup	0.478	0.338	0.932
	-0.738***	-1.084***	-0.0700
	(0.169)	(0.188)	(0.171)
Controls	Yes	Yes	Yes
Graduation Year Fixed Effects	Yes	Yes	Yes
MIT School Fixed Effects	Yes	Yes	Yes
Observations	1811	1933	1746

... = odds ratio

[...] = coefficient on logit

(...) = standard error on logit

Notes: This table reports multinomial logit regressions estimating the likelihood of the type of firm chosen by the individual. The dependent variable is the firm type for which "Established Firm" is the omitted category. Independent variables are the individual's preference levels for particular job attributes on a scale of 1 ("not important") to 4 ("essential"). The unit of observation is the individual. Controls include indicator variables for male and US citizenship. Constants are undisplayed for brevity. Robust standard errors are shown in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < .01$

Table 7B: Job Content and Job Selection

Multinomial Logit DV: Type of Firm Chosen Omitted: Established Firm	**Main Independent Variable: Job Attributes** **Job Content**	 **Creative & Challenging Work**	 **Opportunity for Impact**
VC-Backed Startup	1.479	1.747	1.376
	0.391***	0.558***	0.319***
	(0.149)	(0.151)	(0.114)
Non-VC Startup	0.918	0.914	0.876
	-0.0858	-0.0899	-0.132
	(0.195)	(0.202)	(0.186)
Controls	Yes	Yes	Yes
Graduation Year Fixed Effects	Yes	Yes	Yes
MIT School Fixed Effects	Yes	Yes	Yes
Observations	1992	1949	1845

... = odds ratio

[...] = coefficient on logit

(...) = standard error on logit

Notes: This table reports multinomial logit regressions estimating the likelihood of the type of firm chosen by the individual. The dependent variable is the the firm type for which "Established Firm" is the omitted category. Independent variables are the individual's preference levels for particular job attributes on a scale of 1 ("not important") to 4 ("essential"). The unit of observation is the individual. Controls include indicator variables for male and US citizenship. Constants are undisplayed for brevity. Robust standard errors are shown in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < .01$

Appendix

Table A1: Assessment of Survey Response Bias through Difference in Means Test

	Non-Response (n=1,874)	Response (n=626)	*t*-Stat: Equal Means	Norm. Diff.
Individual Characteristics				
Male	0.42	0.44	0.66	-0.02
US Citizen	0.87	0.89	1.23	-0.02
Graduation Year	2009.74	2011.60	15.39	-1.86
Offer Count	1.95	1.93	0.21	0.01
MIT School				
Architecture and Planning	0.01	0.01	0.74	0.00
Engineering	0.61	0.67	2.45	-0.06
Humanities, Arts, & Social Sciences	0.07	0.06	0.49	0.01
Management	0.11	0.06	3.90	0.06
Science	0.20	0.20	0.10	0.00
Employer Characteristics (Accepted Offer)				
Salary ($2006)	61506.19	61879.32	0.40	-373.13
Industry				
Aerospace and Defense	0.07	0.06	0.71	0.01
Architecture and Urban Planning	0.00	0.01	1.14	0.00
Automotive and Transportation	0.01	0.02	2.32	-0.01
Business Services (Advertising, Real Estate, Retail)	0.02	0.03	0.47	0.00
Chemicals and Materials	0.02	0.02	0.09	0.00
Communications, Arts, Entertainment	0.01	0.01	0.38	0.00
Computer Hardware/ Electrical Engineering	0.03	0.03	0.44	0.00
Computer Software	0.16	0.19	1.67	-0.03
Consulting	0.15	0.16	0.71	-0.01
Education	0.02	0.03	0.71	-0.01
Energy and Utilities	0.03	0.05	2.08	-0.02
Engineering	0.09	0.09	0.02	0.00
Financial Services	0.06	0.05	0.91	0.01
Government	0.01	0.01	1.78	0.01
Health/Medicine	0.04	0.04	0.40	0.00
Industrial and Consumer Manufacturing	0.02	0.02	1.09	-0.01
Money Management	0.15	0.08	4.52	0.07
Law	0.00	0.00	0.17	0.00
Military	0.01	0.01	1.33	0.01
Non-Profit Agency or NGO	0.00	0.01	2.18	-0.01
Pharmaceutical (Biotech, Medical Device)	0.03	0.03	0.57	0.00
Other	0.06	0.06	0.13	0.00

Table A2: OLS Cross-Sectional Wage Regression, US Citizens Only

	Dependent Variable: Log Salary of Accepted Offer						
	All	All	All	All	All	High-Tech only	Non-Finance
Omitted: Established Firm	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VC-Backed Startup	0.138***	0.119***	0.104***	0.0917***	0.0826***	0.0649*	0.118***
	(0.0281)	(0.0276)	(0.0268)	(0.0277)	(0.0256)	(0.0333)	(0.0293)
Non-VC Startup	-0.0773	-0.0886	-0.104*	-0.0746	-0.0349	-0.126*	-0.123**
	(0.0634)	(0.0630)	(0.0570)	(0.0574)	(0.0596)	(0.0706)	(0.0522)
Male		0.130***	0.130***	0.115***	0.0889***	0.0712***	0.088***
		(0.0182)	(0.0178)	(0.0174)	(0.0164)	(0.0254)	(0.0188)
Number of offers received			0.0532***	0.0534***	0.0453***	0.0364***	0.046***
			(0.00641)	(0.00643)	(0.00587)	(0.00826)	(0.00663)
Constant	10.95***	10.95***	10.89***	10.76***	10.74***	10.89***	10.74***
	(0.0221)	(0.0222)	(0.0232)	(0.0880)	(0.0945)	(0.0640)	(0.0897)
Location (State) Fixed Effects	No	No	No	No	Yes	No	No
MIT School Fixed Effects	No	No	No	Yes	Yes	Yes	Yes
Observations	1800	1800	1800	1796	1775	632	1480

Notes: This table reports OLS wage regressions on a sample of domestic (i.e. US citizen) graduating seniors from MIT. The unit of observation is the individual with employment information based on his or her accepted job offer. All specifications include graduation year fixed effects. Robust standard errors are shown in parentheses. * p < 0.10; ** p < 0.05; *** p < .01

Table A3: OLS Cross-Sectional Wage Regression Based on Total Compensation

	Dependent Variable: Log Total Compensation Package of Accepted Offer						
	All	All	All	All	All	High-Tech only	Non-Finance
Omitted: Established Firm	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VC-Backed Startup	0.0925***	0.0705**	0.0690***	0.0589**	0.0407	0.0245	0.077***
	(0.0280)	(0.0278)	(0.0251)	(0.0254)	(0.0250)	(0.0340)	(0.0289)
Non-VC Startup	-0.102*	-0.114*	-0.0554	-0.0314	-0.0468	-0.146**	-0.121**
	(0.0590)	(0.0583)	(0.0591)	(0.0611)	(0.0564)	(0.0612)	(0.0494)
Male		0.137***	0.106***	0.0955***	0.0983***	0.0709**	0.094***
		(0.0187)	(0.0173)	(0.0171)	(0.0167)	(0.0275)	(0.0194)
US Citizen		-0.088***	-0.090***	-0.083***	-0.082***	-0.083*	-0.068**
		(0.0249)	(0.0221)	(0.0223)	(0.0218)	(0.0443)	(0.0290)
Nb. of offers received					0.0532***	0.0440***	0.056***
					(0.00592)	(0.00925)	(0.00692)
Constant	11.05***	11.13***	11.02***	10.68***	10.70***	11.00***	10.76***

	(0.0234)	(0.0320)	(0.0429)	(0.114)	(0.111)	(0.0871)	(0.112)
Location (State) Fixed Effects	No	No	Yes	Yes	Yes	No	No
MIT School Fixed Effects	No	No	No	Yes	Yes	Yes	Yes
Observations	2064	2064	2036	2024	2024	718	1667

Notes: This table reports OLS wage regressions on a sample of 2,064 graduating seniors from MIT. The unit of observation is the individual with employment information based on his or her accepted job offer. Total compensation includes salary and non-salary compensation such as signing bonus. All specifications include graduation year fixed effects. Robust standard errors are shown in parentheses. * p < 0.10; ** p < 0.05; *** p < .01

Table A4: OLS Offer-Level Wage Regression Based on Total Compensation

	Dependent Variable: Log Total Compensation Package of Accepted Offer					
	All		High-Tech only		Non-Finance	
Omitted: Established Firm	(1)	(2)	(3)	(4)	(5)	(6)
VC-Backed Startup	0.0738**	-0.0793	0.109**	-0.0861	0.0810**	-0.0767
	(0.0343)	(0.0555)	(0.0438)	(0.0894)	(0.0379)	(0.0509)
Non-VC Startup	-0.117	-0.0695	-0.112	0.0327	-0.166**	-0.105
	(0.0734)	(0.0591)	(0.0996)	(0.168)	(0.0739)	(0.0739)
Male	0.0853***		0.0377		0.0700**	
	(0.0258)		(0.0388)		(0.0276)	
US Citizen	-0.163***		-0.108**		-0.136***	
	(0.0288)		(0.0521)		(0.0283)	
Constant	11.45***	11.29***	11.17***	11.26***	11.39***	11.31***
	(0.197)	(0.0709)	(0.142)	(0.168)	(0.206)	(0.0915)
Individual fixed Effects	No	Yes	No	Yes	No	Yes
Observations (offers)	658	658	319	319	556	556

Notes: This table shows the result of individual fixed-effects OLS regressions on a sample of 658 job offers. The unit of observation is a job offer made to a graduating senior at MIT. Total compensation includes salary and non-salary compensation such as signing bonus. All specifications include graduation year and MIT School fixed effects. Robust standard errors are shown in parentheses. * p < 0.10; ** p < 0.05; *** p < .01

Table A5: Quantile Wage Regressions

	Dependent Variable: Log Salary of Accepted Offer								
Percentiles	10	20	30	40	50	60	70	80	90
VC-Backed Startup	0.0480	0.0417	0.0737*	0.0631**	0.0751***	0.103***	0.0833***	0.0624**	0.0411
	(0.0496)	(0.0277)	(0.0438)	(0.0319)	(0.0245)	(0.0214)	(0.0224)	(0.0268)	(0.0398)
Non-VC Startup	-0.190**	-0.0449	-0.0220	-0.0715	-0.0704*	-0.0471	-0.0123	-0.0467	0.0399
	(0.0824)	(0.0459)	(0.0727)	(0.0530)	(0.0407)	(0.0355)	(0.0373)	(0.0445)	(0.0662)
Constant	10.67***	10.51***	10.60***	10.65***	10.64***	10.74***	10.79***	10.87***	10.87***
	(0.597)	(0.333)	(0.527)	(0.385)	(0.295)	(0.257)	(0.270)	(0.323)	(0.480)
Observations	2052	2052	2052	2052	2052	2052	2052	2052	2052

Notes: This table reports unconditional quantile wage regressions on a sample of 2,052 graduating seniors from MIT. The unit of observation is the individual with information regarding his or her accepted job offer. All specifications include graduation year, state, and MIT school fixed effects as well as all the control variables used in Table (2-5). Robust standard errors are shown in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < .01$

Chapter 4

Predictable Exodus: Startup Acquisitions and Employee Departures

Abstract: This paper investigates the effectiveness of startup acquisitions as a hiring strategy. Unlike conventional hires who choose to join a new firm on their own volition, most acquired employees do not have a voice in the decision to be acquired, much less by whom to be acquired. Startup acquisitions therefore provide an empirical setting in which non-founding employees are quasi-randomly assigned to a new employer. I argue that the lack of worker choice lowers the average match quality between the acquired employees and the acquiring firm, leading to elevated rates of turnover. Using comprehensive employee-employer matched data from the US Census, I document that acquired workers are significantly more likely to leave compared to regular hires. Moreover, I demonstrate that these departures can be largely predicted ex-ante. Leveraging population data on career histories, I construct a measure of "startup affinity" for each target and acquiring firm based on pre-acquisition employment patterns, and show that this strongly predicts post-acquisition worker retention. Lastly, these departures suggest a deeper strategic cost of competitive spawning: Upon leaving, acquired workers are more likely to found their own companies, many of which appear to later compete against the buyer.

1 Introduction

A vast literature on organizations has long explored how firms gain advantage by hiring, developing, and retaining human capital (Becker 1962; Coff 1997; Acemoglu and Pischke 1998; Lazear and Shaw 2007; Teece 2011). In addition to conventional hiring, whereby an individual job seeker and an employer agree to an employment contract, firms may bring in talent through other channels. Especially in tight labor markets, firms can hire by acquiring other companies that employ talented workers (Ouimet and Zarutskie 2016). This practice is especially common among startup companies whose most valuable – if not the only – asset is their human capital (Chatterji and Patro 2014). Consistent with this view, Mark Zuckerberg once remarked, "We buy companies to get excellent people."

While startup acquisitions allow the buyer to strategically select and hire a team of workers who have proven to work together productively, this hiring method may be contentious from the perspective of these employees. Unlike regular hires who *choose* to join a new employer, most acquired workers do not have a voice in the decision to be acquired – much less by which firm to be acquired.[25] In other words, startup acquisitions provide an empirical setting in which the acquired workers, and in particular the non-founding employees, are quasi-randomly assigned a new employer. This lack of worker choice may lower the average quality of the match between the acquired workers and the acquiring firm. The resulting mismatch is likely more pronounced when an established company acquires a startup because they are fundamentally different types of organizations with contrasting cultures (Saxenian 1996; Turco 2016) and structures (Hannan and Freeman 1984; Baron, Hannan, and Burton 1999; Sorensen 2007). Following this logic, I hypothesize that startup acquisitions result in high rates of employee exits.

Consider the case of Dialpad Communications,[26] which was acquired by Yahoo in 2005. In addition to Dialpad's nascent internet-based calling technology, Yahoo's key motivation in the acquisition was the talent responsible for the early-stage product. Upon the acquisition, almost all of the 40 employees from Dialpad initially joined the acquirer to help develop its own

[25] For example, Eric Jackson, a former executive at PayPal, describes in his book "The PayPal Wars" that he along with most of the PayPal employees were not aware of the acquisition decision until the final deal was reached and publicly announced. Only the top management team from both companies as well as early investors were involved in the deal-making.
[26] Interviewed by the author on May 31, 2018.

internet-calling software. However, despite the economic incentives to stay,[27] disagreements inside the organization led more than 70% of the former Dialpad employees to leave the firm within three years. Among the departing individuals, the shared motivation was their incompatibility with a large company's bureaucratic environment that prioritized procedures and coordination at the expense of speed and execution. Yahoo's voice-over-IP business struggled to scale, leading the company to eventually shut down the business.

In this paper, I empirically investigate the effectiveness of startup acquisitions as a hiring strategy. To that end, I assemble a comprehensive set of high-tech startup acquisitions in the US between 1990 and 2011 by using employee-employer matched data from the US Census. Unlike many entrepreneurship studies that are limited to observing only the founders, a key advantage of this study is the ability to focus on the non-founding employees, who are unwittingly assigned a new employer upon acquisition.[28] The sample contains roughly 4,000 high-tech startup acquisitions, coupled with 300,000 non-founding employees from the target firms and approximately 2 million workers who are hired at the acquiring firms in the same year as the acquisition. Then, I compare the career paths of the acquired workers versus observationally similar regular hires at the same buyer firm with attention paid to not only the retention outcomes, but also the destinations of the departing employees (e.g., joining another firm vs. founding a new firm).

