Career Effects of Working at a Startup*

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Abstract

We study the effect of working for a startup on workers' subsequent labor market outcomes using matched employer-employee administrative data from Chile. We find that moving to a startup implies an average penalty in earnings of 6.7% over the first five years after joining the firm. Roughly one-half of the reduction in earnings is due to lower average earnings while formally employed and the other half to spending more time out of formal employment. On average, over the five years after joining the firm, workers who move to startups have a two-percentage point lower probability of holding a job, have a lower probability of experiencing job-to-job transitions, and hold fewer jobs, suggesting that people who move to startups have a worse performance on the job ladder than those who move to established firms. We provide further evidence that these negative effects are persistent and vary across workers' and firms' characteristics. When compared to the earnings of an average worker who joins an established firm, the earnings penalty is significantly smaller when we condition on startups who survive (-1.32%). The difference becomes a wage premium for startups that are at the top of the sector size distribution by age 5.

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1 Introduction

Young businesses are essential contributors to aggregate net employment growth. However, most startups fail, and only a small fraction of those that survive grow into large firms.[1] Considering that workers change jobs repeatedly throughout their careers and that a significant fraction of those transitions is toward young firms, the lack of job security they offer can generate substantial negative and persistent effects on the career paths of their employees. On the other hand, a startup worker could end up earning more than a worker at an established firm if the firm grows fast. Even if a startup worker switches to a different job in the future, the employment spell at a startup could help them to climb the job ladder faster. Therefore, the effect of taking a job at a startup on the worker's career path could go either way.

In this paper, we study the short- and medium-term effects of employment spells at young firms on Chilean workers' earnings and job ladder performance. The empirical analysis makes use of Unemployment Insurance (UI) data. These data allow us to keep track of individuals' career paths in terms of the firms they work for, their earnings, and periods of non-employment at formal firms. The UI records in Chile cover formal wage and salary employees enrolled in the unemployment insurance system, similar to the Longitudinal Employer-Household Dynamics (LEHD) data in the United States.

There are three key empirical challenges to identifying the effect of interest. First, by construction, separately identifying the effect of worker tenure and firm age is challenging. Older firms have workers with longer tenure because they have been open for longer. Workers in older firms have had the opportunity to accumulate firm-specific human capital, while by definition, workers at a startup have no experience at the firm. If we were to compare the earnings of workers from established firms and workers from startups, some of the observed differences would come from differences in tenure. To the extent that tenure and firm age move together and are the same for workers who join startups, it is not possible to separately identify these effects. To overcome this identification challenge, we focus our analysis on workers with zero initial tenure, that is, workers who transition to a new job.

Second, the effect of transitioning to a startup can last beyond the employment spell

[1] Throughout this document, we refer to young businesses and startups interchangeably. Haltiwanger et al. (2013) and Decker et al. (2014) provide empirical evidence of the importance of young firms in aggregate job creation and their up or out dynamics in the US.

at the firm. If a firm closes, the worker has to find another job and may spend some time out of formal employment. Startups have higher closing rates than established firms, and unemployment spells have persistent effects on earnings (Jacobson et al., 1993; Couch and Placzek, 2010; Illing et al., 2021). If the firm survives, but the worker decides to leave due to poor firm performance, the future career path of the worker can also be affected by transitioning from a low position in the job ladder. To allow for the possibility of lasting effects, we follow workers for five years after transitioning to a new job and follow their earnings and employment history regardless of subsequent moves.

Third, workers choose whether to move to a startup or an established firm. Workers with different characteristics can have different preferences or different alternatives in their choice sets based on their appetite for risk or other factors. This means that the simple mean difference in earnings between workers who move to a startup versus workers who move to an established firm reflects both differences in worker characteristics and the effect of firm age on earnings. We take pains to compare workers who transitioned to a startup to the most similar workers who transitioned to an established firm to get as close to a counterfactual as we can. Intuitively, what is needed for our results to have a causal interpretation is that, after considering age, gender, nationality, employment history, and previous earnings, the potential outcomes of workers are independent of the type of firm to which they transition. In a setting like Burdett and Mortensen (1998), where there are search and matching frictions, and there is randomness as to the type of firm from which a worker ends up getting a job, this seems plausible. We acknowledge that there are other possible explanations and interpretations of our results, and we discuss them in more detail in Section 4.4.

To carry out the analysis, we built a balanced panel with individuals between 18 and 50 years old who had work experience in the formal sector before that transition and made a job-to-job or non-employment-to-job transition after 2012,and who were observed for at least 60 months (5 years) after the first observed transition. We classify a firm as a startup if it is in its first year of operation–that is, if it has been less than a year since the firm first appeared in the UI system records– and if less than 30% of the firm's employees in that first year came from the same previous employer. This additional requirement helps us avoid incorrectly classifying a large company that opens a new branch but with a new firm identifier as a startup.[2] We focus our empirical analysis on two outcomes: earnings and performance on the job ladder.

We estimate the earnings differential over the five years after the initial transition to a job, distinguishing between workers who transition to a startup and workers who transition to an established firm. The raw difference in earnings between these two groups indicates that those who transition to a startup earn 16.3% less on average over the next five years than those who transition to an existing firm. However, this result includes a sorting component since workers who transition to a startup are potentially different from workers who transition to an established firm.

To identify the effect of the treatment –having an employment spell in a startup– we implement two alternative strategies that leverage the fact that we have access to a rich set of information about workers' career histories before the transition to the firm. First, we use pre-treatment characteristics as controls in a linear regression. In this specification, we find a -6.5% earnings effect of taking a job at a startup vs. an established firm. Second, we follow a non-parametric matching approach — in the same spirit as Burton et al. (2018) in a similar setting — that consists of a combination of exact matching and nearest neighbor matching. Under this approach, we build cells containing workers with the same age, gender, country of birth, date of transition, and time out of employment before the transition. Then, within those cells, we look for the two individuals with the closest previous earnings for each worker who took a job at a startup and select them as controls. By doing this, we construct a "triplet" that consists of a treated individual (a worker who transitioned to a startup) and two controls (workers who transitioned to an existing firm). Our results show that the five-year earning effect of working at a startup is -6.7%. Note that these two point estimates are very close and indicate a large negative effect of working for a young firm.

We then decompose the effect of going to work for a startup into how much of the earnings penalty comes from lower earnings while employed and a piece due to more frequent or longer periods out of employment. To estimate the first effect, we re-estimate the earnings penalty using the matching triplets approach, excluding periods when individuals do not have a salaried job. Conditional on formal employment, we find that the effect on average earnings is -3.5%, leaving -3.2 percentage points attributable to more time spent outside

[2] For details, see Section 3.

formal employment. Finally, we explore the dynamics of the effect. We find a difference of -6.5% at the time of the initial job transition, which remains between -8% and -6% over the 60 months horizon. This result reveals a highly persistent negative effect on the earnings trajectory for workers who transitioned to a startup instead of an established firm.

