Do startup employees earn more in the long-run?*

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Abstract: Analyzing Danish registry data, we find that those hired by startups earn roughly 17% less over the next ten years than those hired by large, established firms. About half of this earnings differential stems from sorting—from the fact that startup employees tend to have less human capital. But long-run earnings also vary depending on when individuals are hired. While the earliest employees of startups suffer an earnings penalty, those hired by already-successful startups earn a small premium. Two factors appear to account for the earnings penalty of the earliest employees: Startups fail at high rates, creating costly spells of unemployment for their (former) employees. Job mobility patterns also diverge: After being employed by a small startup, individuals rarely return to the large firms that pay more.

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INTRODUCTION

Entrepreneurship has captured the imagination. Policymakers and pundits alike see startups as a solution to a variety of economic problems, from slowing economic growth to high levels of unemployment. Legislation favoring these fledgling firms has followed. In introducing the JOBS Act, for example, President Obama asserted that "...new businesses account for almost every new job that's created in America" (Liberto 2012).

Individuals worldwide, meanwhile, increasingly see startups as desirable employers and entrepreneurship as an attractive career option (Bosma and Kelley 2018). Jobs in startups and entrepreneurial careers seem more interesting than the more routine roles found in bureaucratic behemoths (e.g., Sheldon 2012). People also see startups as an opportunity to become rich. Stories abound of entrepreneurs and employees—from executives to janitors—who came into a windfall of wealth when their employers went public (e.g., Lien 2017; Staff 2018).

But does this enthusiasm for startup employment reflect reality? Do employees benefit from working at a startup? Given the research on firm founders, entrepreneurs, one might expect that they would. Individuals who become entrepreneurs—who build firms that employ others, as opposed to simply being self-employed—earn more than similar peers who work as employees (Braguinsky et al. 2012; Levine and Rubenstein 2017; Sorgner et al. 2017).[1] Even former entrepreneurs who return to being employees climb faster up the corporate ladder and earn more than peers who never became entrepreneurs (Kaiser and Malchow-Møller 2011; Baptista et al. 2012; Luzzi and Sasson 2016).

The employees of startups, however, differ from the founders of these firms on a number of dimensions. They typically own much less, if any, equity. For both good and bad, their financial rewards depend less on the fate of the firm. Startup employees are also still subordinates. They have neither the independence nor the scope of responsibility of founders. Their sorting into startups

[1] Although many early studies found that "entrepreneurs" earned less than employees (Evans and Leighton 1989; Hamilton 2000; Moskowitz and Vissing-Jørgensen 2002; Hyytinen et al. 2011), these studies included both firm founders and the self-employed as entrepreneurs. The lower earnings of entrepreneurs in these studies stem from the self-employed, those without any employees (Åstebro et al. 2011; Levine and Rubenstein 2017).

versus more established employers, moreover, may also stem less from their own volition than from which firms offer them jobs. Spells at startups therefore could have very different implications for employees than they do for entrepreneurs.

We examined the long-term earnings consequences of startup employment using data from Denmark, from 1992–2012. On average, those who became employees of startups—defined as firms that had been operating for no more than four years—earned about 17% less over the next ten years. But sorting accounted for nearly half of this difference and the consequences of a spell of startup employment varied depending on the size of the startup at the time of hiring. Although the early employees of startups—defined as those hired before the firm reached 50 employees—earned, on average, 10%-15% less over the subsequent decade than observationally-equivalent peers, those hired by successful startups—by startups that had grown to more than 50 employees—earned 2%-4% more. These patterns also do not seem to reflect simply the differing ambitions of the founders of these firms—whether they intended to remain small or to grow large.

We explored what might account for the earnings penalty associated with being an early startup employee. Two factors appeared important. First, startup employees, particularly the earliest hires, had less stable jobs and therefore experienced more and longer spells of unemployment. These spells led both to a short-term loss of income and to slower long-term earnings growth. Second, these individuals joined small firms, and most small startups never grow large. But small firm employment became a near-absorbing state. Even if employees left these firms, they moved to other small firms. This path dependency in employer size creates a bifurcated labor market, with one set of individuals progressing through careers in large employers and a second set holding precarious positions at a series of startups and small firms.

CAREERS AND STARTUPS

Despite the growing prominence of entrepreneurship in public policy and in the public sphere, relatively little research has examined the employees of startups. Why do some individuals and

not others end up employed at startups? How do stints of startup employment influence career progression and earnings? What other effects does being part of a startup have on professional and personal lives?

What limited research has considered startup employees has been focused almost entirely on their short-term earnings. Research has found that young firms, particularly small, young firms pay less than large, established ones. Troske (1998), for example, reported that the youngest manufacturing plants in the United States paid nearly 20% less than the oldest ones. Some of this difference stems from sorting, since startups, on average, hire less educated and less experienced employees. But even accounting for these differences, Ouimet and Zarutzkie (2014) found that startups paid roughly 5% less, on average, than established employers in the United States. Burton et al. (2018) similarly reported that the employees of small startups in Denmark earned at least 5% less than their observationally-equivalent peers at the time of hiring.

But what are the long-term consequences of becoming a startup employee? Although startups may pay less than other employers at the time of hiring (Heyman 2007; Nystrom and Elvung 2014; Burton et al. 2018), startup employees may nevertheless earn more in the long run. Rapidly-growing organizations may offer greater income growth, compensating for any initial pay penalty. Startups also offer employees the opportunity to develop their human capital. Even if these startups do not succeed, other employers may value this human capital (e.g., Campbell 2013; Luzzi and Sasson 2016). Startup experience may also prove beneficial if these individuals become entrepreneurs themselves (Sørensen and Phillips 2011; Kacperczyk 2012; Dahl and Sorenson 2014). Let us consider each of these possibilities.

The growth effect

Startup employees get in on the ground floor. Because these firms operate at smaller scale and have fewer resources, individuals enter them at higher levels than they could command in a larger organization. Someone coming out of business school, for example, might become the Chief Marketing Officer of a startup instead of a regional brand manager for a large corporation. If the startup

grows by introducing new hires as subordinates to these individuals, early employees could attain a senior position in a large company at an earlier point in their career than they could have hoped to achieve through climbing a corporate ladder (Rosenbaum 1979; Stewman and Konda 1983).

Expected earnings growth can also help to compensate for the riskiness of startup employment. Startups fail at much higher rates than established firms (Freeman et al. 1983; Carroll and Hannan 2000; Yang and Aldrich 2017). When they do, employees find themselves out of jobs. Since startups seem particularly vulnerable to economic downturns (Fort et al. 2013), their (former) employees, moreover, may find themselves in search of employment precisely when jobs are most scarce. Given these higher odds of unemployment, one might expect that startup employees would demand higher long-run earnings expectations in exchange. Indeed, much of the literature on the returns to entrepreneurship focuses on the idea that founders should earn more over the long run to compensate them for the higher variability of their expected income streams (e.g., Hamilton 2000; Moskowitz and Vissing-Jørgensen 2002; Åstebro and Chen 2014).

Along both of these dimensions, one would expect the strongest income growth effects for the earliest employees of a startup. These individuals enter their firms before extended hierarchies have been established. They therefore often become the de facto heads of their functions or divisions. These early employees also join their firms at a time when the prospects of the firm are difficult to predict. They therefore should require the largest potential rewards as compensation for this uncertainty.

The human capital effects

Individuals choose jobs based not only on their rewards but also on how they expect those jobs to influence their earnings trajectories and career progression (Bidwell and Briscoe 2010). Internships, for example, pay poorly but offer the promise of attractive future positions. Startup employment has at least two consequences for the endogenous formation of human capital, and therefore for future earnings expectations.

Skill variety. Startup employees must wear multiple hats. Startups usually do not have sufficient needs in many areas to keep a full-time employee busy and they do not have enough resources to maintain much spare capacity. Startup employees, therefore, help out where needed. When a crisis arises, it's all hands on deck. At any given point in time, startup employees therefore engage in a wider range of activities and develop a broader set of abilities than their peers in larger, more bureaucratic organizations (Sørensen 2007; Campbell 2013).

Startup employees also probably develop an even broader set of experiences over time. Firms begin without established roles and routines (Stinchcombe 1965; Freeman et al. 1983; Yang and Aldrich 2017). As they learn-by-doing and develop procedures, roles become better delineated. But those roles and routines change frequently, particularly in the early months and years. Startups face substantial uncertainty in how their operations will evolve. They therefore must redeploy employees to new responsibilities on a regular basis, further expanding the range of abilities and experience that these employees acquire.

This greater variation in abilities and in activities may in turn influence the job prospects of startup employees. Senior levels in the hierarchies of large corporations require a broader range of expertise than more junior ones (Blau 1970). But individuals attempting to climb corporate ladders often have few opportunities to acquire this broad expertise. As established firms increasingly turn to the outside for talent (Bidwell 2011), startup employment may allow individuals to obtain the experience needed to move into managerial roles.