I provide the first large-scale evidence that acquired startup workers exhibit much higher turnover relative to observationally similar organic hires at the same buyer firm. Acquired workers are almost twice as likely to leave as their counterparts who are conventionally hired. Consistent with the Jovanovic (1979) model of worker tenure and turnover, the differences in departure are highest in the first year and monotonically decline thereafter, as acquired workers who are "good fits" tend to stay with their new employer.

Next, I advance the theory of organizational mismatch as the mechanism that explains the pronounced turnover when an incumbent firm acquires a startup company. Because the acquired workers do not choose their next employer (i.e., the acquiring firm), I posit that the severity of post-acquisition turnover is moderated by the degree of mismatch between the target and

[27] Typically, employment contracts used in startup acquisitions offer employee stock options with stay-incentives, such as a vesting schedule of three to four years. See Coyle and Polsky (2013) for more on standard equity incentives used in startup acquisition.
[28] Nonetheless, all results are consistent when including the founding team.

acquiring firms. To measure organizational mismatch, I empirically characterize and compare the two firms' level of "startup affinity" by analyzing the individuals inside each organization, along with the career decisions that they make. In this framework, organizational mismatch is considered high when the target firm exhibits a strong affinity for startups, especially relative to the acquiring firm. To do this, I leverage population-level data on career histories to characterize the target and acquiring firms based on their pre-acquisition employment patterns.

To determine each company's affinity for startups versus more mature employers, I adopt the methodological framework from Sorkin (2018), and construct an ex-ante measure based on the turnover patterns of each firm's former employees. More specifically, I track employee departures *prior* to the acquisition along with their destinations. While these individuals leave before the acquisition, their decisions to join a young firm or an established company provide useful information for predicting their peers' post-acquisition retention outcomes. When aggregated up, these mobility choices reflect the firm's tendency to attract workers who prefer to transition to startups rather than established firms. Following this reasoning, I define firms to have a strong affinity for startups if their former employees – who leave prior to the acquisition – systematically tend to move to other young companies.

Three central insights lay the foundation for using peer turnover patterns to predict post-acquisition worker retention. First, job transitions are not random: they are intentional choices that reveal workers' preferences for employers. Simply put, a worker's decision to join a particular firm – and thereby *not* join another employer – demonstrates her relative valuation for the two firms based on both pecuniary and non-pecuniary factors (Sorkin 2018). Second, pre-acquisition turnover activity is an *ex-ante* characteristic of each firm. This is an essential component of a prediction method to ensure that the predictor and the outcome variable are not simultaneously determined. Third, job transitions by former colleagues are relevant because organizations tend to attract similar individuals. Since both the acquired and former employees initially selected into joining a particular organization rather than other potential employers, these workers likely exhibit similar preferences for employment.

I find that the pre-acquisition departure patterns strongly predict the acquired employees' decision to stay with the buyer. In short, target companies with a strong affinity for startups exhibit much higher rates of turnover following an acquisition.[29] Furthermore, these effects are magnified when the acquiring firm has a lower startup affinity than the target firm, lending empirical support to the role of organizational mismatch. Therefore, ex-ante differences in the target and buyer's organizational type largely explain why many acquisition deals fail to retain the new workers while others succeed in capturing talent. While earlier studies describe worker-firm match as an "experience good" whose quality can only be realized and assessed ex-post (Jovanovic 1979; O'Reilly et al. 1991), these results demonstrate that match quality can actually be predicted – in this case, before the startup target is acquired. An important managerial implication is that this prediction method offers a new tool to enhance the due diligence process preceding acquisitions. Acquirers can pre-diagnose their likelihood of retaining the new employees by measuring and learning from the target company's – along with their own – prior employment patterns.

Lastly, I show that the departures among acquired workers are strategically costly. Upon leaving, acquired workers are significantly more likely to start their own firms – disproportionately in the same industry as the original target firm. Consistent with retention, this effect is strongest among target firms with a high affinity for startups. While these acquisition-induced entrants could be new competitors or complementors operating in the same industry space, they appear to exert competitive pressures on the acquirer. The buyer firm's long-run performance is negatively correlated with the intensity of post-acquisition spawning by the target employees, and this negative relationship grows with industry similarity.

More broadly, this study sheds light on the fundamental role of worker choices in two ways. First, when workers do not exercise choice during an organizational change – as in the case of acquisitions for non-founding employees – they tend to negatively respond to the transition, primarily by leaving the organization. This is consistent with – and perhaps helps explain – the cultural clash (c.f., Cartwright and Cooper 1992; Van den Steen 2010) and integration issues (Puranam, Singh, and Zollo 2006; Paruchuri, Nerkar, and Hambrick 2006; Hoberg and Phillips 2017) that frequently pervade mergers and acquisitions. Second, when workers leave, their subsequent decisions to join a specific firm rather than others are informative signals of their employer preferences. When these mobility patterns are aggregated up using population-level

[29] As a robustness check, I document that this pattern does not hold for the control group of regular hires, based on their prior employers' departure patterns.

data, they methodically characterize each firm's organizational type. Building on Cyert and March's (1963) foundational insight that an organization is a coalition of individuals, this study shows how individuals' career decisions provide an empirical lens to better understand the nature of firms based on the individuals they tend to attract, along with the choices that these workers make.

The rest of the paper is as follows. Section 2 develops the theoretical background and presents a set of testable hypotheses. Section 3 describes the approach to predicting post-acquisition employee outcomes. Section 4 describes the empirical setting, measurement, and sources of data. Section 5 presents the main results on employee departures, predictability of outcomes, and heterogeneous effects. Section 6 concludes with the study's key insights, managerial lessons, and questions for future research.

2 Conceptual Framework

2.1 Motivations for Startup Acquisitions

Acquisitions of high-tech startups in the US have experienced a steady rise in the past several decades (See Figure 1). Several factors drive the demand for startup acquisitions. I broadly discuss the three primary motivations behind why incumbent firms choose to acquire startup companies. First, buyers frequently acquire startup companies to eliminate nascent competitors. By construction, acquisitions of startup firms tilt the existing competitive landscape toward the acquirer (Gans and Stern 2000). In support of this view, several studies document that established firms acquire nascent targets with technologies that pose competitive threats, and subsequently shut down the target company, or its core product, following the buyout (Santos and Eisenhardt 2009; Cunningham, Ederer, and Ma 2017). Accordingly, the heightened M&A activity among industry incumbents has raised policy concerns around the anti-competitive effects of acquisitions on the entry and survival of new enterprises.[30]

[30] Betsy Morria and Deepa Seetharaman, "The New Copycats: How Facebook Squashes Competition from Startups," *The Wall Street Journal*, August 9 2017, https://www.wsj.com/articles/the-new-copycats-how-facebook-squashes-competition-from-startups-1502293444 (Accessed August 1 2018).

Second, established firms commonly acquire startups to bring in a new source of technological innovation (c.f., Granstrand and Sjolander 1990; Puranam 2001). The "markets for ideas" allow incumbent firms to transact on the startup's stock of knowledge through, for example, patent licensing and transfers (Arora, Fosfuri, and Gambardella 2001). Relatedly, acquirers can effectively complement – or outright outsource – their R&D efforts by acquiring young firms that invest in risky technologies (Higgins and Rodriguez 2006; Kaplan and Lerner 2010). By bringing in the target firm's knowledge stock, the acquirer can exploit the opportunity to produce new innovations by recombining knowledge (Fleming 2001) shared between the two firms (Puranam and Srikanth 2007). Consistent with this view, the broader literature shows that M&A has a positive impact on the buyer's innovation output (Ahuja and Katila 2001; Sevilir and Tian 2012) especially when it shares a large technological overlap with the target firm (Bena and Li 2014).[31]

Lastly, a major motivation for acquisitions is the talent inside the target firm (c.f., Ouimet and Zarutskie 2016). Organization scholars have long recognized human capital as an important asset and thus a source of competitive advantage for firms (Hall 1993; Coff 1997; Acemoglu and Pischke 1998; Teece 2011). However, frictions in external labor markets often limit employers' ability to find, train, and retrain workers. In contrast, acquiring a firm, thereby transferring workers across internal capital markets, bypasses the common frictions in traditional labor markets (Tate and Yang 2016). Since the most valuable – if the not the only – asset of a startup firm is its human capital, acquiring a startup company serves as an alternative channel to capture new talent.

A recent phenomenon, particularly in the technology industry, highlights startup firms as a hotbed of talent. In particular, large technology companies such as Facebook and Cisco have received intense attention in the media for increasingly engaging in "acqui-hiring" – a process in which they buy out startup firms, jettison the core business, and retain the employees.[32] In a hand-collected sample of roughly 100 acqui-hires between 2009 and 2013, Chatterji and Patro (2014) show that the acquirer discontinues the startup's product in a vast majority of the cases. However, roughly 90% of the acquired engineers stay with their new employer for at least a year.

[3131] However, for the innovation output of the target firm, several studies including Kapoor and Lim (2007) and Seru (2014) show that M&A has a negative impact.

[32] Coyle and Polsky (2013) as well as Selby and Mayer (2013) provide conceptual discussions of the "acqui-hiring" phenomenon.

This suggests that the acquirers strategically abandon the startup's core business and efficiently allocate the new workers across existing projects in the company. In other words, many startup acquisitions reflect cases in which the acquirer is chiefly interested in talent.

2.2 Startup Acquisitions as a Hiring Strategy

Why might a firm choose to hire through startup acquisitions rather than the traditional labor market? Compared to conventional hiring, acquiring a team in a single transaction can be advantageous for three reasons. First, the startup team's productivity is easily observable prior to the acquisition. In other words, the acquirer can reduce the information asymmetry problem in hiring (c.f., Jovanovic 1979; Abraham and Farber 1987) by identifying and purchasing a team that has already proven to work together effectively.

Second, startup teams likely accumulate team-specific complementarities that disappear once the team is dissolved. For instance, Jaravel, Petkova, and Bell (2018) document team-specific capital among inventors. Specifically, the authors show that an inventor's long-run productivity suffers when her collaborator experiences a premature death. Given the work culture that startups generally embody (Turco 2016; Corritore 2018), their workers plausibly also develop team-specific capital that leads to productivity gains. Moreover, team-specific complementarities may increase employee retention. Growing evidence on peer effects and "co-mobility" suggests that co-workers often prefer to work together (Marx and Timmermans 2017), meaning that they jointly influence one another's decision to stay with the firm. Therefore, wholly acquiring the team could lead to higher retention and productivity among the acquired employees.

Third, it is difficult to infer an individual's level of contribution to a group's outcome. In other words, an outside firm may be limited in its ability to identify, and thus poach the best employees from a startup team. To illustrate this "metering problem," Alchian and Demsetz (1972) describe two men lifting a heavy cargo into a truck. By observing the total amount of cargo lifted each day, it is impossible to accurately determine each individual's contribution to the group-level output. As a result, without costly assessment of each worker, it would be difficult for an outside firm to identify and hire the top contributors. In parallel, the problem of moral hazard in teams limits an outsider's ability to select out the low quality workers; since only the joint output of the team can be observed, subpar contributors – as well as free-riders – often cannot be identified (Holmstrom 1982). Given the limitations in the ability to properly attribute team output to individual inputs, startup acquisitions may therefore generate efficiency gains in hiring by bringing in the entire team rather than a collection of individuals.

Moreover, the acquiring firm can enhance employee retention by offering stronger employment contracts upon the acquisition than they would with regular hires. New employment contracts for acquired workers commonly include both economic incentives and restrictive clauses designed to reinforce employee retention. Typically, employment contracts used in startup acquisitions offer equity incentives with a vesting schedule of three to four years, along with restrictive clauses like non-competition agreements.[33] By and large, startup acquisitions provide several advantages as a hiring strategy – including contractual levers to increase worker retention – in comparison to conventional hiring.

2.3 Acquired Workers vs. Regular Hires

Theoretical Framework

However, this hiring strategy may be contentious from the perspective of the employee. Unlike regular hires who *choose* to join the new employer on their own volition, acquired workers have limited to no discretion in their employer's ownership change. This is because the target company's decision to be acquired is directed by a few major stakeholders, typically the founders and early investors. In other words, non-founding employees are generally excluded from the pre-acquisition talks regardless of their personal preferences for working at the presumed buyer. Even if these employees are able to anticipate that their firm may be acquired in the future, it is unlikely that they can foresee by *whom* they might be acquired. In this sense, startup acquisitions provide a conceptual framework in which most of the target firm's employees are – from the perspective of these individuals – quasi-randomly assigned to a new employer.

In this theory of organizational mismatch, I posit that the absence of worker choice is the wedge that leads acquired workers to leave the firm at a much higher rate than regular hires in the same firm. The starting premise is that acquired workers do not directly choose their next employer (i.e., the buyer). Suppose a directed search model with labor market frictions in which

[33] See Coyle and Polsky (2013) for discussion on standard equity incentives used in startup acquisitions. Regarding non-compete agreements and startup acquisitions, see Schneid (2006) and Younge, Tong, and Fleming (2014).

(1) individuals have idiosyncratic preferences for employment and (2) they choose to accept a job if the expected utility is greater than the reservation wage. But when a group of workers are assigned a new employer without choice, this generates a labor market in which the matches are strictly determined by the employer side. Put another way, the individual preferences are removed from the search process.

Such condition decreases the expected quality of the match between these individuals (e.g., acquired workers) and employers (e.g., the acquirer). This proposition implies that the average worker-firm match quality will be lower for acquired workers than for regular hires, who in contrast, voluntarily select their new employer based on their employment preferences. As a consequence, the mismatch between the target firm's employees and their new employer could lead to higher rates of employee departures. This leads to *Hypothesis 1*: *Compared to observationally similar regular hires, acquired workers are more likely to leave the firm.*

This mismatch issue may be especially severe in the context of startup acquisitions because the target firm (startup) and the acquirer (established firm) are fundamentally different types of organizations. Among the many differences, a primary distinction between the two types is the corporate culture. Unlike established firms, startup organizations tend to reinforce cultural values of openness and autonomy (Turco 2016; Corritore 2018). Relatedly, organizational structure is a key differentiator. While established firms exhibit increasing levels of bureaucracy as they age (Hannan and Freeman 1984; Sorensen 2007) and grow in size (Saxenian 1996), their younger counterparts generally possess a flatter organizational structure that emphasizes execution speed over formal procedures (Slevin and Covin 1990).

In response to their inherent differences, workers endogenously sort into the startups versus established companies based on their personal preferences for employment. More specifically, workers who prefer risk-taking and challenging work environments tend to self-select into startups (Baron, Burton, and Hannan 1996; Roach and Sauermann 2015; Kim 2018). In contrast, individuals who value job security and employer reputation are more likely to join established companies (Kim 2018). This is consistent with the theory of compensating differentials, where individuals take a pay cut to join a firm that closely matches their preferred employment conditions (Rosen 1987; Sorkin 2018) such as autonomy for scientists (Stern 2004). Consequently, the inherent differences between startup employees and their counterparts at more senior firms – as reflected by the endogenous sorting – suggest that startup acquisitions could generate substantial mismatches between the two organizations.