As an additional outcome, we analyze the effect of working for a startup on the performance on the job ladder. We look at three variables: the probability of being employed in a particular month over the five years after the transition, the total number of jobs held over the same period, and the probability of experiencing a job-to-job transition. We find that the average effect of taking a job at a startup is a reduction of 2 percentage points (3%) in the probability of being employed in any particular month. In addition, workers who join a startup have, on average, 0.7 fewer jobs than those who join an established firm. This result is consistent with a part of the negative earnings effect of working at a startup coming from fewer job switches (Topel and Ward, 1992). Finally, our results indicate that working at a startup has a negative effect on the probability of experiencing a job-to-job transition of 4%. These results are consistent with workers at young firms having a harder time climbing the job ladder.

As a last step, we look at the heterogeneity of the effects with respect to worker and firm characteristics. Regarding workers' characteristics, we analyze gender, age, earnings before the transition, and type of transition. We find that the penalty is similar across genders. After age 25, there is a decline in the penalty as age increases. When looking at the effects across the spectrum of previous earnings, we find that the group of workers in the lowest quintile of previous earnings experienced an average penalty of -9.3%, 39% more than the average penalty, and 63% more than the penalty of those in the third quintile of the previous earnings distribution.

When looking at ex-ante firm characteristics- size and main economic sector- we find that compared to a firm in the same category, the earnings penalty increases with size. For startups of one employee, there is an earnings premium of 1.04%, while for startups of 200+ workers, there is a -2.72% penalty. These heterogeneous results are consistent with the negative selection of small established firms. Small startups have a higher growth potential on average than small, established firms. There is also a wide heterogeneity of effects across economic sectors, with Transportation and Storage, and Manufacturing exhibiting the largest penalties — 11.87% and 11.44%, respectively. On the other side of the spectrum, joining a startup in the Construction or the Food and Accommodation Services sectors have the lowest penalties— 2.01% and 3.22%.

When looking at ex-post characteristics- survival after five years and whether the firms are at the 95 percentile of size by age 5- we find that most of the penalty is concentrated among the workers who joined non-surviving startups. Conditioning on firm survival, the earnings penalty of joining a startup is -1% over five years after joining the firm. Finally, when we look at the startups that survive and are at the top of the sector size distribution by age 5, we find a significant wage premium compared to the average established firm.

The rest of the paper is structured as follows. We describe the data and provide summary statistics for workers and firms in Section 3. In Section 4 we explain our empirical approach and describe the impact of joining a startup on earnings over five years after joining the firm. In Section 5, we explore the effect of joining a startup on performance in the job ladder by analyzing subsequent labor market outcomes. In Section 6, we study how the earnings effect of section 4 differ with workers' and firms' characteristics. Finally, we conclude in Section 7.

2 Literature Review

Our work contributes to three strands of the literature. First, it relates to papers that study startup employees' earnings compared to workers at established firms. Brown and Medoff (2003) is one of the first papers to examine the relationship between firm age and wages controlling for worker characteristics. Using a small sample of 500 individuals from the household Survey of Consumers conducted by the Survey Research Center at the University of Michigan, they find that older firms pay higher wages than younger firms. However, the relationship becomes insignificant or negative after controlling for worker characteristics. More recently, Burton et al. (2018) and Babina et al. (2019) control not only for worker characteristics but also firm heterogeneity and find a small but positive young-firm wage premium. So far, most of the work has focused on the contemporaneous earnings differentials between workers at startups and workers at older firms rather than on the medium- and long-term effect of startup employment on future earning trajectories. One exception closely related to our work is Sorenson et al. (2021), who use administrative data from Denmark to estimate the earnings differentials of working at a startup over the next ten years after the transition. They find that startup employees earn 17% less than those hired by large established firms over ten years after joining the firm. We contribute to this literature by accounting for both contemporary and medium-term effects. Additionally, we include a richer set of controls, including the type of transition and the time out of formal employment before the transition. The addition of these controls allows us to account for differences in earnings that would otherwise be attributed to the firm's age. Regarding the additional outputs, we disentangle the effect between wage changes and the frequency of spells without formal employment. Additionally, we analyze the job ladder effects, finding that effects on earnings come partially from the different pace at which workers who join startups climb the job ladder compared to workers who join established firms. Finally, we explore the heterogeneity of the impact across workers of different characteristics.

Second, this paper is related to the literature studying the scarring effects of adverse labor market experiences. Most papers in this literature have focused on the lingering effects of unemployment spells on job and earnings prospects. Using Social Security records for the United States, Davis and Von Wachter (2011) find that real earnings fall sharply at the time of displacement and remain 10 to 20 percent below pre-displacement earnings even 20 years later. They also document that the present value earnings losses associated with job displacement are highly sensitive to labor market conditions at the time of displacement, with displacement in recessions being nearly twice as costly as displacement during an expansion. Krolikowski (2017) and Jarosch (2023) argue that existing models used to study unemployment fluctuations have difficulty generating this observed magnitude and persistence of post-displacement earnings losses. Searching for an explanation, they propose search models with job ladders in which workers coming from unemployment are matched to riskier businesses, i.e., those with a higher separation probability. This mechanism forces individuals to spend more time climbing up the ladder after a displacement, matching the magnitude and persistence of earnings losses from the data. Other papers in this literature look at a similar scarring experience for workers who join the labor market during recessions (Schwandt and von Wachter, 2019; Wee, 2013). We contribute to this literature by documenting the persistent effects of working for a young firm. We also find that workers who join young firms fare worse in terms of earnings and when climbing the job ladder than those who transition to established firms.

Third, our work contributes to the literature on joint worker and firm dynamics. In this line, Ouimet and Zarutskie (2014) use administrative data from the U.S. and find that young firms disproportionately employ young workers. Engbom (2019) shows that older individuals are more reluctant to make job-to-job transitions because they have already reached higher rungs in the job ladder, making separations that lead to falling off the ladder more costly for them. Dinlersoz et al. (2019) argue that young firms tend to hire younger workers and provide them with lower earnings compared to established firms. They formalize this idea in a model with an entrepreneurial sector, where individuals with low assets are more likely to accept job offers from startups. Arellano-Bover (2023) studies the effect of the size of the firm at which a worker takes their first job and finds that initially matching with a larger firm substantially improves long-term outcomes such as lifetime income. We contribute to this literature by characterizing the role of workings at a startup in the earnings paths of workers, painting a clearer picture of the joint dynamics of workers and firms.