To the extent that more varied experience accelerates career progression, starutp employees may enjoy steeper earnings trajectories. Although research has not examined whether the employees of startups benefit from this acceleration, firm founders do rise more rapidly through the ranks if they return to being employees (Kaiser and Malchow-Møller 2011; Baptista et al. 2012; Campbell 2013; Luzzi and Sasson 2016). Presumably the broad experience that they acquired as entrepreneurs helps them to outshine their peers.

Skill idiosyncrasy. But startup experience also has a downside for human capital formation. Because of their small scale and limited role differentiation, startups often define jobs based on the individual backgrounds of the original occupants of those positions, aggregating unusual combinations of responsibilities (Burton and Beckman 2007). As a result, employees become experts in idiosyncratic sets of activities.

If startup employees move to other employers, they may then lose much of the value of the human capital that they have acquired. Those usual bundles of expertise primarily have value to the startups where employees originally acquired them. Other employers may only have a need for a subset of these skills. Atypical experiences and expertise profiles can also serve as a negative signal to potential employers, who often have expectations for how careers should unfold and progress over time (Barnett and Miner 1992; Leung 2014).

Skill variety and sill idiosyncrasy tend to move hand in hand. The earliest employees of startups, for example, should experience both most strongly. As firms mature and grow, they become more stable. Larger scale allows for a more extended division of labor and firms develop more standardized roles for their employees (Stinchcombe 1965; Kalleberg et al. 1996). As a consequence, the opportunities for being exposed to a broad range of activities decline but so too does the probability of having a highly idiosyncratic job. But as both the positives and negatives decline, the net effect of these changes remains an open empirical question.

The entrepreneurship option

Those with experience in startups also improve their odds of being successful entrepreneurs. Firm founders, because they must manage and engage in nearly all of the activities of their firms, benefit from being jacks-of-all-trades (Lazear 2005; Sørensen 2007). As noted above, startup experience can help individuals to build this broad range of abilities. Startup employees also more commonly have connections with the external environment, with suppliers and customers (Blau 1977), putting them in a better position to spot business opportunities and to mobilize resources (Sorenson and Audia 2000; Sørensen 2007; Sorenson 2017).

Startup employment may also give would-be entrepreneurs access to better organizational blueprints. Past employers serve as organizational models for entrepreneurs—templates for who to hire, how to organize activities within the firm, and how to operate (Freeman 1986; Phillips 2002). But the ideal organizational structure for a startup differs from that of an established firm (Sørensen and Phillips 2011). One requires more flexibility, the other more reliability. Individuals who have prior experience in startups may therefore have a better sense of how to organize their own fledgling firms. Prior employment, particularly at a successful startup, may give individuals privileged access to tried-and-true operational routines and organizational designs (Klepper 2001; Phillips 2002).

Consistent with this expectation, studies have found that those employed by startups become entrepreneurs at higher rates than their peers in more established firms (Dobrev and Barnett 2005; Gompers et al. 2005; Sørensen 2007; Kacperczyk 2012), and that their ventures survive longer (Sørensen and Phillips 2011; Dahl and Sorenson 2014). Startup employment may therefore allow those interested in becoming entrepreneurs themselves the opportunity to acquire the abilities and information that they will need to succeed on their own.

DATA AND METHODS

We examined the long-term earnings consequences of startup employment using Danish registry data. Denmark has an economy representative in many ways of other high-income countries, including Canada and the United States. It has high levels of labor mobility and of industrial dynamism – entry and exit of firms – on par with the United States (Sørensen and Sorenson 2007). In terms of areal size, population, average income, and the proportion of the labor force employed in agriculture, manufacturing, high tech, and services, it looks much like the state of Massachusetts.

Our data come from the Integrated Database for Labor Market Research (commonly referred to by its Danish acronym, IDA). This matched employer-employee database, which includes all individuals legally residing in the country each year, contains information on the characteristics of

individuals, on their annual earnings, as well as on the characteristics of their employers. Although IDA begins in 1980, we only analyzed data from 1991 onwards. From the late-1970s to 1990, Denmark underwent a series of regulatory reforms dismantling much of the centralized wage setting system (Madsen et al. 2001). Denmark now has one of the most liberal labor markets in Europe, similar in its dynamics to economies such as Canada, the United Kingdom, and the United States (Bingley and Westergård-Nielsen 2003; Sørensen and Sorenson 2007).

As described above, it is known that startups, as small firms, provide less attractive employment options in the short term. Many of the potential advantages to startup experience are thought to accrue to individuals as a startup grows or after they leave these firms. If their startup experience allows employees to secure better-paying positions at other firms or to succeed as entrepreneurs themselves, one would not capture those effects by associating earnings with the characteristics of the *current* employer.[2] To estimate returns to startup employment, one must instead examine career trajectories, or what individuals earn over extended periods, regardless of whether they remain at the startup or move on to other opportunities.

Employees enter our analyses the first time that they change employers after 1991 and remain in our analyses for ten years following that event. Models exploring even longer windows—up to 20 years—nevertheless revealed that the earnings penalties and premiums reported here persist far past this first decade. We restricted our analyses to individuals who changed employers, after 1991, between the ages of 18 and 50. That effectively restricts the age range for our analyses to individuals from 18 to 60, largely removing retirement from the equation. We also limited our analyses to full-time employees to avoid the confounding effects of hours worked on earnings.

Startup employees: We considered employees to have experienced a spell of startup employment if, between 1991 and 2011, they became an employee of a firm that had been operating for no more than four years. To focus solely on startup employees (and not members of the founding team),

[2]Note that approaches using employee-level fixed effects also typically have this limitation as they associate earnings with the attributes of the current employer (e.g., Schmieder 2013).

we excluded both individuals with equity ownership and those employed at the organization from day one. We chose four years as the age threshold for being a startup because that represents the half-life of a firm in Denmark (Dahl and Sorenson 2012). Failure rates begin to decline when firms survive longer than that.

We used ownership information to exclude foreign subsidiaries, spin-outs, and privatizations as startup employers. As a further check against including these cases, we also excluded new firms in which at least 30% of the employees worked together in the prior year for another employer in the same industry and region. This approach may drop some actual startups but treating well-established entities as new ones seemed more problematic than losing a few cases.

Case-Control Triplets

One challenge when analyzing the career trajectories of startup employees is that startups are known to recruit from a different labor pool than established firms. We account for the sorting of individuals to employers by using the matching method of case-control triplets introduced by Burton et al. (2018). We pair each focal individual who moved to a startup (or to a small, established firm) with two (control) individuals who moved to large, established firms in the same calendar year.

Each focal individual can only appear once in the analyses. If a person has multiple instances of being hired by a startup (or a small, established firm), we included only the first event (after 1991) as a treatment. Control individuals, however, have been selected with replacement, so they may appear in more than one case-control triplet. Matching with replacement introduces some correlation across triplets into the error structure, but it has the advantage of ensuring the retention of more treated cases and therefore of generating estimates that are more representative of the population average effect (Abadie and Imbens 2006). We found matches for more than 90% of the focal individuals.

For each focal individual who joined a startup in a given year, we found all individuals who matched that individual exactly in terms of gender, year of birth, years of education, and prior occupation (i.e. occupation at their previous employer) and who moved in the same year as the

focal individual, to employers that had been operating for more than four years and had more than 50 employees. This first step produces a set job movers who are exact matches on demographic characteristics. Within this set, we then selected as controls the two nearest neighbors on the *prior* year's wage distribution—the closest observation above and the closest below what the focal employee earned in that year.[3] In other words, the case-control triplets had identical characteristics, all changed jobs at the same time, and earned the same amount prior to joining their new employers.

Matching on prior income, in particular, proves powerful. A host of factors can influence individual earnings, many of which, such as social skills or personality traits, elude easy measurement. But, to the extent that they remain relatively stable, these individual-level differences should contribute not only to current and future earnings but also to what individuals made in their previous jobs. By ensuring that the cases and controls have equivalent prior incomes, our matching effectively accounts for many of these difficult-to-observe factors. These differences matter. Indeed, the early employees of startups versus those hired by already-successful startups differ more in their prior incomes than on any other dimension (see Appendix).

Our models on the matched sample include triplet-level fixed effects to adjust for all of the common observed and unobserved characteristics of these triplets. Note that this approach has a number of advantages relative to including the individual-level characteristics as regressors. Most notably, because the fixed effect for each triplet adjusts for a particular combination of attributes, it effectively allows those attributes, such as education, prior earnings, and experience, to have flexible independent and joint effects in the determination of wages (Burton et al. 2018). In other words, it does not assume any functional forms in the relationships between these factors and income. The models also include indicator variables for the year, to adjust for factors such as inflation and for changes in average wages over time.