Qualitative Evidence

In my interviews with several founders and early employees of startups that were eventually acquired, many of the respondents discussed the stark contrast between their original startup employer and the acquirer. A central theme emerged across the many acquisition experiences: because the acquirer is typically an older firm, its level of bureaucracy is antithetical to the common entrepreneurial emphasis on speedy execution and learning through experimentation (Ries 2011). A startup founder, reflecting on why he begrudgingly left the acquirer in just a year, corroborated the cultural disparity following the acquisition:

> It's really hard for an entrepreneur – a high-risk, high-speed type of individual – to settle into a methodical, decision-driven culture of meetings… and the slowness of a big company. (Interviewed on February 8^{th}, 2018)

Even for large tech companies that aim to preserve and emphasize an entrepreneurial culture (e.g., Google and Amazon), the tradeoff between bureaucracy and speed was inevitable. A former early employee of a startup, who came to a large technology firm through an acquisition in 2014, remarked:

> [Acquirer]'s internal processes, language, and rules took months to learn. More importantly, incentives are inverted at big firms. Big companies are process-oriented… and with so much invested and publicized, the priority is minimizing mistakes. Small companies are results-oriented, so we swing for the fences. (Interviewed on June 24^{th}, 2016)

While many of the interviewees shared that they became frustrated with the organizational differences and promptly left the acquirer, others were less resentful of their new employer's culture. In fact, some of the acquired workers seemed to embrace the formal hierarchy as well as the job security that the acquirer provided. When asked about why some of his employees stayed while others left with him, a founder of an acquired startup commented:

> Employees who stayed behind with [Acquirer] were those who enjoyed the comfort and security… and slower pace of a large organization. Those who left – like me – are the type that wants to go make things happen fast… Another driver for leaving [Acquirer] was that, in a large organization, you no longer get to *own*

a part of the product. But in a startup, you are responsible for a big feature of the product, whose first year is dependent on you to build and make it right. (Interviewed on May 31st, 2018)

In other words, startup employees are not uniform in their preferences for work environment. While some strongly prefer an entrepreneurial environment, others may desire a more formal and hierarchical organization. Therefore, there is likely a large variation in the mismatch that results when established firms acquire a startup. This degree of organizational mismatch between the two firms involved in an acquisition may then determine the severity of turnover. This leads to *Hypothesis 2: Turnover among acquired workers is greater when there is a larger organizational mismatch between the target and acquiring firms.*

2.4 Competitive Spawning

Consistent with the preference mismatch theory developed above, prior studies document the impact of M&A on increased turnover among the target firm's executives (Walsh 1988; Cannella and Hambrick 1993; Wulf and Singh 2011). Generally, employee exits – especially among key members like executives – are costly because of the loss of firm-specific intangibles such as knowledge (Castanias and Helfat 1991) and routines (Wezel, Cattani, and Pennings 2006).

However, the strategic cost of turnover – beyond the loss of human capital – is dependent on the *destinations* of the departures. While some departing employees could switch into unrelated industries, others may join competitor or complementary firms. Therefore, the strategic implications of employee exits are likely shaped by which firms receive these workers. For example, using mobility patterns of patent attorneys, Somaya et al. (2008) document that employee exits to competitors are detrimental to the source firm's performance. However, the study also shows that departures to complementary firms (e.g., clients) lead to higher sales for the source firm as a result of the new social ties that are formed.

An alternative career path after leaving a firm is a starting a new venture. A growing literature on employee entrepreneurship illustrates that many workers go on to start their own firms. Many studies document the various antecedents of entrepreneurial spawning, including the social ties (Nanda and Sorensen 2010) and skills (Gompers, Lerner, and Scharfstein 2005) developed while working at the parent company.

Another principal driver of employee entrepreneurship is disagreements that occur inside organizations (Anton and Yao 1995; Klepper and Thompson 2010). Establishing the link between disagreements and spinoffs, Klepper (2007) explains that disagreements arise because the employer is unable to sufficiently recognize an employee's idea or ability. As a result, the contention leads the employee to pursue the idea outside the firm.

Similarly, acquisitions are rife with organizational disagreements for several reasons. First, the integration process in technology acquisitions is frequently discordant between the two firms (Haspeslagh and Jemison 1991; Puranam, Singh, and Zollo 2006). When deciding how to efficiently allocate resources inside the firm (Stein, 1997), new managers may create disagreements by neglecting and not committing sufficient resources to the acquired technology (Rajan and Zingales 1998; Hart and Holmstrom 2010). Moreover, the integration process in technology M&A presents a key tradeoff between coordination and autonomy: while an effective merging of the organizations requires coordination, doing so comes at the cost of autonomy for the target firm (Puranam and Srikanth 2007). The loss of autonomy could result in dissatisfaction among the new employees (Hambrick and Cannella 1993) and ultimately in voluntary departures (Ranft and Lord 2000).

Second, cultural clashes between the acquiring firm and the target firm are common (Van den Steen 2010), creating another source of organizational disagreements. This tension is likely more severe in startup acquisitions because the acquiring firm – typically an established organization – and the target startup noticeably differ in their organizational structures (Slevin and Covin 1990), cultures (Corritore 2018), and practices (Turco 2016). Taken together, the resulting organizational disagreement from the cultural clash and the integration process could frustrate the acquired employees, spurring them to leave and pursue their ideas outside the firm. This leads to Hypothesis 3: *Compared to observationally similar organic hires, acquired workers are more likely to spawn their own companies.*

Conditional on spawning, what kind of firms are departing employees likely to start? Especially in knowledge-intensive settings like legal services (Campbell et al. 2012) and high-tech industries (Burton, Sorensen, and Beckman 2002; Gompers, Lerner, and Scharfstein 2005; Howard, Boeker, and Andrus 2015), the employee-entrepreneurship literature documents substantial knowledge spillovers from the parent firm to the spawned company. In other words, startup founders commonly leverage the resources and knowledge from their prior employer including technological know-how (Franco and Filson 2006), market-related knowledge (Klepper and Sleeper 2005), network of potential suppliers and customers (Burton, Sorensen, and Beckman 2002; Gompers, Lerner, and Scharfstein 2005), and organizational routines (Phillips 2002; Wezel, Cattani, and Pennings 2006). In support of this perspective, Stuart and Sorenson (2003) document that liquidity events in the biotechnology industry – in particular, initial public offerings and cross-industry acquisitions – stimulate the entry of new firms in the same region.

Consequently, when leaving to start their own firms, employees from high-tech startup acquisitions are likely to leverage the knowledge from their prior employer. In line with knowledge spillovers, these new ventures could be disproportionately clustered in the same industry. This leads to Hypothesis 4: *Compared to observationally similar organic hires, acquired workers are more likely to spawn their own companies, especially in the same industry.*

While newly spawned companies can be complementors, they can also be competitors. For example, Campbell et al. (2012) argue that employee-entrepreneurship leads to greater competition that adversely influences the source firm's performance. This leads to Hypothesis 5: *The amount of spawning is negatively related to the acquirer's performance, especially if the new venture is in the same narrow industry.*

3 Predicting Post-Acquisition Departures

3.1 Prior Literature on Worker-Firm Match

An important line of inquiry in labor economics and organizational theory concerns the relationship between worker-firm match quality and turnover. In a seminal paper, Jovanovic (1979) presents a theoretical model in which a worker and a firm learn about the quality of their match ex-post, after the worker is hired. In this model, workers who realize that they are highly productive at the firm are retained, while those who learn that they are less productive choose to leave for another job. Similarly, O'Reilly, Chatman, and Caldwell (1991) study the fit between a person and an organization based on the interaction of individual preferences and organizational culture. As expected, they show that workers with favorable person-organization fit tend to have higher job satisfaction and lower rates of turnover.

While these influential studies clarify the relationship between fit and worker turnover, a fundamental challenge underlying these analyses is the inability to account for the initial sorting of workers into firms. Prior to being hired, individuals make a non-random choice to join a firm rather than other potential employers (Sorkin 2018). In support of this view, a vast literature on compensating differentials demonstrates that workers endogenously self-select into firms based on their personal preferences for both monetary and non-pecuniary features of the employer (c.f., Rosen 1987; Stern 2004; Sorkin 2018). Therefore, the link between person-organization fit and turnover is difficult to interpret because both are shaped by the initial sorting of workers into firms.

Thus, the ideal empirical design to identify the impact of fit on turnover is to randomly allocate workers to firms and subsequently observe worker retention patterns. However, it is unrealistic to randomly assign real workers into firms. A notable exception is a study of an Indian technology firm that randomly assigns its entry-level employees to one of its eight locations (Choudhury and Kwon 2018). The authors find that distance between workplace and home has a negative causal impact on long-term worker productivity. However, randomization in this setting occurs within the firm, rather than across firms. Accordingly, a suitable approach to investigate the impact of worker-firm match quality on employee turnover requires an exogenous source of variation in "fit."

In that vein, startup acquisitions provide an empirical setting in which the target firm's employees firm are quasi-randomly assigned to a new employer. As discussed in Section 2.3, the premise turns on the lack of agency that these individuals have in choosing their next employer – contrary to traditional hires who voluntarily choose to join their new employer. This is especially true for the target startup's non-founding members. While the entrepreneurship literature has traditionally focused on founders with little attention paid to "joiners" (Kim 2018; Roach and Sauermann 2015) commonly due to data limitations, the key benefit of employee-employer data from the US Census is the ability to observe the population of workers that earn wages from startup companies – ranging from the founders to early joiners to late joiners. This study is primarily focused on these rank-and-file startup workers whose experience of an acquisition resembles an "exogenous" organizational change. Therefore, founders and early joiners – or the "founding team" – are excluded from the final sample.

It is worth noting that since the change in ownership via acquisitions occurs at the firm-level, the continuing discussion on "fit" regards that between organizations (i.e., target and acquiring firms) rather than between a worker and firm. This is the appropriate level of analysis in the acquisition context because the acquiring firm's decision-making is principally about the (extensive) margin of whether it ought to acquire a particular company, and less about the (intensive) margin of which specific individuals should be retained upon an acquisition.

The main challenge of testing the impact of organizational fit on employee turnover is the empirical measurement of the match quality between the target and acquiring firms. Following the theoretical development in Section 3.1, I define a startup acquisition's organizational mismatch to be high when the target firm tends to attract individuals who have a strong affinity for startups rather than established firms, especially when the buyer has a low affinity for startups. By adopting the methodological framework from Sorkin (2018), I construct a novel measure to characterize the target firm's level of *startup affinity*.

3.2 Measuring Startup Affinity

A fundamental problem in assessing organizational fit is that it can only be observed and measured ex-post – once the match between two parties is realized. As a result, any attempt to understand the impact of organizational fit on M&A performance, including employee retention, is inherently a post-mortem analysis – with no room for pre-diagnosis. This is perhaps why practitioners widely recognize organizational mismatch (e.g., culture clash) as the leading issue in M&A, and yet the various attempts in the M&A literature to measure and diagnose this phenomenon have been elusive.[34,35] To overcome this issue, I empirically characterize both the target and buyer firms based on information prior to the acquisition. This allows me to construct an ex-ante measure of *organizational mismatch*, and thereby predict post-acquisition retention outcomes.

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[34] According to the report "A McKinsey perspective on the opportunities and challenges" by McKinsey & Company in June 2010, 92% of surveyed executives claim that their M&A deals "would have substantially benefited from a cultural understanding prior to the merger."

[35] See Bouwman (2013) for a literature review on measuring culture clash in M&A.

To do so, I leverage each firm's regular turnover events – in particular, employer-to-employer flows – using a revealed preference argument.[36,37] More specifically, I track the departures of employees *prior* to the acquisition along with their destinations. A departing employee's intentional choice to join a startup, rather than an incumbent firm, reveals her preferences for employment conditions (Sorkin, 2018). When aggregated up, these mobility choices characterize the firm's tendency to attract workers who prefer to transition to startups rather than established firms. While these former employees do not directly influence the behavior of the acquired employees because they are not present at the time of the acquisition, their decisions to join a young firm or an established company provide useful information for peer-based prediction of post-acquisition retention outcomes.

Three core ideas serve as the building blocks for using peer turnover patterns to predict post-acquisition worker retention. First, job transitions are not random: they are intentional choices that reveal workers' preferences for employers. Simply put, a worker's decision to join a particular firm – and thereby *not* join another employer – demonstrates her relative value of the two firms based on both pecuniary and non-pecuniary factors (Sorkin 2018).

Second, pre-acquisition turnover activity is an ex-ante characteristic of the target and acquiring firms. This is an essential component of a prediction method to ensure that the predictor and outcome variables are not simultaneously determined. Therefore, this predictor could be utilized during the due diligence process before the acquisition deal is materialized.

Third, job transitions by former colleagues are relevant because organizations tend to attract similar individuals. Since both the acquired and former employees initially selected into joining a particular organization rather than other potential employers, these workers likely exhibit similar preferences for employment. Following this logic, I define the target and acquiring firms to have a strong affinity for startups if their former employees – who leave prior to the acquisition – systematically tend to move to other young companies.

To construct this measure, I track the departures of each firm's employees *prior* to the acquisitions along with their destinations. Using the LEHD to track their career histories, I find roughly 1 million employer-to-employer transitions – before the acquisition – among the

[36] Haltiwanger et al. (2012) show that turnover is commonly high at young firms (1-10 years old): in a given quarter, roughly 25% of the workforce separates from their young employer, while separations at older firms is around 15%.

[37] In my data, the average target firm exhibits 160 employee separations in its lifetime prior to the acquisition.

employees of the target firms. Similarly, I find 78 million pre-acquisition departures for the buyers. These former colleagues do not directly influence the behavior of the acquired employees because they are not present at the time of the acquisition. Thus, their decisions to join a young firm or an established company are not directly tied to the later acquired workers' choice of departing or staying with the buyer firm. Mechanically, I aggregate all of the pre-acquisition mobility decisions, and then calculate the share of transitions to other startups versus old firms.[38] The resulting firm-level shares characterize each target and acquiring firm's affinity for entrepreneurial organizations.

Figure 3 illustrates the distribution of the firms' pre-acquisition share of departures to startups (henceforth "Startup Affinity Score"). The blue kernel density curve for the target firms suggests that, even after selecting on the relatively homogenous group of young high-tech firms, there is a significant variation in the share of departures to young vs. old employers. In other words, not all startups are entrepreneurial: While some targets exhibit a strong affinity for startups, others show weak affinity for startups. This is the key variation that generates high versus low organizational mismatch in startup acquisitions.

Moreover, the distribution of the acquiring firms is also shown to provide a benchmark for older firms and their share of employee departures to startups. Relative to that of the target firms, the curve for the acquiring firms is shifted to the left. This shows that that workers at the acquiring firms tend to flow to other established firms. Therefore, *Startup Affinity Score* is expectedly correlated with firm maturity, reflecting the systematic and persistent endogenous sorting of workers into nascent versus old firms.[39] In the subsequent analysis, I exploit the differences in these two distributions to identify cases of high organizational mismatch – namely, when the acquiring firm has a lower *Startup Affinity Score* than the target firm.

[Insert Figure 3 here]

Lastly, I similarly compute *Startup Affinity Score* for the set of organically hired workers based on their prior employers. This is possible because, as later described in Section 4.4, the control group is restricted to individuals with some labor market experience prior to joining the acquiring firm. Such condition makes the two groups comparable, as all acquired workers

[38] For this measure, a startup is defined firms that are younger than five years old. All results are consistent when defining startups as younger than ten years old.