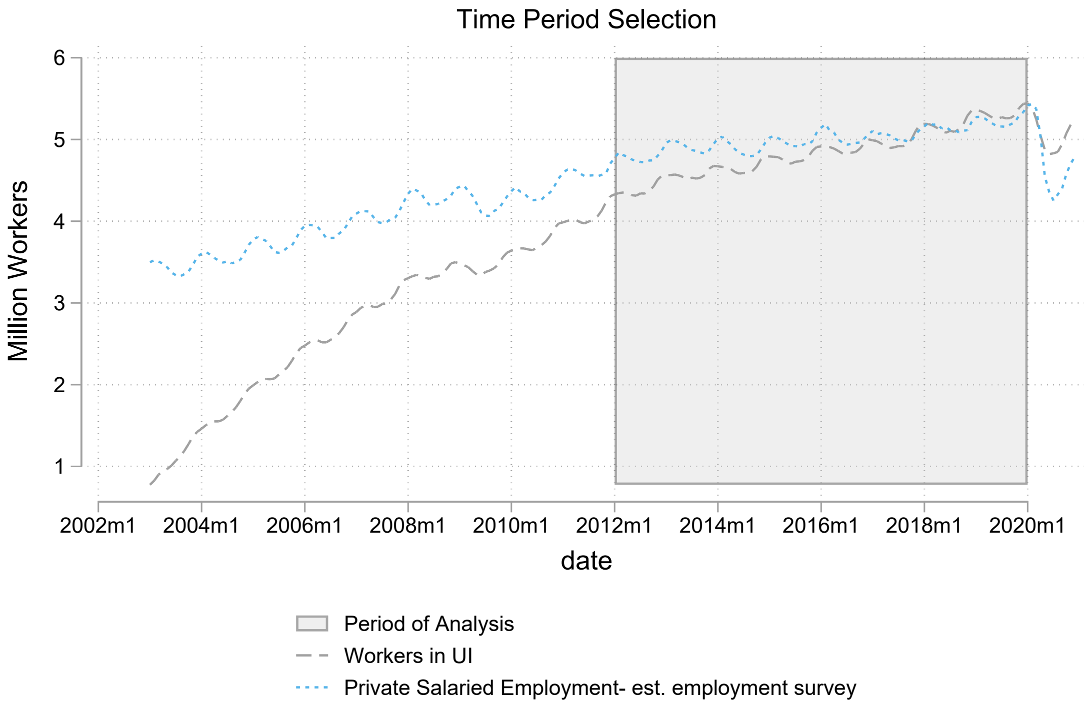
3 Data

We use data from the UI system in Chile through the Central Bank of Chile (hereafter, CBC). Officials of the CBC processed the disaggregated data from the Chilean Pension Supervisor[3]. The UI data correspond to a matched employer-employee dataset, similar to the LEHD in the United States. Unemployment insurance in Chile is mandated for all workers over 18 years old who are employed in private-sector salaried jobs. Workers under 18 years old, the self-employed, and public sector employees are excluded from the mandate but can join voluntarily. Participation in the unemployment insurance system is compulsory for everyone 18 years or older starting a new private sector job after 2002 and voluntary for everyone else. As Figure 1 shows, a large proportion of private salaried employment was outside the UI system in its early years.

According to the Chilean National Institute of Statistics, 27% of employment in Chile was in the informal sector between 2017-2019, defined as not contributing to the UI system. Using data from the Social Protection Survey, the longest longitudinal survey in Chile, Lopez-Garcia (2015) finds that once a worker enters a sector- formal or informal- the probability of switching

[3] To secure the privacy of workers and firms, the CBC mandates that the development, extraction, and publication of the results should not allow the identification, directly or indirectly, of natural or legal persons. We implemented all of the analysis and did not involve nor compromise the CBC.

Figure 1. Private Salaried Employment and UI employment



is very low, around 1% over a two-year period. These facts have two implications. First, our results only apply to formal salaried employment. Second, when we say non-employment is technically non-formal-employment but given the lack of movement across sectors, this difference is not very relevant.

3.1 Sample, Variables and Panel Construction

3.1.1 Sample

Including only workers with zero tenure. By definition, all workers at a startup have zero tenure because the firm recently opened. In contrast, established firms have workers with longer tenure and experience on the job. If we include all workers, regardless of tenure, we would face three additional identification challenges. First, we would only have access to a censored tenure variable for workers with long tenures.[4] Second, tenure and firm age are

[4] In 2012 for workers that have been with the same firm all their career, we only know that the worker has been working at least 10 years.

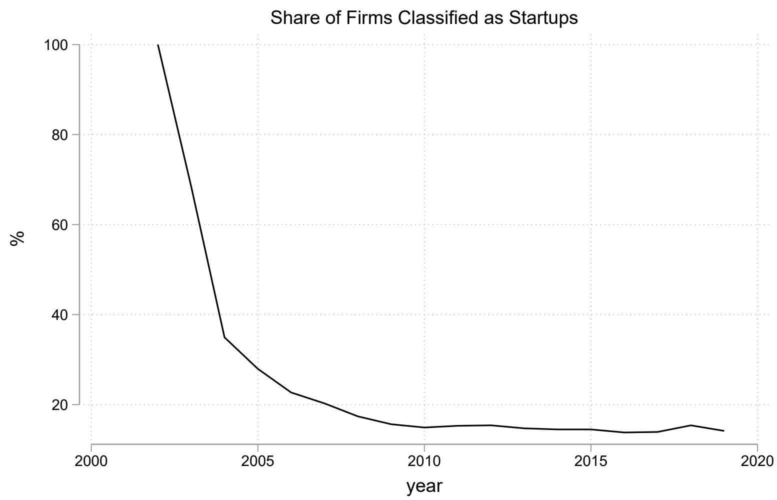


Figure 2. Share of firms classified as startups in UI data

collinear for workers who join startups, so when comparing the earnings of workers at startups with the earnings of workers at established firms, the difference will reflect both firm's age and worker's tenure differences. Third, including information on tenure only partially addresses this issue. Pastorino (2023) shows that jobs vary along the degree to which tenure relates to wage growth. Given our data limitations, the relationship between tenure and firm age, and the complexity of the relationship between tenure and earnings growth, we choose to focus on new hires for our empirical approach.

Excluding workers close to retirement age. To avoid retirement considerations in the 5 years after joining the firm we exclude workers over the age of fifty.

3.1.2 Variable definitions

Startup. Our independent variable of interest is firm age. We define a firm's birth date as the first date in which the firm makes a contribution to the UI system on behalf of an employee. Then, we define startups as follows:

Definition 1. A firm is a *Startup firm* if it is aged one year (12 months) or less.