[3]In essence, our approach combines coarsened-exact matching with nearest-neighbor matching on income. Many approaches to matching have been developed. Coarsened-exact matching has the advantage of guaranteeing balance between the cases and the controls on all dimensions used for matching. For extended discussions of the advantages of coarsened-exact matching relative to propensity score matching, see Iacus et al. (2012); King and Nielsen (forthcoming).

LONG-TERM EARNINGS DIFFERENTIALS

Our primary income measure includes all hourly wages, salaries, and bonuses received by an individual in a year from all employers. One might worry that this measure fails to capture certain forms of compensation, such as fringe benefits or stock options. But the Danish state provides most benefits, so they vary little from firm to firm. Few companies in Denmark, moreover, reward employees with equity or stock options (Eriksson 2001). Because Denmark taxes equity awards as income rather than as capital gains and has historically taxed them in the year awarded (rather than in the year exercised), such awards end up being unattractive.[4] Most private companies in Denmark therefore use bonuses to pay for performance (Eriksson 2001).

We treat joining a startup as an event (a "treatment") rather than as a state variable. In doing so, our approach parallels studies that examine the long-term career and earnings consequences of events such as marriage, recessions, parenthood, or incarceration (e.g. Kahn 2010; Western 2002; Oreopoulos et al. 2012; Schwandt and von Wachter 2019). Note that this approach implies that even if individuals leave their original employers, our indicator variables for having been an employee of a startup continue to have the same values. Our estimates therefore capture the *total* long-term earnings effects associated with a stint at a startup from a multitude of channels—disparities in initial earnings, differing slopes of earnings trajectories, and future episodes of entrepreneurship. In general terms, around 16% of our sample is employed at a startup at a given time. This is consistent, for example, with rates of employment at young firms in the U.S. (Decker et al. 2014).

To explore in depth the long term effects of joining a startup, we first estimate average long-term wage differentials between startup employees and individuals who join established firms. Next, we account for sorting effects using the triplets approach described above. The following subsections unpack these observed differences by exploring whether the penalties for joining a startup vary depending on the relative success of the startup at the time of joining it, whether the the pay trajectories of similar employees who diverge after one joins a startup nonetheless converge over

[4]By contrast, the United States both taxes most equity awards at a lower rate than other types of income and usually does not impose those taxes until the equity has been sold.

time, and whether average wage differentials might still hide a skewed distribution where a few winners experience outsize returns. Next, we check whether our effects are different for employees who join firms with a stronger growth orientation and we assess the extent to which our results may be driven by sorting on unobservable individual characteristics and preferences through an instrumental variable approach.

Average wage differentials of startup employees

Table 1 first reports our regression estimates of the average ten-year differences in earnings for the employees of startups relative to those of more established firms.[5] We begin by regressing annual logged income on an indicator variable for having been a startup employee. Beyond this binary variable, the model includes only a vector of indicator variables for the calendar year and another vector for the year in which each individual entered our analyses (i.e. first changed jobs post-1991). Together these time fixed effects adjust for average differences in earnings over time.

On average, those who became employees of startups earned about 17% less – about $58,000 for the average person in the dataset – over the subsequent ten years relative to those hired by more established employers (firms that had been operating for more than four years).[6]

As mentioned above, however, these estimates do not account for differences in the characteristics of startup employees versus those of more established firms. But startups do not hire at random. They systematically employ younger and less educated people, who earned less in their prior jobs (Nystrom and Elvung 2014; Ouimet and Zarutzkie 2014; Burton et al. 2018).[7] Since these characteristics would influence the amount that an individual could expect to earn at any job, we need to adjust for this sorting to separate the effects of joining a startup from those resulting from the fact that individuals who join startups tend to be different.

[5]The first model includes all eligible individuals who became employees of startups during our observation window but only a simple random sample of those who became employees of established firms.

[6]The coefficient −.184 estimates the difference in logged earnings. One can approximate this difference in percentage terms using the antilogarithm (i.e. $= e^{-.184}$).

[7]Interestingly, high-potential, high-tech companies may benefit from *positive* selection, as they often hire better individuals (at a premium) than more established firms in the industry (Kim 2018).

Wage differentials in matched samples

The third column of Table 1 reveals that a large portion of the observed long-term earnings differential stems from the characteristics of individuals. After adjusting for the sorting of employees to employers, the employees of startups made about 8% less over the next decade (about $27,000 less on average) than their observationally-equivalent peers at large, established firms. Because this ten-year window includes earnings from the startup, from other employers if these individuals leave the startup, and from future entrepreneurship, it should capture any potential long-run upside to being a startup employee.[8] In supplemental analyses, these patterns held even within broad sectors of the economy – manufacturing, services, and high technology – and across sub-periods of our observation window.

Success of the startup at moment of hiring. Joining a startup during its early stages, as one of its first employees, is a categorically different proposition than joining it when the firm has already achieved certain success, reflected in its relative growth. As firms grow, they adjust their business models to better match customer needs, they establish more routines and processes, and overall uncertainty decreases. To explore these differences we estimated the importance of the stage of a startup's development at the time of hiring. Figure 1 describes how long-term pay changes as a function of firm size at the time of hiring. To create the figure, we estimated a series of models based on our matched samples (and matched triplet fixed effects), which allowed the pay penalty over the following ten years to vary freely as a function of the size of the startup at the time of hiring. We smooth the shape of the curve by estimating the pay penalty for overlapping bins of five employees (i.e. 1-5, 3-7,..., 49-54).[9] The gray lines represent the 95% confidence intervals around

[8] Although one might worry that our measure of earnings does not include dividends and capital gains for those who become entrepreneurs, entrepreneurs with and without prior experience as an employee of a startup did not differ significantly in their capital gains. Including these forms of income would therefore add noise to our estimates – because they include dividends and capital gains from all sources – but they would not change either the direction or magnitude of our estimated effects. We nevertheless included these income sources in our calculations of the trajectories shown in Table 6

[9] Because the data become sparser as firm size increases, we can only estimate five-employee bins up to 54 employees. Above that size, we would need larger bins. Largers bins reveal that the estimates turn positive, though with high levels of uncertainty, above that scale.

these estimates. The long-term pay penalties associated with being hired by a startup decline over this range. The more established the startup at the time of hiring, the smaller the penalty. In fact, beginning with the 47-52 employee size range, the confidence intervals always include zero.

In distinguishing between less established and already-successful startups in our subsequent analyses, we therefore focused on a 50-employee threshold. We considered individuals hired after a startup had grown to 50 or more total employees (but before the firm reached five years of age) as "successful startup" hires. Firms with 50 or more employees account for roughly 4% of employers but 58% of employment in Denmark. Only 2% of startups reach that scale before their fifth year of operations. But those firms account for 33% of startup employment.

Returning to Table 1, the fourth column splits startup employees into these two sets (column two shows the separation for the random sample). We similarly split established employers into two size categories: those with fewer than 50 employees and those with 50 or more employees. We treat the latter as the baseline. Consistent with the splined estimates in Figure 1, the long-term earnings penalty associated with startup employment stems entirely from the employees who joined smaller startups. Those hired by already-successful startups that had at least 50 employees, actually earned a small premium over the subsequent decade as compared with people who joined established firms.

The final column of Table 1 adds nearly all forms of social payment to the dependent variable. Denmark has a generous social support system. The state provides payments to individuals in unemployment, on parental leave, and on long-term disability. These payments help to mitigate the negative income consequences of being out of the labor force for long periods of time. The inclusion of these social payments reduces the income penalties associated with being an early startup hire and increases the income premium for those joining already-successful startups. As we detail below, this is unsurprising, as startup employees experience more and longer spells of unemployment. Although the inclusion of this income does not change any of the qualitative conclusions of our analyses, across any of the models, including social payments in income consistently compresses the differences between the long-term income of the early startup hires versus the hires of already-successful startups by 30%-40%.

Differentials in pay trajectories. The estimates in Figure 1 and in Table 1 report average differences over a ten-year period. But wage trajectories matter as well. After an initial penalty for startup employees, the different groups may converge in their pay over time. Figure 1 examines how the earnings differentials unfold for the matched sample. We created this figure by estimating a separate coefficient for each group for each year post-treatment—ten coefficients for each category (e.g., one year after being hired, two years after being hired, etc.). Overall, the penalties associated with becoming an early employee of a startup appear relatively stable—rather than declining, if anything, they increase over time. In analyses on longer panels, the penalties associated with being an early startup hire persist for at least twenty years (the maximum length that our sample allows us to explore). In contrast, the pay premiums associated with being hired by an already-successful startup appear short-lived, converging to a level indistinguishable from zero after only four years.