[39] Despite that generally negative relationship between firm age and *Startup Affinity Score*, I test and show in the Appendix that this measure among the acquired startups is not solely driven by the target firm's size and age.

possess work experience at the target firm prior to being acquired by another employer. Therefore, each worker has a prior employer, whose *Startup Affinity Score* can be determined.[40]

4 Data and Measurement

For this study, I use employee-employer matched data from the US Census Bureau to build a large sample of high-tech startup companies – and their non-founding employees – that are acquired between 1990 and 2011. Along with the acquired workers, I also identify the employees who join the acquiring firm as organic hires in the same year as the acquisition. This approach ensures that all employees are new to the firm, meaning that tenure at the firm is fixed to zero for both groups of workers. In addition, to make sure that the differences in retention outcomes are not endogenously driven by worker characteristics, I use a matching algorithm to find observationally equivalent organic hires for each acquired employee. Then I compare the mobility decision of acquired workers and organic hires in the first, second, and third year following the year of joining. The following section provides a detailed description of the construction of firm- and individual-level data, and the resulting final sample.

4.1 Identifying High-Tech Startups

While M&A activity covers many industries and different types of firms, this study focuses on high-tech startup targets for several reasons. First, in order to examine a setting where human capital – more so than the tangible assets such as land and machinery – is a key asset to acquire, I restrict the sample of acquisition targets to startups. Startups are defined as firms that are younger than ten years old, where the firm's birth year is the year when the first employee is hired.

Second, I focus on the high-tech sector in order to differentiate small businesses from high-growth startups. While many researchers and practitioners alike broadly use the term entrepreneurship, there are different forms of entrepreneurship. Most notably, small businesses and growth-oriented startups are two distinct types of entrepreneurship albeit both tend to consist

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[40] For brevity, the empirical distribution of *Startup Affinity Score* among the control group's prior employers it not shown. Nonetheless, it closely resembles the distribution of the acquiring firms.

of young firms. On the one hand, high-growth startups are a small subset of new firms that quickly scale and account for a disproportionately high share of job (Decker et al. 2014; Guzman and Stern 2016). On the other hand, small businesses tend to remain small because they typically do not have a desire to grow large or innovate in a meaningful way (Hurst and Pugsley 2011).

In the same way, acquisitions of young firms include both high-growth startups and small businesses. According to the M&A database constructed for this study (as further described in Section 3.2), acquisitions – whereby one firm is subsumed under the ownership of another existing firm – among young firms occur predominantly in the small business sector, most frequently restaurants and dentist offices. As Figure 1 shows, roughly 85% of startup acquisitions take place in non-high tech industries. Therefore, given that the prevailing view on startup acquisitions concerns high-growth ventures, it is critical to distinguish the two forms of entrepreneurship in this study.

To differentiate between high-growth startups and small businesses, many studies in the entrepreneurship literature limit their study to venture capital-backed startups or young firms that are granted a patent (Azoulay et al. 2018). Since venture capital financing and patenting are early firm outcomes that reflect the firm's underlying quality – rather than innate traits of the firm – this study does not use these markers in order to avoid selecting on firm quality.

Instead, I attempt to focus on high-growth startups by restricting the sample to high-tech startups. This approach has several advantages. First, the categorization of high-tech versus non-high-tech is a time-invariant measure that is determined at the time of the establishment's birth. Second, high-tech industries are objectively defined by the Bureau of Labor Statistics as the set of NAICS-4 industries with the highest share of STEM-oriented workers. Accordingly, I follow Hecker (2005) and Goldschlag and Miranda (2016) to define the high-tech sector (See Table A1 in the Appendix for a complete list). While I impose the high-tech condition on the target startup firms, buyers can operate in any industry.

4.2 Firm Characteristics

The Longitudinal Business Database (LBD) is the primary firm-level dataset in this study. The LBD is a panel dataset of all establishments in the U.S. with at least one paid employee. The LBD covers all industries in the private non-farm economy and every state in the US. The LBD begins in 1976 and currently runs through 2015. While the underlying observations are at the level of the establishment, the LBD assigns a unique firm identifier to each establishment. This is a useful feature especially for firms with multiple establishments. Furthermore, the longitudinal nature of the LBD allows researchers to identify the birth of startup companies and track important business characteristics including firm age, employment, payroll, and exit.

More importantly, I identify acquisitions in the LBD based on firm ownership changes. The main benefit of relying on the LBD for detecting M&A activity is the systematic coverage of young, private firms, for which standard M&A databases (e.g., SDC Thomson) are known to be limited in coverage. When a firm undergoes an acquisition, its firm-identifier changes to that of the surviving (parent) firm in the following year. I construct a set of firms that experience such change. In order to exclude non-M&A-based changes to firm ownership (e.g., false positives) such as divestitures and corporate restructuring, I leverage the pre-acquisition establishment-level name and EIN information to carefully validate the detected cases of acquisitions. In short, I rule out cases in which (1) the ex-ante names of the acquired and acquiring establishments are highly similar and (2) EINs do not change. Consequently, I build a comprehensive database of firm acquisitions in the LBD between 1985 and 2015. See Figure 1 for trends in startup acquisitions over time.

[Insert Figure 1 here]

In addition, I use the Longitudinal Linked Patent-Business Database (See Graham et al. 2018) to measure whether the target firm owns (or has applied for) a patent prior to the acquisition year. This allows me to distinguish patent-owning from non-patenting target firms.

4.3 Worker Characteristics

Worker-level information is based on the Longitudinal Employer-Household Dynamics (LEHD), which is an employee-employer matched dataset that covers 95% of private sector jobs. The study uses the full available version of the LEHD, which includes all US states except Massachusetts. The current LEHD time coverage spans from 1985 to 2014, although most states are not available before 2000 (See Figure 2 for a map of included states and their earliest year of coverage).[41] The LEHD tracks individuals at a quarterly basis and provides information on earnings, linked employer identifier, and demographic characteristics (e.g., age and gender).

[41] States vary in their first time of entry in the LEHD data. The earliest entrant is Maryland in 1985Q2. Most states enter the data by 2000. See Vilhuber (2018) for a detailed description of the LEHD.

These quarterly worker-firm observations allow me to precisely determine whether and when acquired workers transition to the acquiring firm as well as their post-acquisition mobility decisions. Employers in the LEHD are observed at the state EIN level. I merge the LEHD to the LBD using the crosswalk developed by Haltiwanger et al. (2014).

I use the earnings and join date information in the LEHD to categorize startup employees as founders, early joiners, or late joiners. Similar to Kerr and Kerr (2017) and Azoulay et al. (2018), I define founders as employees who join the firm in the first quarter of operation and are among the top three earners during the firm's first year. Relatedly, early joiners are those who join the firm in the first quarter but are not among the top three earners. Lastly, late joiners are those who join the firm after the first quarter. In order to focus on individuals who are unwittingly acquired, I exclude from the sample the founders and early joiners, who represent 13% of the acquired workers. Nonetheless, all results are consistent when including the founding team in the analyses

One limitation of the worker-level data is that the LEHD does not distinguish voluntary from involuntary turnover. While this study puts forth a narrative around voluntary departures driven by worker choice, many employees at the target firm may simply be fired. Unfortunately, the data do not allow for careful distinction between the two types of departures. However, to mitigate this potential issue, I take two concrete steps in the analysis. First, I restrict my sample of acquired workers to those who work for the buyer at least two quarters, meaning that they initially receive job offers for employment at the acquirer. That is, these workers are not outright dismissed upon the acquisition. The "never-joiners" comprise roughly 10% of the sample of acquired workers, and are removed in the main analyses. Second, I check whether acquired workers who leave are systematically more likely to enter into unemployment relative to regular joiners who leave. The intuition is that higher unemployment rates among acquired workers would validate the concern around involuntary dismissals. Fortunately, the two groups do not appear to show major differences in the propensity for unemployment upon leaving the acquirer.

4.4 Analytic Sample

Beginning with the full set of acquisitions in the LBD, I identify roughly 6,000 cases in which high-tech startups are acquired. After matching to the LEHD and restricting to years between 1990 and 2011 to allow for at least three years of observation following the acquisition, the sample is reduced to 3,700 acquired startups.[42]

At the worker level, there are 300,000 non-founding employees from the target firms who are acquired and transition to the buyer, along with several million workers who are conventionally hired at the buyer firm in the same year as the acquisition. For comparability, I exclude regular hires for whom this is their first job. By construction, acquired workers are experienced workers given their tenure at the target firm prior to the acquisition. By restricting the set to having some prior experience, this provides a comparable set of 5.3 million regular hires.

To ensure that the differences in retention outcomes are not driven by unobserved characteristics such as worker quality or seniority, each acquired worker is matched, using Coarsened Exact Matching (Iacus, King, and Porro 2012), to an observationally equivalent organic hire who joins the same buyer firm during the acquisition year. While worker roles are not observed in the LEHD, I use detailed worker characteristics – namely earnings, age, and gender in the year prior to the acquisition – to adjust for inherent differences in human capital between the two groups.[43] By conditioning the acquisition year to be the join year for organic hires, tenure at firm is mechanically set to zero for both the acquired workers and organic hires. Therefore, differences in retention outcomes in this study are not driven by differences in tenure. The final sample includes 3,700 startup acquisitions, 230,000 acquired workers, and 1.6 million regular hires. Tables 1A and 1B present the summary statistics of the final sample's firms (both the target and buyer) and their employees.

[Insert Tables 1A and 1B here]

4.5 Main Variables

Dependent Variables

[42] Several factors contribute to the reduction in sample size when matching LBD firms to the LEHD. First, because of the imperfect EIN-based matching between the two data sources, roughly 30% of the firms in the LBD are not found in the LBD-LEHD crosswalk. Second, Massachusetts is not included in the LEHD, meaning that the identified firm-level acquisition is dropped from the sample if the target or the acquiring firm is based in Massachusetts.

[43] In order to avoid partial annual earnings, I use "full quarter earnings" which are calculated as the wages in a quarter for which the person receives non-zero wages from the preceding and subsequent quarters.

The main dependent variables in this study are worker-level retention outcomes. *$Depart_{ijt}$* is a binary outcome equal to 1 if worker i is no longer employed at the acquiring firm j in year t since the acquisition. The variable remains as 0 if the worker is employed at the firm for any amount of time during the year of interest. For example, if a worker acquired in 2005 leaves the firm in 2006, then the $Depart_{ij1}$ would equal 0 while $Depart_{ij2}$ would equal 1.

Similarly, $Spawn_{ijt}$ is a binary outcome equal to 1 if worker i in acquiring firm j is a founder of a new firm born by year t (See Section 3.2 for definition of founders). Similarly, $Related\_Spawn_{ijt}$ is a binary outcome equal to 1 if worker i is a founder of a new firm – residing in the same 2-digit NAICs industry as the original target firm – born by year t.

To measure acquirers' post-acquisition performance, I use employment- and revenues-based growth measures. The growth measures are based on a three-year window where the initial year is the year of the acquisition.[44] The growth rate between year t and $t+3$ is calculated as $\frac{Y_{jt+3}-Y_{jt}}{(Y_{jt+3}+Y_{jt})/2}$, where Y_{jt} is acquirer j's employment or revenues in year t. This is a standard measure in the firm dynamics literature – known as the Davis-Haltiwanger-Schuh (DHS) growth rate (Davis et al. 1996) – that weights the rate of growth by firm size. In doing this, this measure minimizes the naturally negative relationship between initial size and growth.

Independent Variable

At the worker-level, the primary independent variable is $Acquired_{ij}$, which is a dummy variable equal to 1 if worker i in acquiring firm j is hired through a startup acquisition, and 0 if the worker is organically hired. For each acquisition, *Number of Spawned Companies* is a firm-level count of startups spawned by the acquired workers within three years since the acquisition.

5 Empirical Results

5.1 Econometric Framework

The main results in this study are based on a series of linear worker-level regressions. These regressions are a variation of the following simple econometric framework with worker i in buyer firm j:

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[44] Robustness checks using a 5-year window are available.

$$Y_{ij} = \beta_0 + \beta_1 Acquired_{ij} + \delta_j + \varepsilon_{ij} \quad (1)$$

Y_{ij} is a set of binary outcome variables including departing from firm j by year k since the acquisition, where $k \in \{1,2,3\}$. Other dependent variables – namely, spawning and related spawning in year k – are similarly constructed as binary outcomes. Furthermore, δ_j is a suite of target-buyer firm fixed effects, meaning that all firm-specific traits including industry, geography, and year of the acquisition are subsumed by these parameters. In other words, workers who are acquired by firm j are solely compared to those who join firm j as organic hires during the same year as the acquisition.

It is important to note why linear (ordinary least squares) regression models are used instead of non-linear models (e.g., probit, logit) given that the dependent variables are binary outcomes. While probit and logit models have the benefit of bounding the estimates between 0 and 1, the resulting estimates may be biased due to the incidental parameters problem. Unlike linear regressions which provide the best linear approximation to the conditional expectation function, logit and probit models may produce biased estimates as the number of parameters grows relative to the number of observations.[45] This issue may be particularly problematic when including many fixed effects in the regression.

Firm fixed effects δ_j in Equation (1) are crucial in this empirical design because they allow β_1 to be interpreted as within-firm effects. In other words, estimates of β_1 identify the effect of being acquired versus hired on the worker's likelihood of exiting the firm, after accounting for firm-specific effects including region, industry, and join year. Therefore, the inclusion of δ_j mitigates the endogeneity concerns that would otherwise arise when comparing across firms, stemming from both observable and unobservable differences. Given the importance of firm fixed effects as the identification strategy in this empirical framework, this study uses a linear probability model in order to avoid the incidental parameters problem.

5.2 Post-Acquisition Employee Departures

Figure 4 shows the unconditional rates of employee retention for acquired workers versus organic hires. Since the set of acquired workers in the sample are those who work for the buyer

[45] See Angrist and Pischke (2009) for a detailed discussion on limited dependent variables (e.g., binary), non-linear models, and the incidental parameter problem.

for at least two quarters, retention rates are mechanically set to 100% in the year of the acquisition. In the following years, acquired workers noticeably exhibit lower retention rates. While 88% of the regular joiners are retained by the year after the join (acquisition) year, the rate for acquired workers is 66%. However, the stark differences in retention rates appear to wane over time.

[Insert Figure 4 here]

In parallel to Figure 4, Table 2 presents the linear probability regression estimates on employee retention, accounting for individual and firm characteristics. The dependent variable is a binary indicator that equals 1 if the employee leaves the acquiring firm by year *k*. All specification include target-acquirer firm fixed effects. While the first three specifications include all workers, the latter three specifications include only the workers that are closely matched in earnings, age, and gender. As a result, acquired workers and traditional hires in the matched specifications are observationally equivalent with regards to key human capital characteristics. Nonetheless, results are consistent with and without matching, suggesting that retention outcomes are not explained by innate individual characteristics.

Overall, all specifications indicate that acquired workers are significantly more likely to leave the acquirer. The effect ranges from 8 to 22 percentage points and is statistically significant at the 1% level. While only 12% of the comparable regular hires leave the firm in the first year after the acquisition, 34% of the acquired workers leave in the same time period. In a three-year window, acquired workers are approximately 15% more likely to leave the firm relative to regular hires. Therefore, even after controlling for important worker traits such as earnings and age, acquired workers exhibit greater turnover relative to organic hires.