The implicit assumption behind Definition 1 is the following:

Assumption 1. *All existing firms pre-2002 signed at least one new labor contract between 2002 and 2012.*

There are two issues with our definition of a startup. First, there is a possibility that established firms, those created prior to 2002, delayed their entry into the UI system by not issuing any new contracts. To deal with this possibility we rely on assumption 1 and exclude data before 2012. Figure 2 shows the startups' share stabilized around 10% beginning in 2009, suggesting that this assumption is quite conservative. Second, established firms can be mislabelled as a startup when a large company opens a new branch with a new firm identifier.[5] In this case, our method would classify this "new" firm as a startup. To deal with this problem, we impose an additional restriction to our startup identification. When a *startup* is born, its share of employees that comes from the same previous employer must be less than 30%.[6]

Our startup definition differs from some previous papers in the literature. For example, Sorenson et al. (2021) label firms as startups during their first four years of operation, while Babina et al. (2019) do it during the first three years of operation. Given our data limitations, we restrict our analysis to the one-year or younger definition.

Job transitions. To identify job transitions, we follow the literature in restricting the analysis to a worker's main job. We define the main job as the job with the highest earnings in a given month. When building workers' transition histories, we drop information on secondary jobs but consider them when calculating total earnings.[7]

We define a job transition as a change in a worker's main job. There are two types of transitions. A job-to-job transition occurs if there are no intervening months with missing

[5] There are regulations in Chile that generate an incentive to subdivide firms into separate entities when in fact, they are not. The incentives include: keeping workers and profits under different legal entities to weaken worker's bargaining power; avoiding size-related requirements like having to provide daycare if the firm has more than 20 female employees regardless of age or marital status; and being able to hire workers under temporary contracts for more than a year. The Chilean Congress passed law 20760 in 2014 with the objective of stopping the use of this multi-id practice (or multi-rut in Spanish). The law contemplates fines and official procedures to prevent the subdivision of firms.

[6] This applies to firms with starting size of more than 3 workers. Firms that start with 2 workers or less by definition have at least 50% of their workers coming from the same previous employer. For firms with three or less workers we make no adjustments

[7] By eliminating non-primary jobs, we drop 3% of observations, indicating a low incidence of workers with multiple jobs. After excluding non-primary jobs, our observation unit is the worker-month.

information for the worker. In other words, a transition is a job-to-job transition if there is a change in the firm id without a gap of reporting to the UI system. Otherwise, we classify that transition as a non-job-to-job transition. Job-to-job transitions are more likely associated with voluntary moves, in contrast with transitions that include a non-employment spell. Therefore, we include the type of transition as a relevant control variable in our analysis.

Earnings. The dependent variable of interest in the first part of our analysis is the individual's monthly earnings over the 5 years after the initial transition. We have information on monthly earnings from all formal jobs. We build a worker's total monthly earnings by adding income from all the jobs held in a given month. If an individual has a gap in his UI contributions, we assume that he was out of a job and measure his income as zero.

Labor Market History Before the Transition. We use the information on workers' job history before the transition in our identification strategy to pin down similar workers, as described before. The idea behind using previous earnings to account for selection is that a worker's previous work history has valuable information about her preferences and human capital. We build previous earnings as the average for the 12 months before the first transition, conditioning on having a formal job. Note that this variable does not include information about the existence or duration of a non-employment spell between jobs. For example, if a worker had earnings only five out of the twelve month before the transition we add up all the earnings and divide them by five. We capture the employment dimension of a worker's history with two additional variables: a dichotomous variable indicating whether the transition was a job-to-job or non-job-to-job transition, and a set of dummy variables indicating the number of months of formal employment during the 12 months before the transition: 0 months, 1-5 months, 6-11 months, 12 months.

3.1.3 Balanced Panel Construction

We build a balanced panel of individuals starting in the period of their first job transition after January 2012 and consider their work history 5 years after that initial transition. We follow the workers regardless of the firm's survival status or any subsequent job transition. As mentioned before, we use previous earnings in our identification strategy. Using previous labor market experience implies that our panel is exclusively composed of workers with previous formal sector experience in 2012 or later.[8] Since we work with a balanced panel, we also exclude people who permanently exit the formal labor market within 60 months after their first transition. An individual leaves the formal labor market the last time an employer makes a contribution to UI on his behalf.

3.2 Summary Statistics

3.2.1 Workers

Table 1 characterizes workers in the UI system, and therefore the Chilean formal labor market in 2012. We use 2012 since it is the first year we are considering in our sample. In 2012, the formal Chilean labor market had 5,403,316 wage and salary employees contributing to the UI system, of which 38% were women. This low share of women is consistent with the fact that females traditionally are less likely to engage in paid employment. Additionally, the distribution of the number of jobs workers have over a year shows a large dispersion over the stability of jobs. In terms of labor market outcomes, Chilean workers spent on average 3 months of the year out of the formal labor market, had monthly average earnings of USD 1,310 in 2012, and median monthly earnings of USD 846. This wide dispersion of labor market outcomes highlights the relevance of considering heterogeneity across the earnings distribution in the effect we study.

Next, we look at the sample of workers in the balanced panel used in our regression analysis. A worker joins the panel after their first transition to a new job post-2012 and stays in the panel for five years, conditional on having previous work experience. The balanced panel has 2,813,905 workers.

Table 2 shows that this selected sample of workers is similar to the overall labor market in some respects. There is a larger share of males than females; and there is a large dispersion of previous earnings, consistent with Table 1. However, there is a notable difference; the balanced panel only includes workers between 18 and 54. The difference in the age distribution

[8] A worker entering the formal labor market in 2012 and who transitions to a new firm in 2013 will be part of the panel starting in 2013. We use her earnings in the first job to calculate her previous earnings. Note that if a worker only has one job during the analysis period, he is not part of the panel because there was not transition.

Table 1. Full Sample: 2012

	mean	sd	p5	p25	p50	p75	p95
Age	36.33	11.82	20.00	26.00	35.00	45.00	57.00
Female	0.38	0.49					
Earnings (2012 USD)	1,309.74	1,284.24	256.24	569.78	846.10	1,512.78	4,280.51
Months of Non-Employment	2.93	3.81	0.00	0.00	0.00	6.00	11.00
Number of Jobs	1.58	0.95	1.00	1.00	1.00	2.00	3.00
Number of Workers	5,403,316						

Note: This table includes all workers that had at least one monthly contribution in 2012. Average earnings refer to the simple mean of monthly earnings across all the jobs the worker had during 2012. Months of non-formal employment can include months of unemployment, inactivity, retirement, or months prior to entry if the worker entered the formal labor market in 2012.

comes from a sample restriction we impose on people to join the balanced panel. We restrict the age at the time of entry to the panel to be between 18-50, to avoid any retirement considerations in the follow-up period. In terms of the characteristics at the time the worker enters the panel, we find that 40% of workers join the panel after a job-to-job transition, while the remaining fraction of workers had at least one month between jobs with no formal employment. Workers' previous earnings in the balanced panel are 991 USD on average. Of workers in the balanced panel, 8% joined a startup. Although this number seems small, it is consistent with new firms having a relevant role in net job creation, even though their gross transition rates is not high. This is because transitions to existing firms can come from new jobs or from churning in existing jobs, while jobs in new firms are always new jobs.