Income dispersion. We also explored whether startup employees had a more dispersed earnings distribution than the employees of large, established firms. Even though the average startup does not succeed, many do, some of them spectacularly. One might therefore expect a fatter right-hand tail in the earnings distribution of startup employees, as has been found for startup founders. Our matched sample shows no such skew. Figure 2 depicts the probability density distributions of ten-year aggregate earnings (in Danish kroner) for early startup hires, for those hired by successful startups, and for the matched sample of employees joining large, established firms.[10] The solid (long-dashed) line displays the distribution for those who became employees of startups before (after) those firms had reached 50 employees while the short-dashed line displays it for the matched individuals hired by large, established firms. Although this finding might seem surprising, two things are worth noticing. First, the most successful startup employees do very well, earning millions of dollars over the ten-year period. At the same time, individuals who climb to the top in corporate ladders can also do well financially.

[10]Over the period being analyzed, the DKK exchange rate to the USD averaged about 6.5:1. Note that the graph truncates the top 1% of the distributions so that the scaling of the horizontal axis can better depict the majority of the mass.

Growth-oriented startups

In the literature on the earnings of entrepreneurs, it has been crucial to distinguish between individuals who hope to build large enterprises ("growth entrepreneurs") versus those interested simply in a more satisfying, and perhaps easier, means of making a living ("lifestyle entrepreneurs"). Entrepreneurs become their own bosses. They can set their own hours and have a schedule that better accommodates other activities, including time with friends and family. Indeed, entrepreneurs regularly report higher levels of job satisfaction (Blanchflower and Oswald 1998; Benz and Frey 2008; Binder and Coad 2013). Entrepreneurs, therefore, might happily exchange a loss of income for a more flexible or more satisfying job, a compensating differential.

Research on entrepreneurs that has attempted to distinguish between these two groups has consistently found that, while lifestyle entrepreneurs earn less than they would as employees, growth-oriented entrepreneurs earn more (e.g., Levine and Rubenstein 2017; Sorgner et al. 2017). Might a similar distinction account for some of the earnings penalties associated with becoming an employee at a small startup? Perhaps many early hires join startups with no real expectation that they will grow, expecting non-pecuniary rewards instead (Roach and Sauermann 2015; Sauermann 2018). Individuals who join growth-oriented startups, in contrast, might experience some of the career and income benefits that have been observed in the founders of these firms.

One of the difficulties in trying to assess this question, however, stems from the fact that registry data do not capture any information on entrepreneurial ambition. In the studies of entrepreneurs cited above, researchers have distinguished between lifestyle and growth-oriented entrepreneurs on the basis of being incorporated or having hired employees (e.g., Levine and Rubenstein 2017; Sorgner et al. 2017). But on those criteria, *every startup* in our sample would qualify as growth-oriented. Because we do not include founders in our analyses, our estimates of the long-term earnings consequences only include incorporated firms with at least one full-time, non-owner employee.

What other information might point to the ambitions of the entrepreneur? One useful proxy

might be the industrial segment that a startup chooses to enter. Certain businesses, like a retail shop or software programming, can survive while remaining small. There are certain industries , however, where scale is a precondition for success, such as shipbuilding or energy. While not deterministic, it is much more likely that an entrepreneur starting a firm in such an industry has the intention to grow. Following this rationale, we calculated the median size for all firms in each four-digit industrial class. We then restricted the analyses to employees moving to employers in industries where the median firm had at least 50 employees. Although they might still start small, entrepreneurs who start businesses in these industries probably hope for their firms to grow large enough to survive.

The first two columns of Table 2 report the average differences in earnings for this subset of industries. Matching appears even more important in this subset of industries than in the population as a whole. The third column, which focuses on the matched sample, shows that employees who join startups that have not yet grown, but operate in industries that require scale, still experience a long-term earnings penalty. This effect, however, appears about one-third smaller in magnitude than in the population of startups as a whole. Those hired by already-successful startups, moreover, earn an even larger premium over the subsequent decade, of more than 4%. These estimates seem consistent with the premiums found by Kim (2018) for successful growth-oriented startups in the United States.

Sorting effects might be particularly important for growth-oriented startups, which typically hire from the right-hand tail of the human capital distribution (Kim 2018). We therefore estimated another set of models, including only individuals who held post-graduate degrees (see Table 2). On average, these highly-educated individuals had smaller long-term earnings penalties associated with a stint of startup employment. But much of this difference stemmed from the fact that they had a higher propensity to join already-successful startups (see Appendix). In the matched sample estimates, the final column of the table, these highly-educated individuals appeared to suffer even larger long-term earnings penalties from being an early employee of a startup. They also received no apparent premium for joining a successful startup, relative to what they would have made in a

large, established firm.

Instrumental variables as test for sorting on unobservables

The analysis so far has sought to account for most selection effects based on differences in individual ability. The triplets approach, which carefully matches individuals to others with similar observables and prior earnings, together with analyses restricted to growth-oriented startups and individuals with higher human capital, should eliminate most stories based on sorting of employees to firms. These results, however, are still likely affected by sorting on other unobservable factors, such as a preference for the flexibility, autonomy, or dynamism associated with startup employment. To test for the extent to which this may be the case, we used estimation with instrumetal variables as an additional robustness check.

An instrument uses variation unrelated to the outcome to estimate the effect of a treatment (Morgan and Winship 2007). As instruments for becoming an employee in a startup (or a small, established firm), we used the proportion of hiring occurring in particular categories (e.g., early startup hire) in an industry and region at the time when the focal individual changed employers.[11] To eliminate reverse causality, we did not include the job entered by the focal individual when calculating these proportions.

We build on the idea that most people search for jobs in specific regions, at specific times, and in specific industries. The supply of jobs, however, varies across places, types of firms, industries, and time. With a similar structure to a Bartik-style instrument (Bartik 1991; Goldsmith-Pinkham et al. 2018), our instrument identifies the effects of becoming a startup employee based simply on the jobs available in a given industry at a given place and time. One might rightly worry that the demography of employers in an industry-region would influence pay (e.g., Haveman and Cohen 1994; Sørensen and Sorenson 2007), violating the exclusion restriction. But the *flow* of job openings in any particular year, which constitute our instruments, are essentially uncorrelated with the overall demography of employers, or the *stock* of jobs ($r \leq .03$).

[11] As regions, we use the 79 labor markets developed by Andersen (2000).

Table 3 reports the results of these IV estimates on our matched sample. The first three columns present the first stages for these models. As one would want, in each case, the prediction of the endogenous variable loads primarily on the associated instrument. All of the instruments have t-statistics in excess of 100 for the variables that they instrument and, overall, the instruments explain roughly 20% of the variance in whether individuals become employees at startups versus more established employers.[12]

The second-stage coefficients suggest that our results are unlikely to be driven by sorting on unobservables. If anything, the estimates in Tables 1 and 2 may underestimate the penalties associated with becoming an early employee of a startup. In our instrumental variables estimation, employees who join a startup before it has grown have an income penalty of roughly 14%. Hires of already-successful startups, by contrast, experience a boost in earnings of about 4%, similar in amount to that seen in growth-oriented startups. Note, however, that the 95% confidence intervals around these point estimates overlap with the confidence intervals for the coefficients reported in Tables 1 and 2. One therefore cannot reject the null hypothesis that the instrumental variables produce equivalent estimates of the effect size.

WHAT ACCOUNTS FOR THE PAY PENALTY?

Startup employees experience a long-term earnings penalty, except those who join after a startup has experienced a measure of success—growing to more than 50 employees—who in fact earn slightly more in the long term. The registry data do not include the type of fine-grained information required to understand the ultimate sources of these disparities, but the data do allow us to explore a set of proximate factors that should relate to these ultimate causes.

A series of qualitative interviews with current and former employees of dozens of startups, conducted in parallel with the first rounds of the archival data analysis, led us to explore three

[12]The Angrist-Pischke F-statistic has values ranging from 6,113 to 8,888. Although no critical values are available for this test, it has the same distribution as the F statistic for which Stock and Yogo (2005) calculated critical values. Our first stages exceed their minimal recommended critical values of 10-20 by two orders of magnitude.

proximate causes for the earnings penalty experienced by early startup employees: (i) employment uncertainty created by precarious positions, (ii) ruined reputations from a higher likelihood of firm failure, and (iii) constrained job mobility after leaving a startup. We explore these factors using the matched sample in Table 4, with the first column serving as the baseline model.

Precarious positions and employment uncertainty

Startups face much higher levels of uncertainty. They fail at higher rates than older firms (Freeman et al. 1983; Carroll and Hannan 2000). They also need to correct course more frequently (which often entails chaning their internal configuration). When they do, employees can unexpectedly find themselves out of jobs (Carroll et al. 1992; Haveman and Cohen 1994). Job loss due to the failure or transition of a startup, moreover, may prove even more problematic than job loss due to a plant closing or a mass layoff. Since startups seem particularly vulnerable to economic downturns, the (former) employees of startups may find themselves searching for a job at precisely the times when those jobs are most scarce, potentially leading to more prolonged spells of unemployment and to dampened earnings potential (Schwandt and von Wachter 2019).