[Insert Table 2 here]

It is important to note that the differences in retention between the groups become much smaller over time. This is consistent with the view that the elevated rates of turnover among acquired workers is largely driven by the underlying worker-firm match quality. Following the the Jovanovic (1979) model of worker tenure and turnover, acquired workers who learn that they are a good match tend to stay with their new employer. Consequently, rates of employee exits among the two groups appear to converge over time. Taken together, these results imply that new employees learn about the quality of their match with the firm relatively quickly, as reflected by the large share of employee outflows in the first year of employment.

5.3 Mechanism: Organizational Mismatch

In this section, I investigate organizational mismatch as the mechanism that explains the greater turnover among acquired employees in comparison to regular joiners. I test this hypothesis in two ways. First, I assess whether target firms with greater affinity for startups exhibit greater rates of employee departures relative to those with lower affinity for startups. Since acquiring firms tend to be larger and older than targets, strong affinity for startups implies a greater degree of organizational mismatch between the target firm and the buyer. Second, I examine whether the effects are stronger when the buyer has a low affinity for startups – a situation in which organizational mismatch is likely to be especially pronounced. To test these predictions, I use the firm-level measure *Startup Affinity Score*, which is separately measured for each target and buyer, as developed in Section 3.2. As a robustness test to document that this measure is generally uncorrelated with non-acquired workers' retention patterns, it is also calculated for the prior employers of the regular hires.

Panel A in Figure 5 depicts the 3-year employee departure rates by the prior employer's *Startup Affinity Score* quartiles, where Q1 represents the set with the lowest affinity for startups.[46] For the prior employers of the acquired workers – the target firms – the rate of employee exits are increasing in *Startup Affinity Score*. In other words, target firms that demonstrate higher shares of pre-acquisition departures to startups are more likely to exhibit elevated rates of post-acquisition turnover. In contrast, this pattern does not hold for the prior employers of regular hires, whose departure outcomes are unrelated to *Startup Affinity Score*. Therefore, this difference suggests that organizational mismatch heightens employee turnover only when the workers do not exercise agency in choosing their next employer.

Panel B demonstrates an even stronger relationship between *Startup Affinity Score* and employees leaving to start their own firms. While target firms with the lowest Startup Affinity Score (Q1) exhibit a spawning rate of merely 0.6%, the rate for those in the highest quartile is 1.6%. Again, the positive correlation between *Startup Affinity Score* and spawning rates does not hold for the regular hires' prior employers. All results are consistent when examining related spawning as the employee exit outcome.

[46] Quartile threshold values are determined based on the target firms. For consistency, the same cutoffs are imposed on the control group, meaning that the quartile bins among the control group does not necessarily contain equal number of observations.

[Insert Figure 5 here]

Table 3 is the regression counterpart to Figure 5. This set of regressions is identical to that in Table 2, but with interaction terms between the acquired worker dummy and the target firm's *Startup Affinity Score* quartiles. *Startup Affinity Score* quartiles are independently included to control for their impact on employee retention for the control group of regular hires. This term can be estimated since *Startup Affinity Score* is separately measured for the control group based on their prior employers. Furthermore, the key omitted group is *Acquired x Startup Affinity Score[Q1]*. Therefore, the regression estimates corresponding to the interaction terms indicate the marginal effect relative to the omitted group.

[Insert Table 3 here]

Consistent with the patterns in Figure 5, the regression estimates for *Startup Affinity Score* quartiles alone – which correspond to the control group – are generally insignificant. In other words, *Startup Affinity Score* is not systematically related to the regular joiners' retention patterns. Although the estimates are statistically significant in the first column given the large sample size, the economic magnitudes are small, translating to a 0.7 percentage point premium for the control group with the highest Startup Affinity Score (Q4). Therefore, as expected, *Startup Affinity Score* does not predict the retention rates of the regular hires.

In contrast, the row corresponding to the acquired workers interacted with the highest *Startup Affinity Score* (Q4) demonstrates the highest rate of turnover. Relative to the acquired workers in the lowest quartile, workers in this category are roughly 12 percentage points more likely to leave the acquirer within three years. Consistent with the trends in Figure 5, the subsequent rows are also positive, albeit smaller in magnitude. While some estimates are statistically indistinguishable from zero, the highest quartiles (Q3 and Q4) are always positive and significant, implying that target firms with the greatest affinity for startups demonstrate the highest rates of employee separations.

In principle, organizational mismatch is determined by not only the characteristic of the target firm, but also that of the acquiring firm. Accordingly, I test whether the effects vary by the acquiring firm's *Startup Affinity Score*. Organizational mismatch is likely more severe when an entrepreneurial target firm is purchased by a less entrepreneurial organization. To test this premise, I re-run the analysis from Table 3 by splitting the sample into high vs. low organizational mismatch: Organizational mismatch is defined as high (low) if the target firm's *Startup Affinity Score* is greater than (less than or equal to) the buyer's *Startup Affinity Score*.

[Insert Table 4 here]

It is first important to note that the subsample of high organizational mismatch is roughly twice as large as the subsample of low organizational mismatch. In other words, high-tech startup acquisitions are generally cases of a high organizational mismatch. This is a sensible pattern given that acquiring firms tend to be significantly older and larger than the targets.

When organizational mismatch is high (Specifications 1-3), target firms with higher *Startup Affinity Score* exhibit much greater rates of employee turnover. Moreover, when using longer time windows of two or three years, the main effect (e.g., difference in employee departures between acquired workers and regular joiners) is statistically insignificant for target firms with the lowest *Startup Affinity Score* (Q1). This means that when the target firm has a low affinity for startups, the acquired workers and regular joiners are statistically equally likely to be retained. However, the departure effects are strongly positive and significant when – hence entirely driven by – the target firms with a higher *Startup Affinity Score* (Q2-Q4). In other words, when there is a substantial organizational mismatch between the acquired and acquiring firms, the target firm's affinity for startup systematically explains the rate of post-acquisition employee exits.

In contrast, Specifications 4-6 show that the target firm's *Startup Affinity Score* has a null effect on explaining employee departures when the acquiring firm has a comparable or higher *Startup Affinity Score*. While the main effect on acquired workers (relative to regular hires) is positive and significant, interaction effects with the target's *Startup Affinity Score* are statistically insignificant from zero. Therefore, when organizational mismatch is low, the target firm's affinity for startups does not account for the variation in employee exits. Taken together, these results validate Hypothesis 2 and empirically support the role of organizational mismatch in explaining post-acquisition employee retention patterns.

5.4 Post-Acquisition Employee Spawning

Next, I test the hypothesis that acquired workers are not only more likely to leave, but also more likely to start their own companies upon leaving. Similar to Section 4.2, I empirically test this prediction using a series of cross-sectional regressions with a binary dependent variable

$Spawn_{ijk}$ that equals one if the worker i in acquiring firm j is a founder of a new firm by year k following the acquisition. Specifications 1-3 in Table 5 exhibit the resulting regression estimates.

[Insert Table 5 here]

As in the case of employee departures, acquired workers are roughly 0.3 percentage points more likely to launch their own ventures relative to organic hires. In all specifications, this effect is positive and statistically significant at the 1% level. Although employee spawning is a rare outcome[47], meaning that the absolute size of the effect is seemingly small, the economic magnitudes are substantial in relative terms. Compared to regular hires, acquired workers are 80% more likely to enter into entrepreneurship within two years of being acquired. Thus, Hypothesis 3 is empirically supported.

Unlike the results on employee departures, the relative effects on employee spawning do not decline over time. This is likely due to the lag between leaving an employer and starting one's own company, especially since firm birth is measured as the year that the first employee is hired. For example, an acquired worker who realizes a poor match with his employer may leave in the first year, subsequently gather the necessary resources to start his own firm, and eventually hire the first employee the second or third year following the acquisition. Therefore, it is not surprising that the impact of acquisitions on employee spawning – unlike that on employee departures – does not monotonically decline over time.

Moreover, I investigate whether the spawning effect demonstrates knowledge flows from the original target firm to the new venture. In other words, I test whether entrepreneurial entry following startup acquisitions disproportionately occur in the same industry as the target firm. Same-industry spawning would reflect knowledge flows from the original target to the new venture. Accordingly, results on related spawning are shown in Specifications 4-6, where the dependent variable is a series of binary outcomes equalling one if the worker founds a new firm in the same industry – defined at the two-digit NAICS level – as the target firm.[48]

The estimates range from .08 to .23 percentage points in the first and third year following the acquisition, respectively, and are all significant at the 1% level. Considering that related spawning is a very rare outcome, the economic magnitudes are substantial: By the second year following the acquisition, acquired workers are roughly 100% more likely to launch a company

[47] About 0.3% of the acquired workers spawn their own company within three years of the acquisition.
[48] Results are robust to using four-digit NAICS classification when defining same industry.

in the same industry relative to organic hires. Therefore, Hypothesis 4 is confirmed: acquired workers are not only more likely to become entrepreneurs, but also more likely to start companies in the same space as their former employer.

5.5 Heterogeneity by Patents and Non-Compete Enforceability

Having established that acquired workers are more likely to exit as well as transition to entrepreneurship relative to traditional hires, I next assess the heterogeneity in the effects (1) by whether the target firm possesses a patent, and (2) by the underlying region's degree of non-compete enforceability.

First, I examine how the results vary by whether the target startup owns a patent. On the one hand, employee departures could be higher among patent-owning targets if the acquirer is primarily interested in buying the patent and therefore less inclined to retain the workers. In support of this argument, Cunningham et al. 2017 find in the pharmaceutical setting that only 22% of the inventors from the target firm are retained following an acquisition. On the other hand, employee departures may be lower with patent-owning target firms if the workers are complements to the acquired knowledge. In this case, the acquirer is less likely to dismiss the target employees especially if it decides to preserve and commercialize the purchased patent (Gambardella, Ganco, and Honoré 2014). To test these predictions, I use the Longitudinal Linked Patent-Business Database (Graham et al. 2018), and define a target firm to be patent-owning if has applied for or been granted a patent prior to the acquisition year.

Table 6 presents the heterogeneous effects by target firm's patenting. The outcome variables include employee departures, employee spawning, and employee spawning in the same in industry. For brevity, only three-year outcomes are reported; results are consistent with using one- and two-year windows.

[Insert Table 6 here]

The first two specifications show the results on employee departures by the third year of the acquisition. Specification 1 subsets on workers from patent-owning target firms, while Specification 2 consists of only those in non-patenting target firms. Overall, the higher rates of employee exits among acquired workers consistently remain positive and statistically significant. More specifically, the departure effect – as well as the employee spawning effect – is marginally lower among patent-owning targets. Thus, these results reject the view that the acquirer is likely to dismiss workers en masse after purchasing a target firm and its intellectual property. Though the differences are small, the results are more consistent with the view that workers and patents are complements, leading to higher retention of workers when the underlying target startup owns a patent.

Interestingly, Specifications 5 and 6 show a large difference in the impact on related spawning. When the target firm owns a patent, the propensity of acquired workers to launch a new company in the same original industry is significantly lower. This finding reflects the role of appropriability in knowledge production (Teece 1986; Gans and Stern 2003): When the acquirer purchases a patent through an acquisition, the exiting employees are less inclined to start new businesses within the boundaries of the intellectual property. Therefore, transactions in the markets for technology shapes not only the buyer's technological trajectory (Arora, Fosfuri, and Gambardella 2001), but also that of former employees who seek to start new ventures.

Next, I test whether the effect varies by how enforceable non-competes are in the underlying state. Given that acquired employees are typically required to sign new employment contracts with the buyer, I base the degree of non-competes enforceability on the acquiring firm's state laws.[49] To measure the degree of enforceability, I use each state's non-compete enforceability scores computed by Starr (2018). I code the acquisitions as operating in High (Low) non-compete enforceability regimes for states above (below) the median score.

[Insert Table 7 here]

Specification 1 includes only workers whose acquiring firms are located in the states with high non-compete enforceability, while Specification 2 uses the remaining states with low non-compete enforceability. Relative to the original results in Table 2, the point estimates are roughly unchanged in both the direction and magnitude. This is not surprising since the original estimates are within-firm effects, meaning that both the acquired workers and regular hires are subject to the same degree of non-compete enforceability. Therefore, while the levels may shift depending on the intensity of non-compete enforceability, the relative difference between the two groups appears to remain stable. Although the differences are small, the departure and spawning effects among the acquired workers are slightly larger when acquired by a firm in a state with low non-

[49] Interviews with lawyers in Massachusetts and California confirmed that acquiring firms generally require the target workers to sign new employment contracts that are enforced under the employer's "law of choice", which is typically based on the acquirer's state.

compete enforceability. This moderating effect of non-compete enforceability is consistent with Stuart and Sorenson's (2003) analysis of the biotech sector. However, in terms of related spawning, the effect is slightly larger when non-compete enforceability is high. Nonetheless, the enforceability of non-compete agreements does not appear to significantly influence whether acquired workers or traditional hires are more likely to exit or spawn new firms.

5.6 Firm Performance and Competitive Spawning

How does post-acquisition spawning, especially when occurring in the same industry, affect the acquirer's long-run firm performance? While acquisition-induced entrants born in the same industry can be complementary firms that provide network and trading benefits to the source firm, they can also be new competitors. I analyze the impact of new ventures founded by acquired employees by turning to the following firm-level regressions for acquiring firm *j* in industry k, state *s*, year of acquisition *t*:

$$Growth_{jt+3} = \alpha_0 + \alpha_1 Count\_Spawn_{jt+3} + \gamma_{kt} + \tau_s + \boldsymbol{X}_j'\Theta + \varepsilon_{ij} \quad (2)$$

The dependent variable $Growth_{jt+3}$ is the acquiring firm's DHS rate of growth between year *t* and *t+3*, measured in employment as well as revenues (See Section 3.4 for more on DHS growth rates). To account for industry-specific trends, which also may vary with time trends, acquisition year-industry interacted fixed effects γ_{kt} are included.[50] Since the underlying sample contains roughly 3,500 firms, the interacted year-industry fixed effects are defined at the 2-digit NAICS level in order to allow for sufficient number of observations in each of the estimated bins. Moreover, state fixed effects τ_s, defined by the location of the acquiring firm's headquarters, are included to absorb geographic trends that may affect firm performance. A vector of buyer firm traits $\mathbf{X}_j$ controls for firm age as a series of four dummy variables for each of the acquirer firm age quartiles.

[50] In the LBD, NAICS industry is defined at the establishment level. For firms with multiple establishments, I determine the firm's dominant NAICS-2 industry as the one with the highest share of the firm's employment. Within this dominant NAICS-2 industry, I again use employment shares to determine the dominant NAICS-3 industry. This process is repeated until the level of six-digit NAICS industry.

Table 8 presents the firm performance regressions. Panel A uses employment growth while Panel B uses revenue growth.[51] The first specification in both panels counts the number of companies spawned – outside the original target firm's two-digit NAICS industry – by the acquired workers by year three since the acquisition. The subsequent specifications count the number of companies spawned by the acquired workers in the same industry. The degree of industry similarity between the original target company and spawned firm becomes higher across the specifications from two- to four- to six-digit NAICS industries.