Next, to have an initial idea of the role of selection in firm type, we split the sample by whether the worker joined a startup or an established firm. We show descriptive statistics by the firm age in Table 3. On average, workers transitioning to startups are one year older than those transitioning to established firms [9]. In line with previous literature, we find that there is a higher share of males who transition to startups. Previous earnings of those transitioning to a startup are 9% lower than for workers transitioning to an established firm. Additionally, people who transition to startups have a higher probability of having experienced at least one month without formal employment immediately before the month they start a new job.

[9] Note that in our sample, we exclude people just entering the labor market because we require the earnings in their previous job for our identification strategy; by definition, new labor force entrants do not have that information.

Table 2. Panel Sample

	mean	sd	p5	p25	p50	p75	p95
Age	33.99	8.78	22.00	27.00	33.00	41.00	49.00
Female	0.36	0.48					
Job-to-Job Transitions	0.40	0.49					
Previous Earnings	990.67	980.49	199.46	464.34	668.65	1118.36	2948.05
Startup	0.08	0.27					
Earnings	1178.50	1416.25	0.00	0.00	828.23	1552.98	4106.26
Earnings exc. zeros	1198.71	1399.73	71.15	82.47	828.23	1552.98	4106.26
Number of Workers	2,813,905						

Note: This table contains the summary statistics of workers included in the sample used in the regression analysis, distinguishing between those who transitioned to an established firm and those who transitioned to a startup. It has one observation per worker per month starting on the date of the first transition to a new job after 2012 and ending 60 periods after that transition.

Finally, workers who transition to a startup have 16% lower average earnings than those who transition to an established firm over the five years after the transition. The difference in previous earnings is smaller than the difference in earnings over the five years after the transition, suggesting a negative effect of working for a startup compared to working at an established firm. Differences in gender, age, and type of transition also indicate that selection plays a role in the firm's age to which a worker moves. People who transition to startups are systematically different from people who transition to established firms. Therefore, it is important to control for these differences when estimating the causal impact of joining a startup.

Table 4 is a table equivalent to Table 3 but for the sample of triplets. The main takeaway from this table is that workers' characteristics in this sample are similar to that of Table 3 regardless of the type of firm to which workers transition. Even after implementing this matching procedure, we find systemic differences in average earnings over the 5 years after the transition.

3.2.2 Firm Dynamics in Chile and the Role of Startups

In this section, we provide an overview of differences in firm dynamics over their life cycle. To the best of our knowledge, this is the first paper to document firm dynamics for Chile

Table 3. Workers' characteristics by type of firms: Balanced Panel

	Panel (a): Startups						
	mean	sd	p5	p25	p50	p75	p95
Age	35.00	8.76	22.00	28.00	34.00	42.00	50.00
Female	0.31	0.46					
Previous Earnings	936.46	933.43	192.59	448.94	636.00	1041.39	2751.94
Job-to-Job Transitions	0.33	0.47					
Earnings	999.78	1304.18	0.00	0.00	702.71	1323.05	3563.73
Earnings excluding zeros	1023.13	1286.63	70.88	79.91	702.71	1323.05	3563.73
Number of Workers	228,749						

	Panel (b): Established Firms						
	mean	sd	p5	p25	p50	p75	p95
Age	33.90	8.78	22.00	27.00	33.00	41.00	49.00
Female	0.37	0.48					
Previous Earnings	995.46	984.38	200.17	465.84	671.93	1125.19	2964.69
Job-to-Job Transitions	0.40	0.49					
Earnings	1194.32	1424.67	0.00	0.00	843.19	1572.61	4151.48
Earnings excluding zeros	1214.25	1408.25	71.15	82.63	843.19	1572.61	4151.48
Number of Workers	2,585,156						

Note: This table contains the summary statistics of workers included in the sample used in the regression analysis distinguishing between those who transitioned to established firms and those who transitioned to startupS. It has one observation per worker per month starting on the date of the first transition to a new job after 2012 and ending 60 periods after that transition.

using UI data. For this analysis, we focus on the period 2017-2019 to capture a group of firms with a diverse age range. We limit our analysis to firms up to eight years old, given the censored nature of the age variable we have available. Figure 3 shows four measures that highlight the disproportionate role of startups as drivers of the Chilean labor market along key dimensions. Panel (a) shows average net job creation and job destruction rates at the firm level as a share of average employment between t and $t-1$ by firm age. Following our definition of startups as those firms that did not exist during the previous year, we show the job creation and destruction rates of firms aged one and above. We can think of firms of age one here as those firms that were startups in the previous year and survived to the next year. Consistent with previous work on firm dynamics, younger firms exhibit larger job creation

Table 4. Workers' characteristics by type of firms: Triplets Sample

	Panel (a): Startups						
	mean	sd	p5	p25	p50	p75	p95
Age	34.84	8.75	22.00	27.00	34.00	42.00	50.00
Female	0.30	0.46					
Previous Earnings	921.66	884.92	215.91	457.98	638.93	1029.74	2583.83
Job-to-Job Transitions	0.33	0.47					
Earnings	996.27	1277.26	0.00	0.00	713.15	1327.72	3468.27
Earnings excluding zeros	1019.34	1259.62	70.90	80.30	713.15	1327.72	3468.27
Number of Workers	209,122						

	Panel(b): Established Firms						
	mean	sd	p5	p25	p50	p75	p95
Age	34.78	8.75	22.00	27.00	34.00	42.00	50.00
Female	0.30	0.46					
Previous Earnings	912.73	864.04	222.62	460.90	638.86	1020.41	2511.61
Job-to-Job Transitions	0.33	0.47					
Earnings	1100.33	1310.42	0.00	0.00	805.10	1474.55	3632.61
Earnings excluding zeros	1121.11	1293.29	71.15	82.19	805.10	1474.55	3632.61
Number of Workers	363,878						

Note: This table contains the summary statistics of workers included in the sample used in the regression analysis distinguishing between those who transitioned to established firms and those who transitioned to a startup. It has one observation per worker per month starting on the date of the first transition to a new job after 2012 and ending 60 periods after that transition.

rates than older firms, similar job destruction rates, and a higher net aggregate growth. The latter result is what makes startups the engine of net aggregate employment creation.[10]