To assess how job uncertainty at startups might affect long-term earnings, we included a variable that measures, for each individual, the proportion of each year spent in the labor force. *Time in labor force* divides the number of days worked (across all employers) by the total number of possible workdays for the year. Column 2 reveals that this variable accounts for 80% of the effect for being an early employee of a startup.

Note also that the coefficient for time in labor force substantially exceeds one. The negative earnings effects of unemployment therefore do not stem solely from lost wages—in which case the coefficient would have a value of roughly one (because it has been scaled to the proportion of the year in employment). Unemployment due to job loss imposes a penalty on employees even after they secure another job. A number of factors might account for this effect. Career advancement and pay increases often depend on tenure within a given firm (Bidwell and Briscoe 2010; Bidwell and Mollick 2015). Every time an employee of a firm changes jobs their tenure clock resets to

zero. They must start over. The former employees of failed firms also have no backup option and therefore less bargaining power. They probably end up settling for positions that pay less, perhaps even less than their previous jobs. Each setback, however small, builds on those before it, leading to increasingly-divergent income paths.

Firm failure and ruined reputations

Unemployment, even when due to firm failure, may also create a stigma, a sense that the job loss must reflect something about the individual. Rider and Negro (2015), for example, found that the employees of a failed firm experienced substantial downward mobility in their job searches, moving to less desirable employers. In firms with a small number of employees, outsiders may perceive each individual as being even more responsible for the fate of the firm. The former employees of failed firms, particularly of small failed startups, may therefore find themselves disadvantaged when competing for jobs.

To capture the reputation effect associated with being employed at a failing firm, we included two additional variables. One, *left firm*, captures the general effects of leaving an employer and of shortened firm tenure. It turns on when individuals leave their original employers and remains turned on in all subsequent years. The second, *left failed firm*, captures the effects of leaving a firm close to its time of failure. It has a value of one—and remains one for all subsequent years—when an individual departs a firm in the same year that the firm ceases to exist. This second variable should allow us to separate the effects of any potential stigma associated with firm failure from the more general negative consequences of unemployment.

The third column reports a model that includes these variables. In general, moving across employers has a negative, though very small (less than 1%), effect on income. The effects of leaving a firm just before or as it fails, however, appears slightly positive. Individuals who remained on board until the very end earned just over 1% more in subsequent years. Notice, however, that the inclusion of *left firm* and *left failed firm* has a negligible influence on the point estimates for the long-term earnings effects of startup employment.

Constrained job mobility after startup employment

Startups, particularly before they reach a sufficient size, have less role differentiation and job stability. On the one hand, this can mean broader work experiences that can translate to a "jack of all trades" profile, attractive for managerial roles (Lazear 2005). The flipside of engaging in a large number of activities, however, is spending less time learning each one. The employees of startups also often find themselves performing atypical jobs that aggregate idiosyncratic sets of activities (Burton and Beckman 2007). The inherent instability of startup jobs compounds these issues. Because the needs of the firm shift over time, the scope of jobs within the company fluctuate. The early employees of startups may therefore end up disadvantaged in the development of the specialized experience valued by large firms (Rosen 1983).

The evidence presented so far is inconsistent with the notion that a startup experience would make employees valuable for higher positions in established firms. In contrast, if broad-but-shallow work experiences result in idiosyncratic jobs, then the early employees of startups may appeal primarily to other early-phase startups or to other small firms. Even if individuals first become the early employees of startups by chance, this endogenous human capital formation might restrict their ability to move to jobs in larger firms.

Alternatively, startup employees may prefer to work for a startup or small firm. Even if they do not initially sort into startup employment for that reason, experience in a startup might lead individuals to develop a preference for being in a younger or smaller firm. The fact remains, however, that larger firms tend to pay more (Oi and Idson 1999), and wage progression is most effectively achieved by moving up the hierarchy across large organizations (Bidwell and Briscoe 2010). Therefore, regardless of whether startup employees are less attractive to large firms, or large firms are less attractive to former startup employees, being employed by only small firms could have lasting effects on the earnings of these individuals.

To assess this possibility, the final model accounts for the characteristics of the current employer. Being an employee of a small startup has a large negative effect of earnings. Being an employee

of a small, established firm also comes with a pay penalty. Once one includes these measures, however, the residual long-term effect associated with having been an early employee of a startup shifts to being positive, though small (about 2%). Put differently, startup employees who later find themselves working in large, established firms, seem to experience a small, long term wage premium.

Although the estimates in Table 4 suggest that the characteristics of current employers account for an important piece of the penalty associated with being an early employee at a startup, they do not give us a sense of the strength of the apparent path dependencies. We borrow a log-linear modeling technique from stratification research, used to describe educational and occupational mobility, to examine mobility across different types of employers (for details, see the Appendix). Firm-to-firm mobility is restricted both in terms of firm size and firm age, but more so in terms of size. An individual has about an 85% higher probability than one would expect in random mixing of moving to a firm of similar age but a more than 140% higher probability of moving to a firm of similar size.

The observed patterns of firm-to-firm mobility, therefore, reveal strong path dependence in employer age and size. The early employees of startups, in particular, tend to move to other small startups and to small, established firms. Those employed at large, established firms, in contrast, circulate among these higher-paying employers.

Career and wage trajectories of startup employees

We can use these proximate mechanisms and the information underlying our analyses of firm-to-firm mobility to describe a variety of potential paths experienced by those hired by startups and by other employers. Table 5 details the most common career paths and the average predicted long-term earnings associated with those trajectories.[13]

For the early employees of startups, the most probable sequence, experienced by roughly 45%

[13]Note that the paper does not include all possible paths. Some trajectories that apply to less than 5% of individuals have been omitted from the table.

of these individuals and associated with a long-term pay penalty of about 1.5%, has the startup surviving but failing to grow, leaving these individuals employed in mature but small firms. If, however, the startup grows large, these early employees enjoy a boost in their long-term earnings of roughly 2%. In this sense, the popular wisdom about the long-term benefits of getting in on the ground floor appear to hold. But these outcomes are relatively uncommon, occurring in only about 3% of cases.

More common paths for these early startup hires involve transitions to other small employers, either another small startup (27% of cases) or a small firm that has been operating for more than four years (12% of cases). Both of these trajectories predict large long-term pay penalties, of 21% on average for those moving to small startups and of 12% for those moving to more established small employers. These penalties emerge in part because those transitioning to other jobs, on average, experience a spell of unemployment first.

Even those who become entrepreneurs, roughly 7.5% of these early employees, earn about 22% less, on average, than their observationally-equivalent peers in large, established firms. That figure includes earnings from the dividends and capital gains associated with their startups. Although firms founded by former startup employees have been found to survive longer (e.g., Sørensen and Phillips 2011), that experience does not appear to translate into stronger financial performance.

By contrast, the majority of paths available to those joining startups that have already been suceeded enough to reach a substantial size – remaining at the firm, moving to another successful startup, or moving to a large established firm – predict long-term earnings premiums. Interestingly, even as entrepreneurs, these individuals have the highest odds of financial success. Having access to successful organizational blueprints may trump the breadth of experience gained in smaller firms (Burton et al. 2002; Phillips 2002).

The consequences of employment at a small, established firm, by contrast, differ from those of being an early startup hire. Employees who join small, established firms tend to stay in their jobs longer and are less likely to experience extended spells of unemployment. If they do move, they tend to go to other small, established firms rather than to startups.

DISCUSSION

For most of the twentieth century, industrial policy focused on fostering the growth of large, established enterprises, titans of the global economy, with the idea that these juggernauts would provide good jobs and promote economic expansion (e.g., Johnson 1982; Dertouzos et al. 1989). But beliefs have changed. Policymakers increasingly see entrepreneurs, not established enterprises, as the engines of economic growth (e.g., Liberto 2012; Stemler 2013; OECD 2017). Changes in regulation and public support have followed, directing more and more resources, attention, and favorable regulation toward startups. Largely absent from this shift in orientation, however, has been a consideration of how these changes might affect the career trajectories and long-term earnings prospects of employees.

Using Danish registry data, we examined the long-term earnings associated with being hired by a startup. On average, those hired by startups earn substantially less over the long term than their counterparts at more established firms. Roughly half of this earnings differential, however, stems from the sorting of individuals into organizations. Similar to what one sees in other parts of Europe and in the United States (Nystrom and Elvung 2014; Ouimet and Zarutzkie 2014), startups in Denmark hire younger and less educated individuals who had been earning less in their previous jobs. In other words, these firms disproportionately hire those who would probably earn less at any employer.