[Insert Table 8 here]

In both panels, Specification 1A shows that spawning in unrelated industry has a negative, albeit small, impact on the acquirer's performance. Relative to the acquirers that do not experience any unrelated spawning within three years of the acquisition, an entry of one unrelated spawned entrant is associated with a 1.6% lower rate of employment growth within the three-year window. These modest effects likely reflect the cost associated with general employee turnover (e.g., replacement cost) independent of competitive spawning.

However, the negative effect is substantially larger in Specification 2A that counts the number of related spawning in the same NAICS-2 industry following the acquisition. An additional company founded in the same NAICS-2 industry is linked to a 2.5% (2.0%) decrease in long-run employment (revenue) growth. Moreover, the negative impact grows even larger as the industry similarity becomes narrower.[52] For instance, as shown in Specification 4A, one spawned company in the same NAICS-6 industry, which is the most granular industry level, is associated with a 3.5% decline in employment growth. All of these results are statistically significant mostly at either the 1% or 5% levels, and the findings based on employment are strongly consistent with those using revenues.

It is worth mentioning the firm-level analyses in Table 8 is subject to endogeneity concerns, meaning that the interpretation of these results is suggestive rather than causal. For example, industry lifecycles could be a credible alternative explanation: Acquirers operating in industries in a time with heavy entry rates may both see declines in performance due to competition and experience a rate of post-acquisition spawning. To account for this possibility, Specifications 1B,

[51] Given the limited coverage of firms and their revenue information in the LBD, especially among young firms, the observation count is noticeably lower than when using employment growth.

[52] NAICS industry categorization ranges from two (broadest) to six (narrowest) digits. Narrower NAICS industries are subsets of broader NAICS industries.

2B, 3B, and 4B in Table 8 directly control for industry-year-specific entry dynamics. In particular, these specifications include a variable that counts the number of new entrants during the acquisition year *t* in the same industry, where industry is defined at the level of the corresponding column (e.g., 2-digit NAICS industry in Specifications 1B and 2B; 4-digit NAICS industry in Specification 3B). Results are strongly consistent with the earlier results of Table 8.

Despite the limitations from other potential omitted variable bias, these persistently strong results suggest a negative relationship between post-acquisition spawning among the acquired employees and firm performance. This view is corroborated by the fact that this negative correlation grows larger as the industry similarity between the entrants and the acquirer becomes more narrowly defined. Generally, these findings suggest that employee departures following an acquisition can lead to the creation of new competitors that impair the buyer's long-run performance.

Finally, I test whether the performance implications for the buyer depend on whether the target startup owns a patent. Earliest results from Table 6 show that acquired workers from patent-owning target firms are less likely to spawn new companies in the same industry, compared to their counterparts from non-patenting target firms. These findings imply that patents play an important role in keeping out entrepreneurial activity in the intellectual domain. Therefore, in theory, the competition effect from spawning should be much weaker when acquiring a startup with patents. Accordingly, Table 9 presents the results on performances implications by separating out acquisitions of patenting vs. non-patenting target startups.

[Insert Table 9 here]

Panel A represents acquisitions of patent-owning startups. Consistent with prior findings, the estimates are generally negative, implying a negative association between acquirer's long-run performance and spawning among acquired workers. However, the size of these effects are significantly smaller than those in Table 8, and almost all of the specifications are statistically insignificant. In other words, when the underlying target startup owns a patent, the impact of spawning on the buyer's performance is statistically indistinguishable from zero.

In contrast, Panel B shows a strong negative relationship between spawning and performance when the target firm does not own a patent. For both employment and revenue growth as the performance outcome, the negative estimates are noticeably larger in magnitude relative to those in Table 8. This suggests that that much of the competitive spawning occurs in cases where the acquired target lacks intellectual property. Therefore, the target firm's patenting seems to strongly influence the competitive dynamics of the acquirer and employees who choose to leave and start new businesses. Although acquired workers from patenting startups remain more likely to become entrepreneurs – and even more so in the same industry – than regular hires, the creation of new ventures appears to occur outside the boundaries of the underlying intellectual property.

6 Conclusion

While a vast literature in entrepreneurship examines the birth and growth of new enterprises in isolation, several studies have demonstrated a rich interaction between young firms and industry incumbents – whether in a competitive or cooperative context (c.f., Gans and Stern 2003). This study sheds light on a growing trend that dynamically shapes the competitive landscape between nascent and incumbent firms: Startup acquisitions. Among other factors, a common motivation behind buying out startup firms is the desire to bring in superior talent.

This paper provides the first large-scale empirical investigation on the effectiveness of startup acquisitions as a hiring strategy versus conventional hiring. The fundamental takeaway is that acquired workers are significantly less likely to be retained in comparison to traditional hires, even after accounting for worker and firm-specific traits that may influence retention outcomes.

Even more intriguing is that these departures can be largely predicted based on information before the acquisition. To show this, I leverage population-level data on career histories and construct a measure of "startup affinity" for each firm based on its pre-acquisition employment patterns. I demonstrate that this measure is systematically related to the degree of employee turnover following an acquisition. When target startups exhibit a strong affinity for entrepreneurial (established) organizations, the resulting employee retention tends to be considerably lower (higher). In line with the proposed narrative of organizational mismatch, these departure effects are even more severe when the target is acquired by a firm with a lower affinity for startups.

In contrast to prior studies that treat match quality as an ex-post object that can be assessed only once the two parties are matched (Jovanovic 1979; O'Reilly et al. 1991), this study contributes to the literature by illustrating that organizational fit can be assessed ex-ante. This takeaway provides important managerial implications, especially for how M&A due diligence is conducted. In practice, buyers can use the prediction tool introduced in this study to pre-diagnose the likelihood of retaining a potential target company's human capital. In turn, a more informed understanding of this dimension can shape not only the decision to (not) acquire a particular organization, but also the pricing and personnel incentives underlying the deal.

At the heart of these empirical results is the lack of choice for the acquired workers: unlike regular hires who choose to join a new employer, acquired workers seldom have a choice in their employer's ownership change. Therefore, precisely because the target employees do not choose their new employer, acquisitions often create poor matches between the target workers and the acquiring firm, resulting in elevated rates of employee turnover. Taken together, the central lesson is that worker choices matter: When workers do not exercise choice amid an organizational change, they may resist the transition by electing to leave the firm.

Moreover, this study shows that departures among the acquired workers can be strategically costly. Upon leaving, acquired workers are significantly more likely to launch their own firms, which are disproportionately in the same industry as the original target firm. These new ventures appear to exert competitive pressures on the acquiring firm as reflected by the negative relationship between same-industry spawning and the buyer's post-acquisition performance. Although these results do not necessarily merit a causal interpretation, they suggest that employee turnover is costly due to not only the loss of talent, but also the potential creation of new competition.

There are several limitations to this study worth highlighting. First, the underlying data do not capture each's occupation inside the firm. In order to avoid comparing fundamentally different types of workers – for example, executives to entry-level employees – I use earnings and age to proxy for the worker's level of human capital. Nonetheless, it would be informative to clarify the nature of the work that is assigned to individual. For instance, the results on departures may differ between technical versus non-technical workers.

Second, the inability to observe employment contracts is a constraint in this study. A common view of startup acquisitions is that target employees become much wealthier upon being acquired, financially enabling these individuals to leave and pursue other career opportunities. However, liquidity effects from an acquisition greatly vary by the specific terms of the employment contract including the equity vesting schedule. Moreover, personal wealth gains from startup acquisitions tend to be heavily concentrated among the founders, with much smaller shares distributed among the non-founding employees.[53] Since this study focuses on the non-founding employees by excluding both the founders and the early joiners from the sample, it is unlikely that wealth effects are the primary driver of employee turnover among the acquired employees. Nevertheless, it would be informative to understand how much of the post-acquisition retention patterns can be accounted for by each individual's financial gains from the buyout.

This study concludes by highlighting a few areas for future research. As a first step, an insightful exercise would be to validate this study's Startup Affinity Score against other measures of organizational culture. For instance, Sull et al.'s (2019) machine learning-based scores of firm culture using Glassdoor reviews could be a useful platform.[54] To the extent that it captures the entrepreneurial culture of a firm, Startup Affinity Score likely positively corresponds to similar cultural values on Glassdoor like "agility" and "execution".

An important question for future research is how the price of startup acquisitions – which frequently surpass a billion dollar valuation in spite of the uncertainties associated with new markets and technologies – accounts for the post-acquisition retention patterns of the target workers. Put differently: What is the price of (retained) entrepreneurial talent? Although acquirers may rationally price their transactions by accurately predicting the likelihood of preserving the human capital, it could be the case that acquirers systematically overpay in light of the markedly high turnover documented in this study.

In addition, future research may address the policy implications of startup acquisitions. Given that an important motivation in acquiring a startup in eliminating nascent competitors (Santos and Eisenhardt 2009; Cunningham et al. 2017), there has been a growing discussion of the anti-competitive effects of acquisitions on the entry and survival of young firms. While this study documents the entry of new firms in the same industry following an acquisitions, the net impact of acquisitions on competition remains unclear. Therefore, more work is needed to clarify

[53] Even for startup acquisitions with extremely large valuations, as in the case of Facebook's $19 billion acquisition of WhatsApp in 2014, the non-founding employees experienced much smaller wealth gains compared to founders and early investors. https://blog.wealthfront.com/whatsapp-acquisition-employees/

[54] Sull et al. (2019) compute culture scores by analyzing over 1.2 million text reviews on Glassdoor. https://sloanreview.mit.edu/projects/measuring-culture-in-leading-companies/

the competitive dynamics between M&A and subsequent entrepreneurship by the departing workers.

Another avenue is to explore how the acquired technology is integrated and implemented inside the buyer firm. Extending a broad literature on this topic (c.f., Puranam and Srikanth 2007; Bena and Li 2014), a novel topic is the duality of technology and individuals that flow during an acquisition. Although this study documents nuanced effects depending on whether the target firm owns a patent, reflecting the important role of appropriability, more attention should be paid to the interplay between the actual inventor and the underlying patents. Given that startup acquisitions are an empirical setting in which there is co-mobility of patents and individuals – including cases when one asset moves but not the other – the complementarity between knowledge and individuals can be empirically assessed. In other words, how useful is knowledge without the original source? Insofar as knowledge and talent are valuable assets for firms, this seems to be a first-order line of scholarly inquiry. More broadly, the increasingly popular use of comprehensive employee-employer datasets is promising for future research streams on how human capital not only shapes the creation and growth of new ventures, but also how incumbent firms can acquire such entrepreneurial talent.

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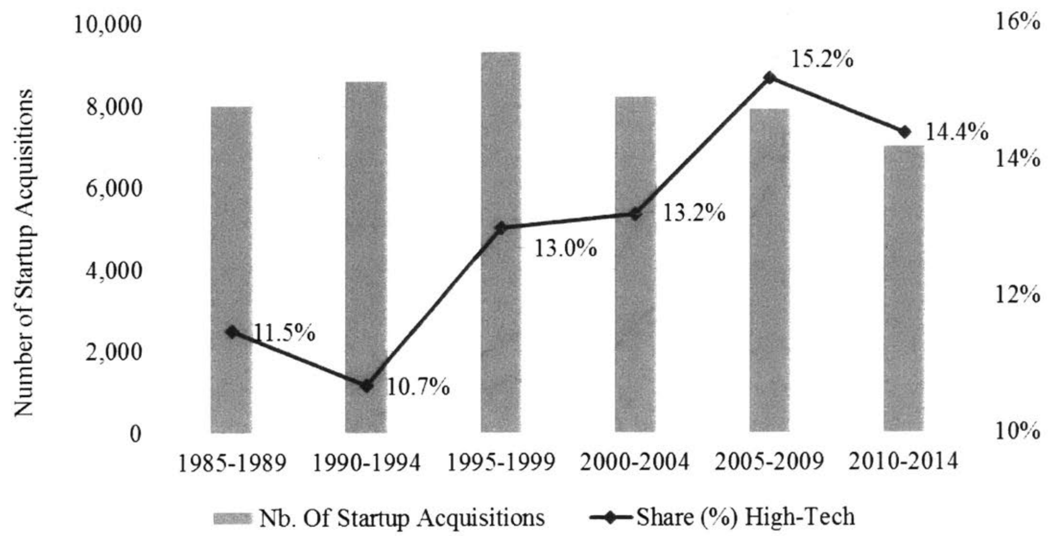
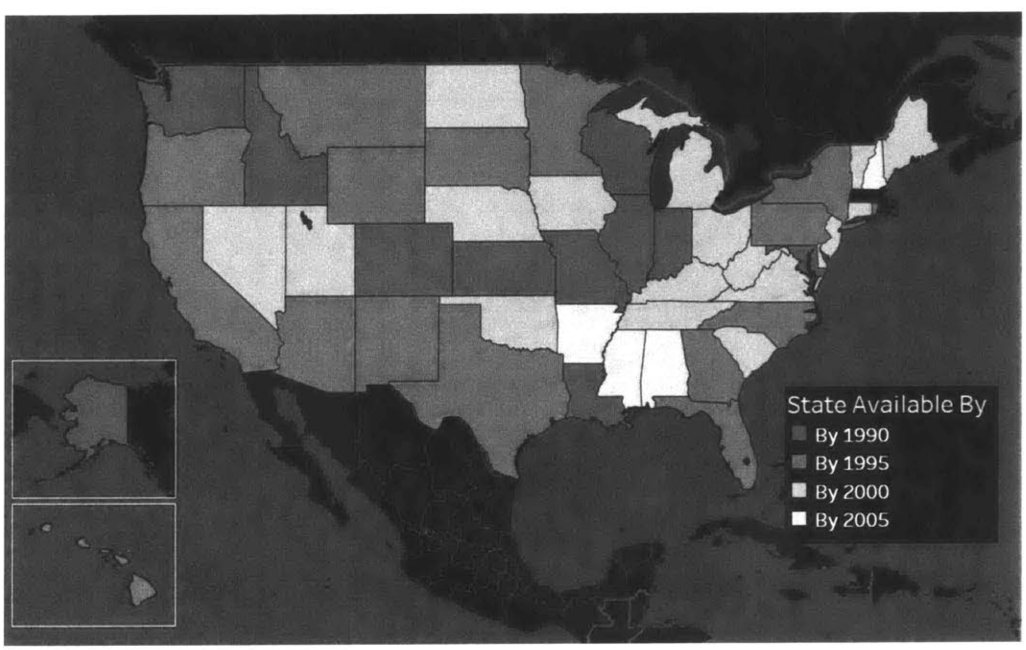
Figures and Tables

Figure 1: Time Trends in US Startup Acquisitions



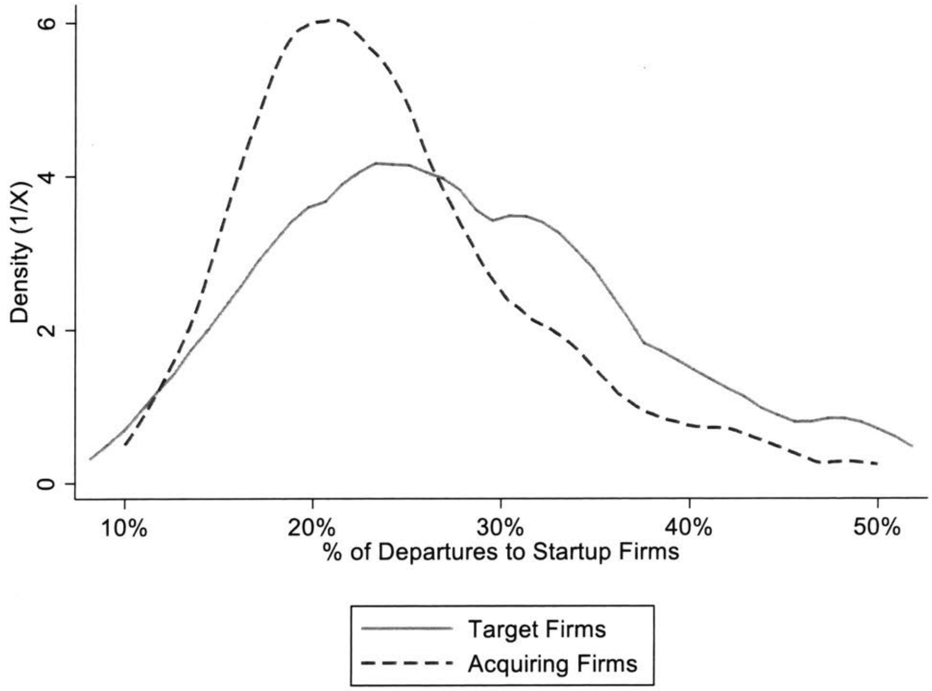
Note: This figure counts the number of times that startups, defined as younger than ten years old, are acquired by existing firms in a given five-year window. Acquisition activity is measured using the author's algorithm based on firm ownership changes in the LBD. Share of high-tech is the percentage of startup acquisitions that occur in industries with the highest shares of STEM-oriented workers (See Section 3 for detailed description of defining high-tech industry).