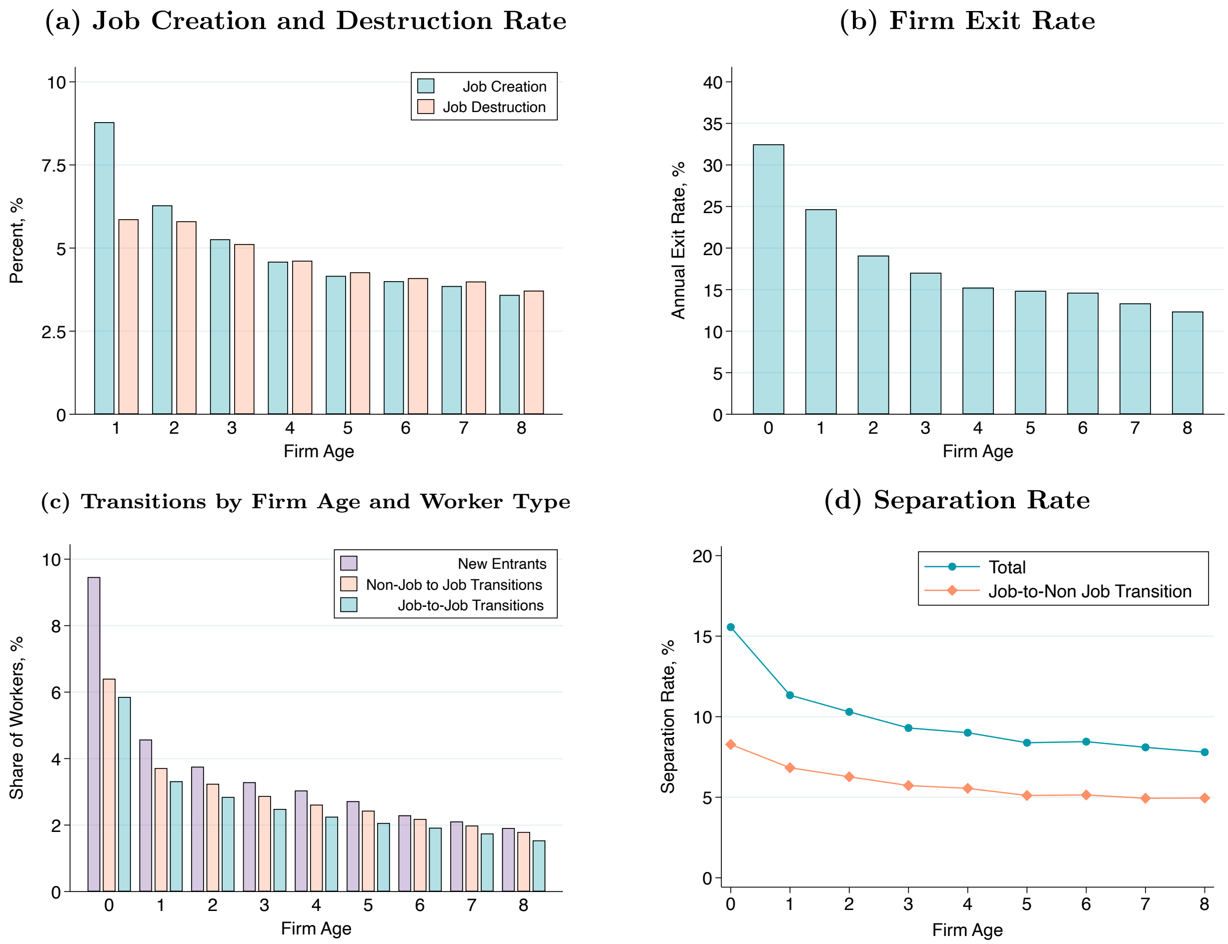
Panel (b) shows that firm exit rate decreases with age, and in particular startups have higher exit rates than older firms. Startups have an annual exit rate of around 33% while firms aged five to eight exhibit exit rates of less than 15%. In panel (c) we decompose transitions into new jobs by firm age and transition type. We classify the transitions in three groups, people joining the labor market for the first time (New Entrants), workers with previous experience who started a new job after at least one month of non-employment (Non-Job to

[10]See Haltiwanger et al. (2013) and Decker et al. (2014) for empirical evidence of the importance of young firms in aggregate job creation and their up or out dynamics in the US.

Job Transitions), and workers with previous work experience who moved to a new job without a break in their contributions (Job-to-Job Transitions). Startups represent a larger share of worker transitions, regardless of type. The purple bars show the fraction of new entrants that went to firms of different ages. 9% of entrants get their first job at a startup. In the orange and green bars, we see that for experienced workers, non-job-to-job, and job-to-job transitions, startups account for around six percent of the transitions, exceeding the share of firms at any other age. Finally, Panel (d) shows separation rates, the share of workers whose work relationship ended, as a percentage of employment in the previous period. The green line shows that around 15% of individuals separate from startups within a year. The orange line shows that half of these individuals went to non-employment i.e. they had a job-to-non-job transition.

Overall, these figures suggest that startups play an important role in labor market dynamics in Chile. However, startups are different than established firms. They are riskier, as they exhibit higher exit and separation rates. In the following section, we look deeper into what this means for workers' earnings and their employment prospects.

Figure 3. A First Look at Firm Dynamics in Chile



Note: Panel (a) shows job creation and destruction rates by firm age. Panel (b) computes the annual exit rate by firm age. We compute these by taking the ratio of the number of firms that do not appear in the current year but were present last year. Panel (c) shows the share of workers in each transition type that are accounted for by firms at different ages. For example, of the total number of new entrants, 9 percent go to startups (firm with age zero). Of the total number of non-job-to job transitions, around 6 percent went to startups (orange bars). Finally, of the total number of job-to-job transitions, around 6 percent went to startups (green bars). Panel (d) plots separation rates along the firm age distribution. The green line shows the total separation rate, i.e. the fraction of all workers in the previous year that was not present at the firm in the current year. The orange line includes only those workers that left the formal labor market (hence, Job-to-Non Job Transition). All panels use data between 2017 – 2019 from the Chilean Unemployment Insurance Data. Panels (a) and (c) do not add up to 100 because we are excluding from the figures, but not from the calculations firms older than 8 years

4 Earnings Effect Over the 5 Years After the Transition

In this section, we study the effects of joining a startup on workers' earnings up to 5 years after the transition.