Income trajectories also vary considerably depending on the size of the startup at the time that the employee joined it. Individuals who became one of the first 50 employees of a startup earned 7% to 14% less over the subsequent ten years relative to peers employed at large, established firms. By contrast, those hired by successful startups that had grown to at least 50 employees in their first four years, earned a long-term earnings *premium*, of 2% to 4%.

Only a small fraction of startups then provide attractive career opportunities, at least in terms of long-term financial rewards. Even though these successful startups account for almost one-third of startup employment, only about 2% of startups ever reach a size of 50 employees. Much as the

literature on "gazelles" points to the fact that a small proportion of startups account for most job creation (Henrekson and Johansson 2010), our results suggest that these same successful startups also create the most attractive jobs in terms of their earnings prospects.

But it remains unclear how early one can identify these promising startups. Coad et al. (2016) have argued that "the fog is thick" around these future growth and survival prospects. Consistent with the idea that potential employees have difficulty predicting the success of startups, we found large long-term earnings penalties associated with being early startup employees, even among the well educated and in settings where the entrepreneurs almost certainly have ambitions to become large.

Two proximate mechanisms, precarious positions and constrained job mobility, appear to account for our observed effects. First, those in startups, particularly small startups, have less stable jobs. They experience more frequent and longer spells of unemployment. Even when they find work, they fall behind in income relative to those continuously employed. Second, those who become the employees of startups before they reach sufficient scale appear to find themselves segregated into employment in small firms. These tracked transitions help to explain both a portion of the lost earnings and why those caught in lower-paying jobs do not leave them.

The limitations of our data do not allow us to identify the ultimate causes behind our observed mechanisms. But existing research on labor market dynamics provides strong theoretical foundations for interpretation. Both of the identified proximate mechanisms seem to best relate to a common ultimate cause: Because of their small scale, their inchoate roles, and their ever-changing environments, startups often create idiosyncratic jobs, closely aligned to the needs of the organization but disconnected from the external labor market (Burton and Beckman 2007). The early employees of startups therefore seem to develop sets of skills and acquire experiences that have limited value outside of their current employer and which may not even have that much value to the startup as it evolves into a more bureaucratic organization. Further progress on understanding the mechanisms and ultimate causes, however, will require detailed information both on what a large set of startup employees do on a day-to-day basis and on their experiences after leaving their

original employers.

Our results nevertheless raise an obvious question: Given these long-term earnings consequences, why would anyone become one of the first employees of a startup? We see at least three possible explanations.

First, employees may see startup employment as a form of compensating differential. Startups offer their employees more interesting jobs, thanks to the variety of activities (e.g., Roach and Sauermann 2015). The smaller size of these firms may also foster camaraderie – social cohesion – among early hires. Individuals may even come to appreciate these advantages in the course of being an early employee—that is, rather than selecting into these firms as a result of having a particular set of preferences these tastes may emerge endogenously from their work experiences. Regardless of the particular source of satisfaction, employees may happily earn less in exchange for these psychological benefits.

But a second possibility is that people simply do not have a good sense of the pros and cons of startup employment. These earnings differences emerge over time. Given the noisiness of career sequences and the time between cause and effect, people might find it difficult to recognize the penalties associated with being an employee in a small startup. Noise may drown out the signal. Individuals may also have a tendency to overweight the positive outliers which appear to receive most of the press attention. And the notion that startup employment can generate long term benefits is rooted in some truth—but only for the winners of an extremely unlikely tournament, who probably end up being the most visible. Similar dynamics, in fact, have been observed in other career trajectories, notably those of academics (e.g. Sauermann and Roach 2016; Freeman et al. 2001).

A final possibility is that many employees do not choose their employers. Although students about to leave school often entertain multiple job offers simultaneously, post-graduation, job searches generally do not follow a standardized calendar. Employees and job searchers receive offers one at a time. Instead of choosing among an exhaustive set of options, they decide whether to accept a job or to decline it and continue searching. The asynchronous and sequential nature

of this search process introduces considerable friction into the matching of employees to employers (Mortensen 2004). Moreover, even fully aware of the consequences, job searchers may prefer an offer on the table from a small startup to the uncertain prospect of something better. Although distinguishing between these explanations lies outside the scope of the current paper and will require more detailed information on the job search process, it represents an important question for future research.

To exploit the high quality of its registry data, our research has focused on Denmark. Would other countries exhibit similar patterns? We suspect that many would.

Research on the short-term earnings effects of joining startups, for example, has revealed surprisingly consistent effects, and even effect sizes, across Denmark, Germany, Sweden, and the United States (Schmieder 2013; Nystrom and Elvung 2014; Burton et al. 2018; Babina et al. 2018). Babina et al. (2018), for instance, confirm that the United States has almost the same gross earnings differential between startups and more established firms and that the sorting of employees to employers accounts for almost exactly the same proportion of this differential as Burton et al. (2018) found in Denmark.

But at least three country-level factors might influence the size of any earnings penalties associated with being an early employee of a startup. The first concerns the relative stature of startups in the economy. Startup employment may prove more costly when changing employers in countries, such as Spain and Japan, where people have stronger preferences for large, established employers, than it would in places, such as Ireland and the United States, where entrepreneurship has been celebrated. Denmark, on this dimension, appears somewhat in the middle, with Danes giving greater esteem to entrepreneurs than those in Finland, Germany, or Spain but not as much as those in Ireland or the United States (Schøtt 2007).

A second factor involves the strength of the social support system. Denmark has a strong safety net and relatively uniform benefits across employers. But in countries where benefits vary from one employer to the next, one might expect even larger disparities to emerge between the early employees of startups and those in large, established firms. Benefits such as health care

insurance and retirement plans in the United States vary with firm age and size, with older and larger firms offering better fringe benefits (Kalleberg et al. 1996; Litwin and Phan 2013). Spells of unemployment also prove more costly when these benefits depend on employment, leading the involuntarily unemployed to settle for less attractive jobs (Nickell 1997).

A final factor may involve the extent to which firms compensate their employees with equity. In Denmark, few startups offer stock or options. But in the United States and some other countries, equity awards have been more common (Hand 2008). Equity has the potential to tie long-term compensation more tightly to the success of startups, perhaps creating a fat right-hand tail. But, even in the United States, the odds of these awards paying out end up being very low. Even when they do pay out, they typically have little value. The press loves to cover the janitor or receptionist who became rich from being employed at a high-tech startup. But these events are as likely and as representative of the common experience of startup employees as is the multi-million-dollar lottery winner among those buying tickets.

To a large extent, this paper represents a first step. We need to understand better how these patterns vary across countries, what ultimate causes account for them, and why some but not others end up being employed in startups. Our findings suggest that all of these topics represent important questions for future research.

But they also suggest a need for caution. Most people rely on employment for income and wealth. Even if by chance, becoming the early employee of a startup has large negative earnings penalties, similar in size to not completing a college degree.[14] Our results therefore provide a cautionary tale for job candidates entertaining multiple offers and for policymakers harboring unfettered enthusiasm for entrepreneurship as the primary engine of job creation.

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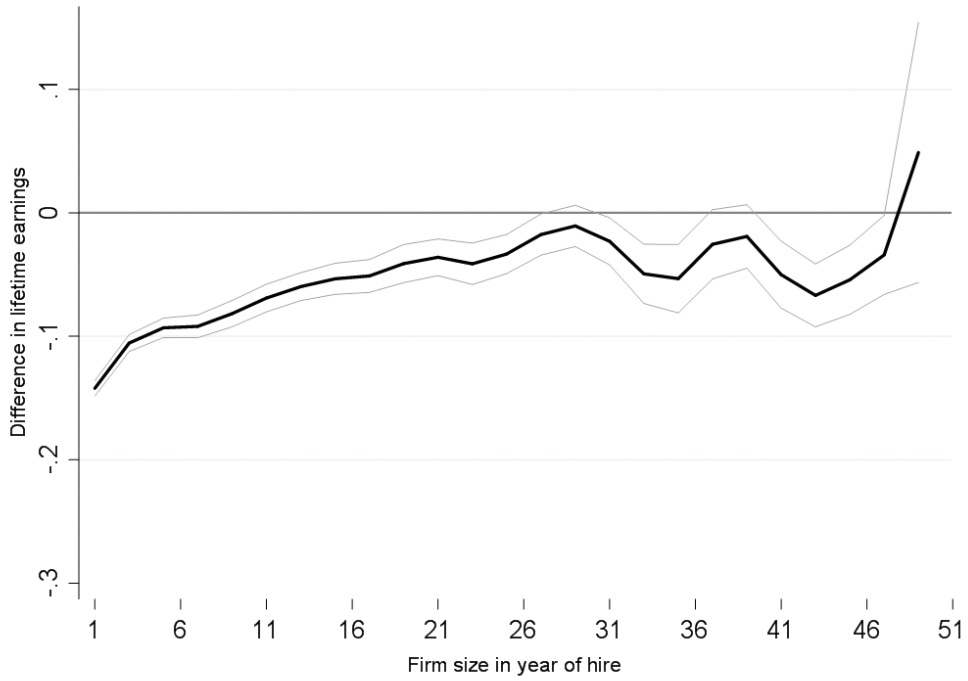