Figure 2: Map of US States and Entry Year in LEHD



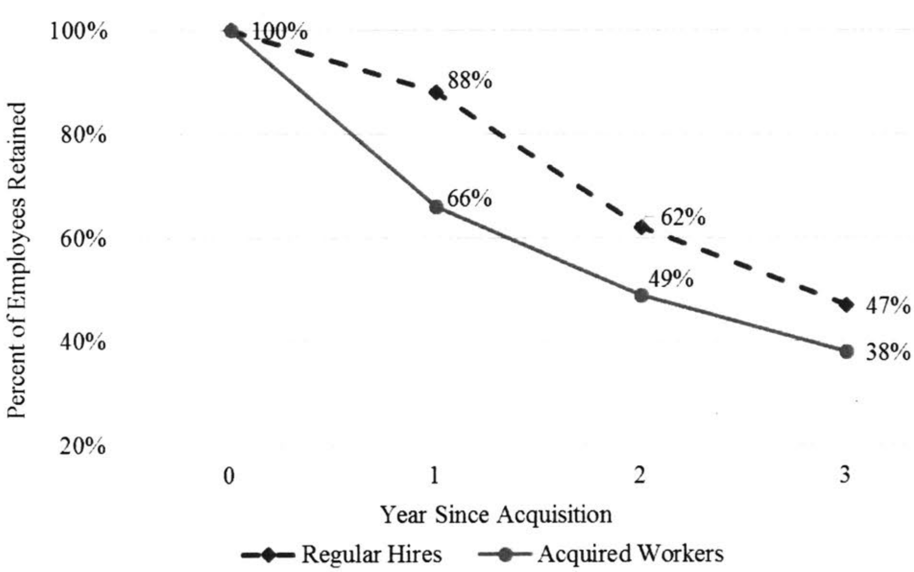
Note: See Vilhuber (2018) for a detailed description of the LEHD. This study uses all available states in the LEHD.

Figure 3: Distribution of Pre-Acquisition Departures to Startups



Note: This figure is the kernel density plot of the firm-level share of pre-acquisition employee departures to startup firms (5 years old or younger). Since the age of the receiving firm is the variable of interest, only employer-to-employer flows are counted.

Figure 4: Employee Retention Rates: Acquired Workers vs. Regular Hires

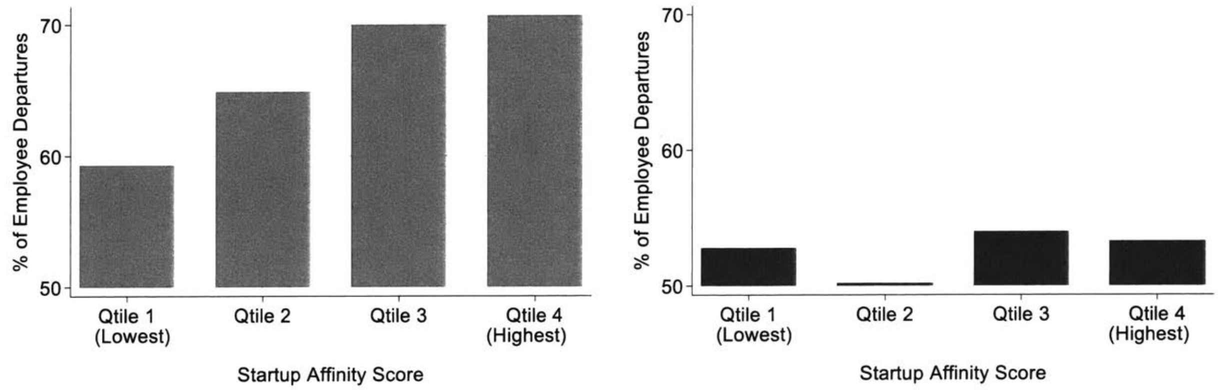


Note: This figure plots the unconditional retention rates. Both acquired workers and regular hires join the acquiring firm in year 0. Employee is retained in year *t* if she works for the firm for at least a quarter in year *t*.

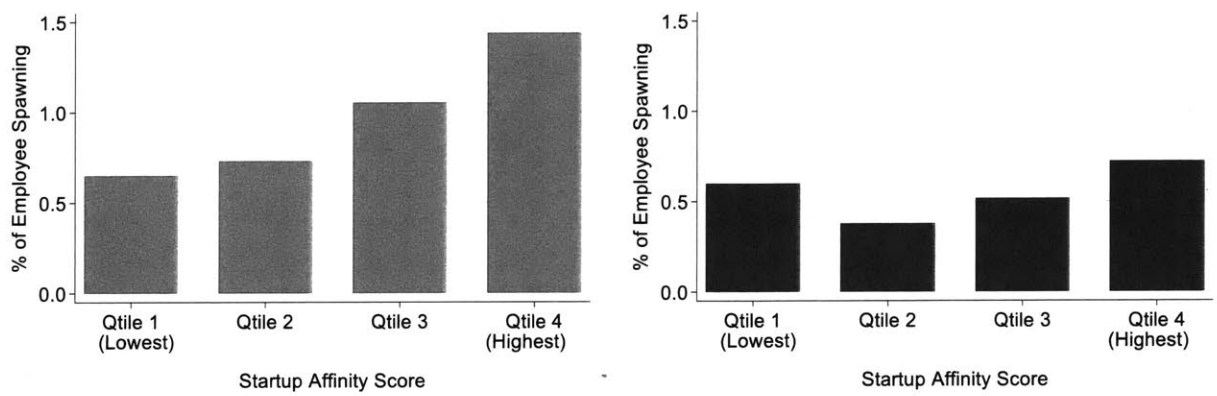
Figure 5: Employee Exit Rates by Prior Employer's Startup Affinity Score

Target Firms   Prior Employers of Regular Hires

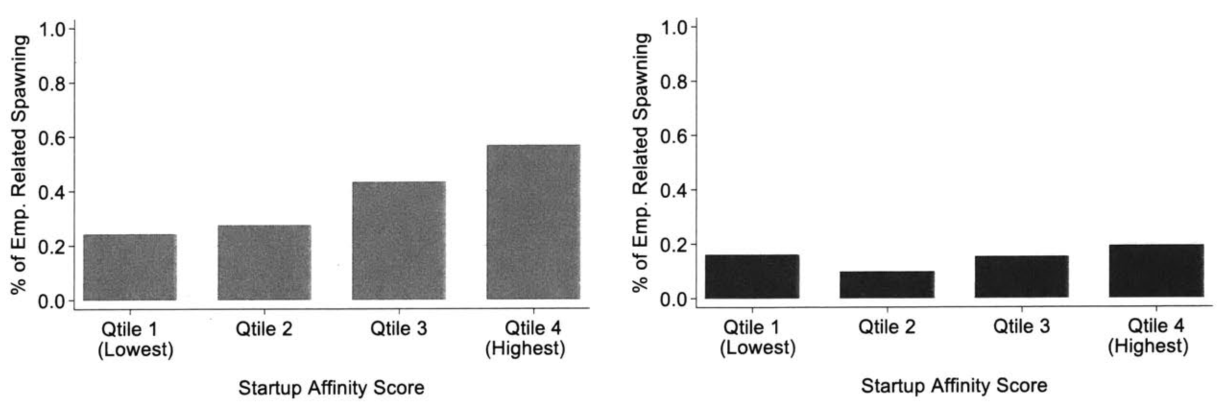
Panel A: Employee Departures (Within 3 Years of Acquisition)



Panel B: Employee Spawning (Within 3 Years of Acquisition)



Panel C: Employee Related Spawning (Within 3 Years of Acquisition)



Note: These figures plot the unconditional rates of employee departures, spawning, and related spawning in Panels A, B, and C, respectively. X-axis is the quartile-indicator for the prior employer's share (%) of pre-acquisition worker departures to startups (5 years old or younger). Prior employer for the acquired workers is the target startup. Employee departure in year *t* equals 1 if she does not receive any wages from the firm in year *t*. Employee (related) spawning in year *t* equals 1 if she is a founder of a new (same NAICS-2 industry) firm in year *t*.

Table 1A: Firm-level Summary Statistics

Characteristics	Target Firms (N=3,700) Mean	Median*	SD	Acquirers Mean	Median*	SD
Firm Size (Employee Count)	150	42	460	12,500	1,900	31,100
Firm Age	4.1	4.0	2.9	22.4	23.0	8.6
Payroll ($M)	10	3	30	860	130	2,700
Top NAICS-4 Industries (%)						
Computer Systems Design And Services	0.20		0.40	0.08		0.27
Mgmt., Scientific, and Technical Consulting Svcs.	0.12		0.32	0.02		0.15
Architectural, Engineering, and Related Services	0.11		0.31	0.06		0.24
Scientific R&D Services	0.07		0.26	0.03		0.16
Professional and Commercial Equipment & Supplies	0.07		0.26	0.05		0.22
Software	0.06		0.24	0.05		0.21
Data Processing, Hosting, and Related Services	0.06		0.24	0.03		0.17

Note: Observations are at the level of distinct target firms. Serial acquirers are counted multiple times based on their characteristics at the time of each acquisition. Following Census disclosure rules, quasi-medians (the average of observations in between the 41st and 59th percentile values) are shown.

Table 1B: Worker-level Summary Statistics

Panel A: Before Matching

Characteristics	Acquired Workers (N=295,000) Mean	Median*	SD	Regular Hires (N=5,267,000) Mean	Median*	SD
Annual Earnings ($)	81,000	54,700	980,600	65,500	45,900	160,000
Age	38.5	37.0	10.3	36.5	35.0	11.0
Male (%)	0.66		0.47	0.60		0.49

Panel B: After Matching

Characteristics	Acquired Workers (N=226,000) Mean	Median*	SD	Regular Hires (N=1,648,000) Mean	Median*	SD
Annual Earnings ($)	77,000	55,900	238,000	75,800	58,500	147,000
Age	37.5	36.5	9.6	36.3	35.5	9.2
Male (%)	0.67		0.47	0.68		0.46

Note: Observations are at the worker level. Founders and early joiners are removed from this sample. In other words, only late joiners (employees hired in or after second quarter since firm's birth) are included. Following Census disclosure rules, quasi-medians (the average of observations in between the 41st and 59th percentile values) are shown.

Table 2: Effect of Hiring Channel on Employee Departures

	Full Sample			Matched Sample		
	Depart by t+1	Depart by t+2	Depart by t+3	Depart by t+1	Depart by t+2	Depart by t+3
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired Worker	0.2173***	0.1316***	0.0860***	0.2172***	0.1363***	0.0898***
	(0.0130)	(0.0133)	(0.0130)	(0.0147)	(0.0151)	(0.0149)
Mean DV of Regular Hires	0.122	0.376	0.535	0.108	0.350	0.517
Buyer-Target Firm FE	YES	YES	YES	YES	YES	YES
Observations	5,562,000	5,562,000	5,562,000	1,874,000	1,874,000	1,874,000
R-squared	0.1066	0.1150	0.1094	0.1187	0.1117	0.1137

Note: This table is a set of worker-level regressions using OLS. Specifications 4-6 are based on matched workers using Coarsened Exact Matching. Depart by *k* equals 1 if the worker does not receive any wages from the firm in year *k*. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effect of Hiring Channel on Employee Departures by Startup Affinity Score

	Departure by *t+1*	Departure by *t+2*	Departure by *t+3*
	(1)	(2)	(3)
Startup Affinity Score[Q4] × Acquired Worker	0.0978**	0.1239***	0.1167**
	(0.0400)	(0.0438)	(0.0459)
Startup Affinity Score[Q3] × Acquired Worker	0.1061**	0.1213***	0.1037**
	(0.0419)	(0.0434)	(0.0450)
Startup Affinity Score[Q2] × Acquired Worker	0.0412	0.0476	0.0585
	(0.0383)	(0.0420)	(0.0441)
Startup Affinity Score[Q4]	0.0074***	0.0010	-0.0064
	(0.0025)	(0.0040)	(0.0042)
Startup Affinity Score[Q3]	0.0078***	-0.0053	-0.0111**
	(0.0024)	(0.0040)	(0.0046)
Startup Affinity Score[Q2]	0.0012	-0.0088**	-0.0106**
	(0.0021)	(0.0034)	(0.0045)
Acquired Worker	0.1568***	0.0650*	0.0224
	(0.0332)	(0.0374)	(0.0407)
Mean DV of Regular Hires	0.108	0.350	0.517
Matched Workers	YES	YES	YES
Buyer-Target Firm FE	YES	YES	YES
Observations	1,874,000	1,874,000	1,874,000
R-squared	0.1202	0.1127	0.1143

Note: This table is a set of worker-level regressions using OLS. All specifications are based on matched workers using Coarsened Exact Matching. Depart by *k* equals 1 if the worker does not receive any wages from the firm in year *k*. Startup Affinity Score[Qn] is a quartile-indicator for the prior employer's share of pre-acquisition worker departures to startups (5 years old or younger); prior employer is the target firm for the treated group, and the preceding job for the control group. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effect of Hiring Channel on Employee Departures by Startup Affinity: High vs. Low Organizational Mismatch