4.1 Linear Controls

To quantify the importance of selection in the observed differences between workers who transition to startups to those who transition to established firms, we start by calculating the average difference in medium-term earnings. To do so, we estimate the following equation:

$$\texttt{ihs}(\text{Earnings}_{it}) = \beta \text{Startup}_i + \phi_s^{\text{init}} + \lambda_t + \rho_\tau + \boldsymbol{\Lambda}' \boldsymbol{X}_i + \varepsilon_{it}, \tag{1}$$

where Earnings_{it} is earnings of worker i at time t. Startup_i is an indicator variable that takes the value one if the worker enters the panel after transitioning to a startup and zero if the worker enters the panel after transitioning to an established firm. ϕ_s^{init} is a set of initial sector fixed effects i.e. the sector at which the workers transitioned to at the beginning of our sample. λ_t is a time fixed effect, ρ_τ is the date of the first transition fixed effect, and $\boldsymbol{X}_i$ is a vector of controls that include gender (dummy), age in years (continuous), country of birth (dummy), and job-to-job transition (dummy). All these controls take their values at the moment workers enter our balanced sample and thus do not vary over time. Finally, ε_{it} is an error term.

Note that this specification assumes a specific linear relationship between the vector of controls, $\boldsymbol{X}_i$, and the dependent variable, Y_{it}. Moreover, since to estimate the OLS coefficient we minimize the square distance between the projected and the actual output across workers, groups where the Startup variable has a larger variance will exert more influence on the estimates[11]. We include initial sector fixed effects (ϕ_s^{init}) to make sure our results are not coming from intrinsic differences across sectors. If some sectors are, for example, more dynamic than others in terms of business creation, excluding the sector would mean that

[11] By groups of workers we mean groups of workers defined by the covariates. It is easiest to think about groups defined by dummy variables like gender but the same concern applies to all types of variables. For example, if the Startup_i variable has a larger variance across women than across men, and the average treatment effect is different between genders, the weight given to the treatment effect on women on the average effect is going to be larger than the share of women in the sample.

the startup variable would include the earnings effect in those sectors. In other words, if startups are concentrated in some particular sectors exhibiting wage premia/penalties, then our estimate of γ will put more weight on the earning differentials coming from startups in those sectors.

In all specifications related to earnings, we use the inverse hyperbolic sine (`ihs`) transformation. This function is similar to the natural logarithm function but is defined at zero. Given that zero earnings can play an important role in the difference between taking a job at a startup and taking a job at an established firm, the `ihs` transformation is more appropriate for our estimation. However, estimates using the `ihs` are sensitive to how we scale variables. Following Aihounton and Henningsen (2021), we choose 10^{-9} as the scale of the earnings variable. We discuss this scaling problem and compare the results using `ihs` and the natural logarithm transformation for different values of the variable of interest in Appendix A. [12]

Column (1) of Table 5 reports the results of the OLS specification without controls nor fixed effects. Given that our independent variable of interest is dichotomous, following Bellemare and Wichman (2020) we calculate the implied semi-elasticity and show it as the first statistic. We also report the p-value of the semi-elasticity calculated using the delta method.[13]

According to column (1), a worker who transitions to a startup earns, on average, 16.3% less over five years after joining the firm relative to those who transitioned to an established firm. This may simply reflect differences in workers' characteristics across firms, on top of the effect of joining a startup on workers' earnings, our causal effect of interest.

[12]In Appendix A.2, we include two separate exercises with natural logarithms to address the fact that this function is not defined at zero. First, we aggregate the real earnings over five years after joining the firm and estimate the same specification using the average real earnings defined in two ways: by summing up all earnings and dividing them by 60; and by summing up the real earnings and dividing them by the number of periods with employment. Second, we keep 60 periods per worker and replace the zeros with the first percentile of earnings. The advantage of the first exercise is that we do not use an imputation or a scale-sensitive function. While the advantage of the second approach is also that we do not use a scale-sensitive function and this allows us to interpret the coefficient of interest as the semi-elasticity of earnings directly. The qualitative results are similar regardless of the function we choose or the exercise we perform. For details, see Appendix A.2.

[13]We calculate the semi-elasticity as $\epsilon \approx 100 * \dfrac{\texttt{sinh}(\hat{\beta}+\alpha)}{\texttt{sinh}(\alpha)} - 1$ where $\texttt{sinh}()$ is the hyperbolic sine function and $\alpha = \overline{\texttt{ihs}}(\text{Earnings} \mid \text{Startup} = 0)$ is the unconditional average of workers' earnings, over time and across workers, at established firms after applying the `ihs` transformation.

In column (2) of Table 5, we include the confounding variables as linear controls, and also the full set of fixed effects. After accounting for selection using OLS, we find that taking a job at a startup still has a negative effect on worker's earnings of 6.5% on average over the 5 years after the transition.

Table 5. Earnings Effect

	Earnings ≥ 0			Earnings > 0	
	(1)	(2)	(3)	(4)	(5)
Startup	-16.25***	-6.50***	-6.73***	-3.51***	-3.12***
	(0.0338)	(0.0284)	(0.0314)	(0.0241)	(0.0236)
Observations	163,319,316	163,319,316	36,391,967	25,838,819	25,838,819
Adj. Within R^2	0.001	0.227	0.001	0.001	0.001
Time F.E.	✓	✓	✓	✓	✓
Transition Date F.E.	✓	✓			
Controls		✓			
Triplet F.E.			✓	✓	✓
Initial Sector F.E. (3 digits)		✓	✓	✓	✓
Contemporaneous Sector F.E.					✓

Note: This table shows the implied semi-elasticity of the Startup dummy. Startup is a dummy variable that takes the value of 1 if the firm the worker joined at the beginning of the panel was a startup and zero otherwise. We have a balanced panel. We follow each worker for 60 periods after the first transition starting in 2012. Notice that since we use previous earnings as a matching variable, we only include workers with previous experience. We use the ihs transformation on earnings given that we have periods of zero earnings. Following Aihounton and Henningsen (2021), we choose 10^{-9} as the scale using the R^2 criteria. Column (1) estimates equation 1 including both transition date fixed effects and time fixed effects. Column (2) adds as controls gender, country of birth, age, date of transition, and a dummy to indicate if the transition was a job-to-job transition or a non-job-to-job transition. In addition, it also includes an initial sector fixed effect. Column (3) follows the specification in equation 2. Column (4) follows the specification in equation 2 but restricting to periods with positive earnings. Column (5) adds a contemporaneous sector fixed effect to the specification in column (4).