Figure 1: Long-term earnings based on startup size at the time of hiring

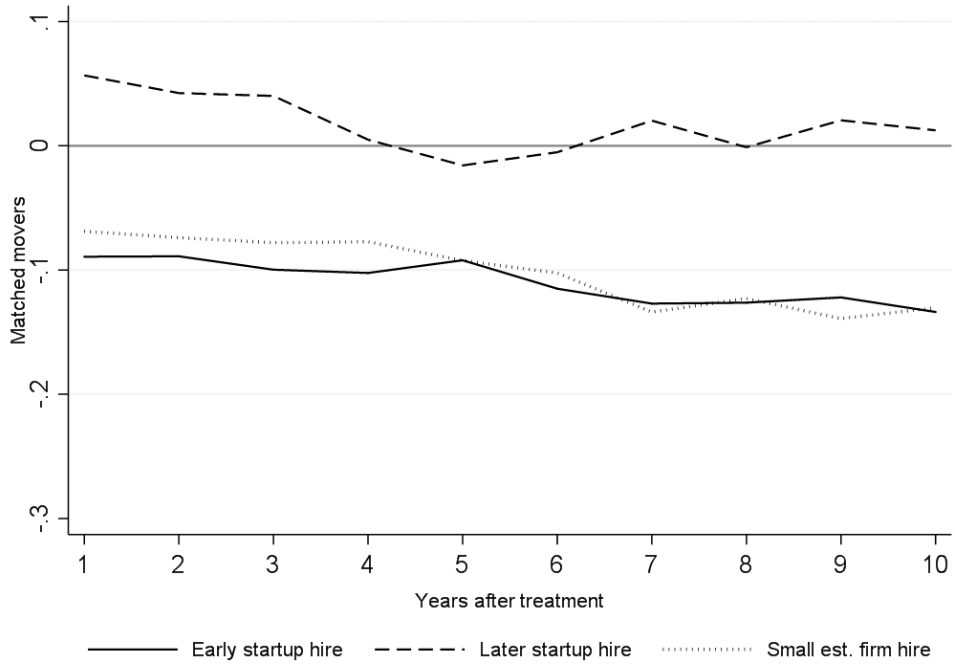


Figure 2: Earnings trajectories for the matched hires of startups and of small, established firms relative to those of large, established firms

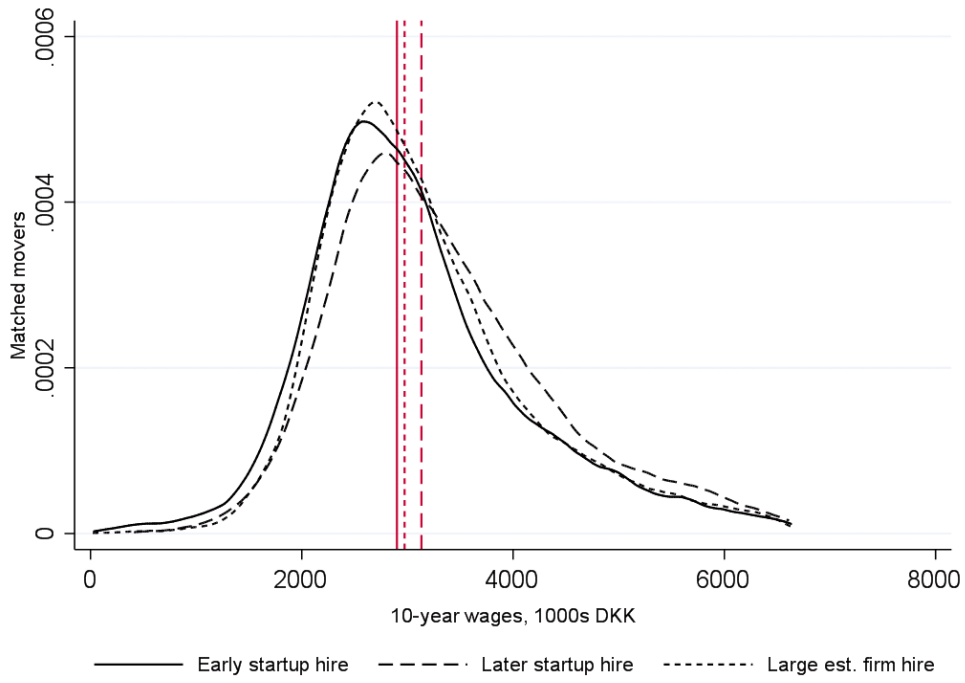


Figure 3: Ten-year accumulated earnings for early and later startup hires versus the matched hires of large, establish firms

Table 1: Estimates of long term differential in earnings

	Random	Random	Matched	Matched	Matched (w/ social payments)
Startup hire	-0.184**		-0.081**		
	(0.002)		(0.004)		
Small startup hire		-0.287**		-0.105**	-0.054**
		(0.002)		(0.004)	(0.006)
Large startup hire		-0.066**		0.022**	0.031**
		(0.003)		(0.007)	(0.009)
Small, est. firm hire		-0.317**		-0.093**	-0.042**
		(0.002)		(0.004)	(0.005)
Treatment year FE	Yes	Yes	No	No	No
Triplet FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2,017,476	2,017,476	1,440,984	1,440,984	1,440,980

Table 2: Estimates of long term differentials in earnings (subsets)

	Large-firm industries			Graduate degree holders		
	Random	Matched	Matched	Random	Matched	Matched
Startup hire	-0.187**	-0.034**		-0.161**	-0.115**	
	(0.002)	(0.008)		(0.006)	(0.015)	
Small startup hire			-0.065**			-0.138**
			(0.010)			(0.018)
Large startup hire			0.044**			-0.019
			(0.013)			(0.018)
Small, est. firm hire			-0.036**			-0.225**
			(0.008)			(0.021)
Treatment year FE	Yes	No	No	Yes	No	No
Triplet FE	No	Yes	Yes	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,154,168	623,660	623,660	137,756	75,026	75,026

Table 3: Instrumental variables regression estimates of the long-term effects on logged income

	First stages			Second stage
	Early startup	Later startup	Small, est.	
Small startup instrument	0.837**	0.020**	0.338**	
	(0.007)	(0.002)	(0.007)	
Large startup instrument	0.106**	1.046**	0.103**	
	(0.005)	(0.008)	(0.006)	
Small, est. instrument	0.246**	0.016**	0.874**	
	(0.005)	(0.001)	(0.006)	
Small startup hire				-0.152**
				(0.015)
Large startup hire				0.041**
				(0.012)
Small, est. hire				-0.159**
				(0.013)
Year dummies	Yes	Yes	Yes	Yes
F statistic	6,853	6,113	8,888	

Based on 106,741 triplets over 1,367,962 person-years. * p <0.05, ** p <0.01.
Clustered standard errors in parentheses.

Table 4: Exploring the mechanisms underlying the long-term differences in logged income

	1	2	3	4
Small startup hire ($t = 0$)	-0.105**	-0.022**	-0.021**	0.020**
	(0.004)	(0.003)	(0.003)	(0.003)
Large startup hire ($t = 0$)	0.022**	0.041**	0.041**	0.044**
	(0.007)	(0.005)	(0.005)	(0.005)
Small, est. hire ($t = 0$)	-0.093**	-0.032**	-0.031**	-0.006*
	(0.004)	(0.002)	(0.002)	(0.002)
Time in labor market		1.852**	1.851**	1.801**
		(0.008)	(0.008)	(0.008)
Left firm			-0.007**	0.002
			(0.002)	(0.002)
Left failed firm			0.013**	0.011**
			(0.004)	(0.004)
Small startup (t)				-0.130**
				(0.004)
Large startup (t)				0.004*
				(0.002)
Small, est. (t)				-0.035**
				(0.002)
Triplet FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,440,984	1,393,525	1,393,525	1,317,322

* p <0.05, ** p <0.01. Clustered standard errors in parentheses.

Table 5: Common career trajectories for startup employees

	Proportion	Earnings
Small startup hire		
Stay at startup, remains small	45%	-1.5%
Stay at startup, grows large	3%	2.0%
Move to a small startup	27%	-21%
Move to a small firm	12%	-12%
Become entrepreneur	7%	-22%
Large startup hire		
Stay at startup, remains large	43%	4.5%
Move to large firm	25%	3.6%
Move to large startup	16%	4.0%
Move to a small firm/startup	11%	-5.0%
Become entrepreneur	5%	-3.8%
Small firm hire		
Stay at firm, remains small	53%	-4.0%
Stay at firm, grows large	4%	-0.6%
Move to small firm	29%	-10%
Become entrepreneur	5%	-15%
Large firm hire		
Stay at firm, remains large	68%	Baseline
Move to large firm	27%	0%
Become entrepreneur	2%	-9.5%

Appendix

This appendix provides descriptive statistics and analyses to supplement the main text.