	HIGH Organizational Mismatch			LOW Organizational Mismatch		
	Departure by *t+1*	Departure by *t+2*	Departure by *t+3*	Departure by *t+1*	Departure by *t+2*	Departure by *t+3*
	(1)	(2)	(3)	(4)	(5)	(6)
Startup Affinity Score[Q4] × Acq. Worker	0.1237***	0.1373***	0.1303***	-0.0199	0.0608	0.0316
	(0.0367)	(0.0385)	(0.0500)	(0.0761)	(0.0749)	(0.0741)
Startup Affinity Score[Q3] × Acq. Worker	0.1321***	0.1387***	0.1149**	0.0747	0.0998	0.0960
	(0.0414)	(0.0390)	(0.0498)	(0.0524)	(0.0626)	(0.0619)
Startup Affinity Score[Q2] × Acq. Worker	0.0950**	0.0938**	0.1049**	0.0040	0.0110	0.0183
	(0.0444)	(0.0435)	(0.0520)	(0.0421)	(0.0491)	(0.0518)
Startup Affinity Score[Q4]	0.0092***	0.0057	-0.0041	0.0025	-0.0101*	-0.0123**
	(0.0031)	(0.0051)	(0.0055)	(0.0042)	(0.0055)	(0.0058)
Startup Affinity Score[Q3]	0.0091***	-0.0042	-0.0106*	0.0039	-0.0084*	-0.0133***
	(0.0030)	(0.0053)	(0.0063)	(0.0034)	(0.0050)	(0.0046)
Startup Affinity Score[Q2]	0.0028	-0.0089**	-0.0116*	-0.0023	-0.0090	-0.0092
	(0.0022)	(0.0042)	(0.0061)	(0.0043)	(0.0056)	(0.0056)
Acquired Worker	0.1380***	0.0521*	0.0123	0.1584***	0.0666	0.0246
	(0.0284)	(0.0300)	(0.0448)	(0.0396)	(0.0442)	(0.0474)
Mean DV of Regular Hires	0.108	0.350	0.517	0.108	0.350	0.517
Matched Workers	YES	YES	YES	YES	YES	YES
Buyer-Target Firm FE	YES	YES	YES	YES	YES	YES
Observations	1,245,000	1,245,000	1,245,000	629,000	629,000	629,000
R-squared	0.1187	0.1134	0.1150	0.1238	0.1108	0.1133

Note: This table is a set of worker-level regressions using OLS. All specifications are based on matched workers using Coarsened Exact Matching. Organizational Mismatch is a binary variable that equals 1 (0) if the target firm's Startup Affinity Score is higher than (less than or equal to) the buyer's Startup Affinity Score. Depart by *k* equals 1 if the worker does not receive any wages from the firm in year *k*. Startup Affinity Score[Qn] is a quartile-indicator for the prior employer's share of pre-acquisition worker departures to startups (5 years old or younger); prior employer is the target firm for the treated group, and the preceding job for the control group. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of Hiring Channel on Employee Spawning

	Any Spawn by $t+1$	Any Spawn by $t+2$	Any Spawn by $t+3$	Related Spawn by $t+1$	Related Spawn by $t+2$	Related Spawn by $t+3$
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired Worker	0.0009***	0.0021***	0.0032***	0.0008***	0.0013***	0.0020***
	(0.0002)	(0.0003)	(0.0003)	(0.0001)	(0.0002)	(0.0002)
Mean DV of Regular Hires	0.0018	0.0032	0.0049	0.0004	0.0008	0.0013
Matched Workers	YES	YES	YES	YES	YES	YES
Buyer-Target Firm FE	YES	YES	YES	YES	YES	YES
Observations	1,874,000	1,874,000	1,874,000	1,874,000	1,874,000	1,874,000
R-squared	0.0044	0.0051	0.0058	0.0058	0.0065	0.0077

Note: This table is a set of worker-level regressions using OLS. All specifications are based on matched workers using Coarsened Exact Matching. Any Spawn *k* is a binary variable equalling 1 if the worker is a founder of a new firm in year *k*. Related Spawn is a binary variable equalling 1 if the spawned company is in the same industry, measured at the level of 2-digit NAICS, as the target firm. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Heterogeneous Effects by Patenting

Dependent Variable	Departure by $t+3$		Any Spawn by $t+3$		Related Spawn by $t+3$	
Target Owns a Patent	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired Worker	+***	+***	+***	+***	+***	+ +***
	(.)	(.)	(.)	(.)	(.)	(.)
Mean DV of Regular Hires (Control Group)	-	-	-	-	-	-
Matched Workers	YES	YES	YES	YES	YES	YES
Buyer-Target Firm FE	YES	YES	YES	YES	YES	YES
Observations (Workers)	-	-	-	-	-	-

Note: Since these results are not yet disclosed from the US Census, only qualitative results (e.g., direction of estimates and level of statistical significance) are shown. This table is a set of worker-level regressions using OLS. All specifications are based on matched workers using Coarsened Exact Matching. Patent is a binary indicator on whether the target firm applies for or is granted a patent prior to the acquisition year. For brevity, only employee outcomes using a 3-year window are reported. All three outcomes – Departure, Spawning, Related Spawning – are defined the same way as in Tables 2 and 4. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Heterogeneous Effects by Non-Compete Enforceability

Dependent Variable	Departure by $t+3$		Any Spawn by $t+3$		Related Spawn by $t+3$	
Non-Compete Enforceability	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired Worker	+***	+***	+***	+***	+***	+***
	(.)	(.)	(.)	(.)	(.)	(.)
Mean DV of Regular Hires (Control Group)	-	-	-	-	-	-
Worker Controls	YES	YES	YES	YES	YES	YES
Buyer-Target Firm FE	YES	YES	YES	YES	YES	YES
Observations (Workers)	-	-	-	-	-	-

Note: Since these results are not yet disclosed from the US Census, only qualitative results (e.g., direction of estimates and level of statistical significance) are shown. This table is a set of worker-level regressions using OLS. All specifications are based on matched workers using Coarsened Exact Matching. Non-compete enforceability is a binary indicators (High or Low) defined at the level of the acquiring firm's state laws. For brevity, only employee outcomes using a 3-year window are reported. All three outcomes – Departure, Spawning, Related Spawning – are defined the same way as in Tables 2 and 4. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Association Between Target Employee Spawning and Acquirer's Firm Performance

Panel A: Employment Growth Rate Between Years t and t+3

	Unrelated Industry (1A)	Unrelated Industry (1B)	Same NAICS-2 (2A)	Same NAICS-2 (2B)	Same NAICS-4 (3A)	Same NAICS-4 (3B)	Same NAICS-6 (4A)	Same NAICS-6 (4B)
Number of Spawned Companies	-0.016***	-0.015***	-0.025***	-0.027***	-0.026***	-0.029***	-0.035**	-0.036**
	(0.004)	(0.004)	(0.007)	(0.007)	(0.009)	(0.009)	(0.014)	(0.014)
Ln(No. of Entrants in Target's Industry-Year)		0.010		0.023**		0.011		0.005
		(0.011)		(0.011)		(0.007)		(0.006)
Acquisition Year × Acquirer Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,200	3,200	3,200	3,200	3,200	3,200	3,200	3,200
R-squared	0.160	0.160	0.160	0.161	0.158	0.158	0.157	0.157

Panel B: Revenue Growth Rate Between Years t and t+3

	Unrelated Industry (1A)	Unrelated Industry (1B)	Same NAICS-2 (2A)	Same NAICS-2 (2B)	Same NAICS-4 (3A)	Same NAICS-4 (3B)	Same NAICS-6 (4A)	Same NAICS-6 (4B)
Number of Spawned Companies	-0.011*	-0.011*	-0.020**	-0.021**	-0.037***	-0.037***	-0.051**	-0.049**
	(0.006)	(0.006)	(0.009)	(0.009)	(0.014)	(0.014)	(0.023)	(0.023)
Ln(No. of Entrants in Target's Industry-Year)		0.003		0.014		0.003		-0.007
		(0.016)		(0.016)		(0.010)		(0.009)
Acquisition Year × Acquirer Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	1,700	1,700	1,700	1,700	1,700	1,700	1,700	1,700
R-squared	0.179	0.179	0.180	0.181	0.182	0.182	0.181	0.181

Note: This table shows a series of firm-level OLS regressions on the acquirer's long-run performance. *Number of Entrants* is the total number of new firms born during the acquisition year in the target firm's industry, where industry is defined at the level of corresponding column (e.g., NAICS-2 for columns 2A and 2B, and NAICS-4 for columns 3A and 3B). All specifications control for acquirer's firm age, included as four separate indicator variables for each quartile. State and Industry (NAICS-2) fixed effects are based on those of the acquiring firm. A new firm is unrelated if its NAICS-2 industry is different from that of the original target firm. To calculate growth, DHS (1996) growth measures are used (See Section 3.4). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: How Do Patents Change the Performance Implications of Spawning?

Panel A: Patenting-Owning Target Firms

	Employment Growth Between Years *t* and *t+3*				Revenue Growth Between Years *t* and *t+3*			
	Unrelated Industry	Same NAICS-2	Same NAICS-4	Same NAICS-6	Unrelated Industry	Same NAICS-2	Same NAICS-4	Same NAICS-6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Spawned Companies	–*	–	+	–	–	–	–	–
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Acquisition Year × Acquirer Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	-	-	-	-	-	-	-	-
R-squared	-	-	-	-	-	-	-	-

Panel B: Non-Patent-Owning Target Firms

	Employment Growth Between Years *t* and *t+3*				Revenue Growth Between Years *t* and *t+3*			
	Unrelated Industry	Same NAICS-2	Same NAICS-4	Same NAICS-6	Unrelated Industry	Same NAICS-2	Same NAICS-4	Same NAICS-6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Spawned Companies	–***	–***	– –***	– – –***	–***	–*	– –***	– – –**
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Acquisition Year × Acquirer Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	-	-	-	-	-	-	-	-
R-squared	-	-	-	-	-	-	-	-

Note: This table shows a series of firm-level OLS regressions on the acquirer's long-run performance. *Number of Entrants* is the total number of new firms born during the acquisition year in the target firm's industry, where industry is defined at the level of corresponding column (e.g., NAICS-2 for columns 2A and 2B, and NAICS-4 for columns 3A and 3B). All specifications control for acquirer's firm age, included as four separate indicator variables for each quartile. State and Industry (NAICS-2) fixed effects are based on those of the acquiring firm. A new firm is unrelated if its NAICS-2 industry is different from that of the original target firm. To calculate growth, DHS (1996) growth measures are used (See Section 3.4). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix

List of High-Tech Industries

As discussed in Section 3.1, I follow Hecker (2005) and Goldschlag and Miranda (2016) to label a set of industries as high-tech. More specifically, I identify the NAICS-4 industries with the highest shares of STEM-oriented workers. Table A1 displays the list of high-tech NAICS-4 industries represented among the target startups.

Table A1: List of High-Tech (NAICS-4) Industries

NAICS-4	Industry
2111	Oil and Gas Extraction
3254	Pharmaceutical and Medicine Manufacturing
3341	Computer and Peripheral Equipment Manufacturing
3342	Communications Equipment Manufacturing
3344	Semiconductor and Other Electronic Component Manufacturing
3345	Navigational, Measuring, Electromedical, and Control
3364	Aerospace Product and Parts Manufacturing
5112	Software Publishers
5161	Internet Publishing and Broadcasting
5171	Wired Telecommunications Carriers
5179	Other Telecommunications
5181	Internet Service Providers & Web Search Portals
5182	Data Processing, Hosting, and Related Services
5191	Other Information Services
5413	Architectural, Engineering, and Related Services
5415	Computer Systems Design and Related Services
5417	Scientific R&D Services (including Life Sciences)

Additional Analyses on Startup Affinity Scores

A central concern with interpreting results in Section 5.3 is that *Startup Affinity Score* may be systematically related to other firm characteristics. For instance, it could be the case that small firms tend to exhibit higher post-acquisition turnover as well as high share of pre-acquisition departures to startups. In this case, *Startup Affinity Score* – which is calculated by the share of pre-acquisition departures to startups – would be an endogenous reflection of firm size rather than a measure of affinity for startup employers. To address this concern, I first regress *Startup Affinity Score* on important target startup characteristics including firm size, age, and NAICS-4 industry, and provide empirical support that this measure is not driven by other firm covariates. Furthermore, I use the residuals from the preceding regression to replicate the original results in Section 5.3 that net out the effects from firm characteristics.

Table A2 shows the results from regressing *Startup Affinity Score* on observable characteristics of the target firm. *Startup Affinity Score* is the percent share of pre-acquisition departures to startups, bounded between 0 and 1. All specifications include a fully saturated set of NAICS-4 industry indicators. Specification 1 and 2 demonstrate that the effect of firm age and firm size on *Startup Affinity Score* is statistically indistinguishable from zero. When entered together in Specification 3, the results are consistently zero. These null findings show that *Startup Affinity Score* is not systematically related to firm age, firm size, or industry-specific traits.

Table A2: Predicting Startup Affinity Score

	DV: Startup Affinity Score [0,1]		
	(1)	(2)	(3)
Firm Age	-0.00043		-0.00036
	(0.00118)		(0.00115)
Log Firm Size		-0.00107	-0.00096
		(0.00275)	(0.00270)
Observations	3,400	3,400	3,400
R-squared	0.02600	0.02602	0.02605
Industry (NAICS-4) FE	YES	YES	YES

Note: This table shows a series of firm-level OLS regressions. Startup Affinity Score is a percent share (between 0 and 1) of pre-acquisition departures to startup (5 years old or younger). Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Next, I provide further robustness by using the residuals from Table A2. In particular, I test the impact of *Startup Affinity Score* on acquired employee outcomes, after netting out the portion of *Startup Affinity Score* explained by firm size, age, and industry. Instead of converting *Startup Affinity Score* into quartiles values as done in Table 3, Table A3 uses the raw score for clarity. For brevity, only outcomes using a one-year window are reported; results are consistent with using two- and three-year windows.

Table A3: Employee Outcomes by Startup Affinity Score: Actual vs. Residual Values

Dependent Variable	Departure	Any Spawn	Related Spawn	Departure	Any Spawn	Related Spawn
Time Window	by t+1	by t+1	by t+1	by t+1	by t+1	by t+1
	(1)	(2)	(3)	(4)	(5)	(6)
Startup Affinity Score x Treated	0.3109***	0.0052***	0.0025**			
	(0.0992)	(0.0016)	(0.0011)			
Residual Score x Treated				0.3259***	0.0050***	0.0028**
				(0.1102)	(0.0017)	(0.0012)
Treated	0.1280***	-0.0003	0.0001	0.2145***	0.0012***	0.0008***
	(0.0325)	(0.0004)	(0.0003)	(0.0114)	(0.0002)	(0.0001)
Mean DV of Control Group	**0.105**	**0.0016**	**0.0004**	**0.105**	**0.0016**	**0.0004**
Observations	2,354,000	2,354,000	2,354,000	2,354,000	2,354,000	2,354,000
R-squared	0.1123	0.0035	0.0043	0.1123	0.0035	0.0043
Matched Workers	YES	YES	YES	YES	YES	YES
Buyer-Target Firm FE	YES	YES	YES	YES	YES	YES

Note: This table shows a series of firm-level OLS regressions. Startup Affinity Score is a percent share (between 0 and 1) of pre-acquisition departures to startup (5 years old or younger). Residual scores are calculated for each from Table A1. Standard errors, clustered at the firm level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Specifications 1-3 in Table A3 use the original *Startup Affinity Score*. Consistent with Table 3, the interaction between the score and *Treated* indicator are positive and significant. In other words, acquired workers from startups with a high *Startup Affinity Score* are more likely to leave as well as spawn new companies, compared to their counterparts from a firm with a low *Startup Affinity Score*. Specifications 4-6 repeat these regressions, except with using residuals from Table A2. The results are strongly consistent. Therefore, over and above the target firm's size, age, and industry, pre-acquisition departures to startups are a robust predictor of the acquired employees' career outcomes.