Descriptive statistics

Table A1 provides descriptive statistics for our random and matched samples.

Employee sorting

Prior research has found that startups employ different sorts of workers (Ouimet and Zarutzkie 2014; Burton et al. 2018). To assess the magnitude of these differences in our sample, we estimated a set of logistic regressions, characterizing the extent to which each individual-level characteristic predicts being hired by a particular category of employer. The results of these models appear in Table A2. The first column compares those who joined a startup as an early employee to the employees of large, established firms; the second assesses how the later employees of startups differ from the employees of large established firms; and the third compares the hires of small, established firms to those of large, established firms.

Comparing across the columns, one can see that the early employees of startups are younger, more commonly men, and individuals with substantially lower prior incomes than the employees of large, established firms. Even once they reach 50 employees, startups continue to hire more men than large, established firms.

Log-linear analysis

To examine further the idea of path dependency in careers, we borrowed a modeling technique, log-linear analysis, from the stratification literature in sociology. Log-linear models have been deployed to analyze occupational and class mobility, both over time and across generations (See Torche (2015) for a recent review). For more technical information about the statistical technique see (Fienberg 2007).

These analyses begin with a cross-classified table of counts, often called a contingency table, with rows that represent the origin state and columns that represent the destination state. Under the null hypothesis of independence, one would expect each cell in the table to contain a proportion of the cases equal to the product of the marginal distributions. In other words, imagine that the rows and columns represented two sequential flips of a coin. In that case, the table would have two rows and two columns, with each state having a probability of 0.5. If one flip did not influence the next one, then each cell should contain roughly 25% of the cases ($= .5 \times .5$).

In the context of mobility, the observed cell counts represent the total amount of class or occupational mobility. Researchers examine the proportions of people who are immobile – beginning and ending in the same category – versus those who are upwardly or downwardly mobile. Stratification researchers have argued that total mobility has two distinct components: a structural component and a relative component. The structural component stems from exogenous factors that shape the marginal distribution over time. For example, the shift from a manufacturing economy to a service economy changes the relative numbers of white-collar versus blue-collar jobs. The relative component captures mobility net of structural constraints. It measures the association between origins and destinations, allowing researchers to explore the extent to which those from different origins have equal opportunities.

Log-linear models usefully allow researchers to explore different forms of interdependency between the cases, conditional on the marginal distributions of the origin and destination states. In the coin flipping example, one could estimate the tendency for one side of the coin to repeat. In class or occupation studies, researchers often study inheritance or persistence, sometimes referred to as quasi-independence, which assumes a higher probability of remaining in the origin state than in moving to a different destination.

We are interested in mobility between different categories of firms. At the beginning of a year, employees could be employed in any one of four types of firms: small startup, large startup, small firm, or large firm. They could also be employed in any of those four types of firms at the end of the year. Each year, therefore, has a corresponding 4×4 contingency table.

Given our multiple years of data, one option would be to aggregate all of the data to produce a single two-dimensional table (in this case, with four rows and four columns). But another option, which we pursue, involves creating a three-dimensional table, stacking the two-dimensional tables for each year. This disaggregated approach has some advantages in terms of better accounting for the marginal distributions, the availability of jobs in particular types of firms from one year to the next. We therefore have a contingency table with 320 cells $= 4 \times 4 \times 20$.

We actually constructed two different contingency tables. In the first, people who stay in the same firm year after year contribute to the cell totals. But they could still move to a different firm type if their employer matured or grew into a different category. The second table only includes individuals who changed employers in the cell counts (and the marginal distributions).

We first estimated the independence model. In other words, how much of the variation in the cell counts stems simply from the marginal distributions, the number of jobs in different sorts of firms at the beginning and end of the year? We then estimated more complex models to assess the extent to which employees tended to remain within employers of the same age category (startups versus established firms) and of the same size category (smaller or larger than 50). We also estimated models which allowed for age and size to interact in predicting the destination state. We estimated the log-linear models using maximum likelihood Poisson regression.

Table A3 summarizes the results of these models. Because our models include such large counts in each cell, the traditional measures of model fit for log-linear analyses – the chi-squared tests – would almost never suggest that one should prefer a more parsimonious model. We therefore report and focus on the amount of variance explained by each model.

Beginning with the independence model, which essentially tells us the degree to which the observed patterns in the relationships between the origin and destination states stem simply from the distributions of employment opportunities available from year to year, can account for roughly 58% of the variance in employer age-size transitions and 76% of the age-size mobility across firms. A very large share of the movement of individuals across employers of varying ages and sizes therefore appears to stem from the opportunity structure—that is, the distribution of jobs available.

The second and third models include terms for age and size inheritance, respectively—the odds that an individual remains with an employer of small size or young in age. Comparing these models, which each use one additional degree of freedom, back to the baseline conditional independence model, we see from the relative increases in the explained variance that firm size predicts more of the variance in employee transitions than firm age. The inclusion of these two terms at the same time explains almost all of the remaining variance (Model 4).

More complicated log-linear model specifications only add marginally to the fit of the model. Model 5, for example, adds an age-size interaction term, to capture the extent to which transitions occur primarily within an age-size quadrant (e.g., only from small startups to other small startups). Although this term does improve the model fit, it only captures another 1% of the variance above and beyond the main effects of age and size. More generally, Model 4 already explains more than 92% of the variance for job changers using only 29 degrees of freedom, 26 of which simply capture the marginal distributions. Because a fully saturated model would involve interactions between the inheritance terms and each year, it would require roughly 300 additional terms to explain the remaining 3% to 8% of the variance.

Table A1: Descriptive statistics

	Random sample mean/sd	Matched movers mean/sd
Ln wage	5.67	5.55
	(0.76)	(0.81)
Small startup hire	0.13	0.13
	(0.34)	(0.33)
Large startup hire	0.03	0.03
	(0.17)	(0.17)
Small, est. hire	0.17	0.17
	(0.37)	(0.37)
Large, est. hire	0.67	0.68
	(0.47)	(0.47)
Time in labor market	0.91	0.88
	(0.23)	(0.26)
Left firm	0.62	0.62
	(0.48)	(0.49)
Left failed firm	0.05	0.06
	(0.22)	(0.23)
Small startup (t)	0.07	0.08
	(0.26)	(0.28)
Large startup (t)	0.09	0.09
	(0.29)	(0.29)
Small, est. (t)	0.19	0.22
	(0.39)	(0.42)
Large, est. (t)	0.59	0.54
	(0.49)	(0.50)
Observations (person-year)	2,017,476	1,440,984

Table A2: Logistic regression estimates of sorting by age-size categories (relative to large, old firms)

	Early startup	Small, old	Later startup
Age	-0.041**	-0.041**	-0.024**
	(0.001)	(0.000)	(0.001)
Male	0.088**	0.107**	0.136**
	(0.010)	(0.009)	(0.022)
Years of education	-0.004	-0.025**	0.001
	(0.002)	(0.002)	(0.005)
Occupation			
Top management	-0.087*	-0.185**	-0.341**
	(0.035)	(0.031)	(0.073)
Upper white collar	-0.219**	-0.336**	-0.184**
	(0.018)	(0.017)	(0.034)
Lower white collar	-0.425**	-0.405**	-0.288**
	(0.016)	(0.015)	(0.031)
Unskilled blue collar	-0.028	0.046**	0.089*
	(0.018)	(0.016)	(0.036)
Undefined or missing	0.465**	0.338**	0.354**
	(0.014)	(0.012)	(0.030)
Prior wage	-0.273**	-0.281**	-0.003
	(0.007)	(0.006)	(0.017)
Treatment year FE	Yes	Yes	Yes
Observations	322,617	339,341	280,505

* p <0.05, ** p <0.01. Robust standard errors in parentheses.

Table A3: Log-linear analysis of employer age-size mobility

	Label	Specification	d.f.	Year-to-year Pseudo R^2	Job changes Pseudo R^2
1	Conditional independence	$A_o \times S_o + A_d \times S_d$	26	.576	.760
2	Age inheritance	[1] $+A_o \times A_d$	27	.708	.780
3	Size inheritance	[1] $+S_o \times S_d$	27	.833	.880
4	Additive inheritance	[1] $+A_o \times A_d + S_o \times S_d$	28	.961	.914
5	Joint inheritance	[4] $+A_o \times A_d \times S_o \times S_d$	29	.971	.920