4.2 Matched Specification: Triplets

One of the ways to summarize the concern about selection in this setting is that treated and control workers are different in ways that can explain differences in earnings, aside from the fact that they took a job at a startup instead of an established firm. An intuitive way to address this concern is to find workers similar to the treated workers but in the control group. Following Burton et al. (2018), we combine the exact coarse matching methodology proposed by Iacus et al. (2012) with nearest neighbor matching with replacement. More precisely, we generate cells of individuals with the same values in all confounders except for previous earnings. Then, among workers within the same cell, we choose the two closest ones in the control group for each treated worker in terms of previous earnings. To ensure that treated and control individuals are similar, we restrict the difference between previous earnings to 10%. In the final triplets dataset, the average absolute difference in previous earnings between treated and controls is 3%. To estimate the coefficient of interest, we restrict the sample to workers in a triplet and calculate the mean difference between the two groups. We do the matching with replacement meaning that control group individuals can potentially be part of more than one triplet. There are two differences between the OLS without controls and the triplets' specifications. First, the sample of workers included in the estimation differs. In the case of OLS with controls, we use all individuals in the balanced panel. In contrast, for the triplets, we only include workers who transitioned to a startup and the two closest workers who transitioned to an established firm in the sample. The second difference is the inclusion of the triplet fixed effects.

This methodology does not impose a linear relationship between earnings and the covariates as the OLS does and is, therefore, more flexible. We run the following regression

$$\texttt{ihs}(\text{Earnings}_{it}) = \gamma \ \text{Startup}_i + \phi_s^{\text{init}} + \lambda_t + \nu_g + \epsilon_{it}, \tag{2}$$

where in addition to the fixed effects specified earlier ($\lambda_t, \phi_s^{\text{init}}$), we include triplets fixed effects (ν_g)).

Column (3) of Table 5 shows our result using the triplets specification. After accounting for selection in a flexible way and ensuring that workers in the treatment and control groups are as similar as possible, we find that, on average, transitioning to a startup has a negative earnings effect of 6.7% over the next five years.

Notably, the estimated coefficients in columns (2) and (3) are very similar. We choose column (3) as our baseline for three reasons. First, we know that control and treatment groups are balanced by construction. Second, the functional form of the relationship between the covariates and the outcome is more flexible. Finally, it is clear that we are identifying the effect of treatment on the treated (or average treatment on the treated) when the sample is built around the treated.

4.2.1 Decomposing the effects

We now decompose the earnings effect into earnings while employed and periods out of employment. We exclude periods of zero earnings from formal employment and compute the five-year average monthly earnings differences conditional on formal employment between workers whose first transition was to a startup and workers whose first transition was to an established firm.

Average earnings while formally employed The impact of taking a job at a startup can manifest in two ways. On the one hand, given their high exit rates, working at a startup can cause more periods of future unemployment. On the other hand, startups may offer lower or higher earnings while employed. To characterize the source of the impact, we decompose the total difference in earnings during the first five years after the transition into two components: (i) differences in periods of non-formal employment and (ii) differences in monthly earnings. In practice, this means we run the specification described by equation 2 but *excluding* periods without formal employment.

Column (4) of Table 5 shows our estimate of γ when we restrict the sample to those periods where individuals have positive earnings. The average earnings difference goes down to 3.5%. As expected, the effect on earnings is smaller than the total average effect. This means that workers who transition to a startup tend to have more periods without formal employment. However, the difference in monthly earnings is still statistically and economically significant. All in all, working for a startup reduces, on average, average monthly earnings by 3.5% over the 5 years after the transition.

Sector of Contemporaneous Job A potential source of differences in earnings for workers who transition to startups is differences in the sectors they work at after they leave the startup. In our baseline specification, we control for the initial sector of destination (ϕ_s^{init}), meaning the sector of the startup they transition into at the start of the five-year period. To check the relevance of the contemporaneous sector of employment, as a last exercise, we control for the sector of their main job by including a set of sector of contemporaneous job fixed effects ($\phi_{s(i,t)}$). In doing so, we are now effectively comparing individuals whose initial transition was to a startup relative to those that move to an established firm within the same cell of observables and where both the initial and contemporaneous sector of employment is the same.

Column (5) of Table 5 shows that the contemporaneous sector of employment cannot fully explain the earnings penalty we previously found. In particular, this last column shows that those who transition to a startup have average monthly earnings while employed 3.1% lower than those who transition to an established firm, even when we condition on the subsequent sector.

Therefore, this decomposition exercise shows that working for a startup has a negative earnings effect above and beyond the more frequent unemployment spells and differences across sectors where individuals work after they leave the startup.

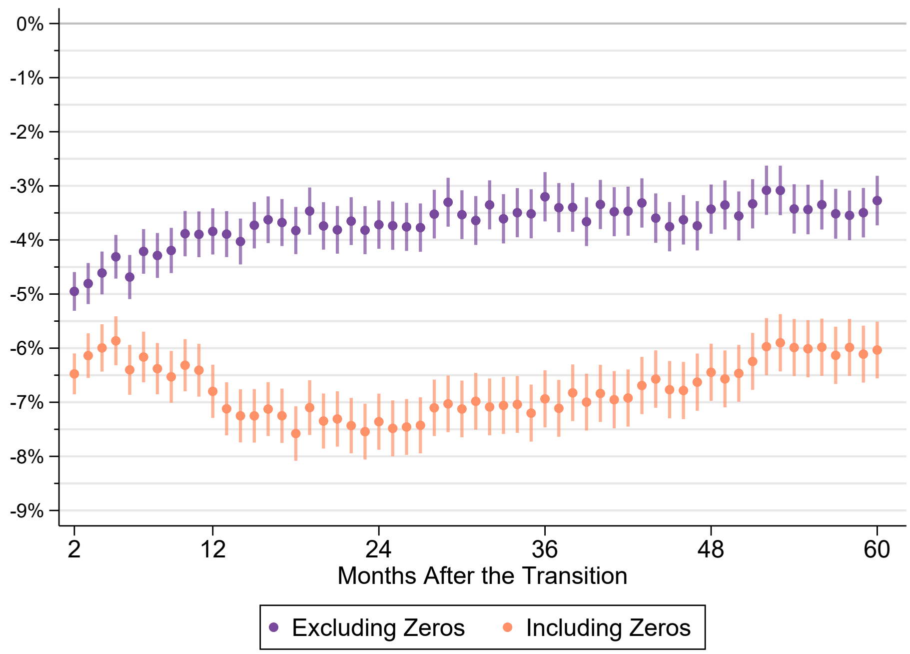
4.3 Dynamics of the Earnings Effect

We are now ready to characterize differences in the path of earnings between individuals who transitioned to a startup vs those that transitioned to an established firm during our 5 years period of analysis. To this end, we estimate one regression per time period after the transition, comparing the earnings of those who moved to a startup to those who moved to an established firm, according to the following equation:

$$\texttt{ihs}(\text{Earnings}_{ik}) = \beta_k \text{Startup}_i + \lambda_t + \nu_g + \varepsilon_{ik}; \quad k = \{2, ..., 60\}. \tag{3}$$

This equation is effectively a cross-sectional regression at different dates where our panel dictates the time dimension. Figure 4 plots β_k over the period of study and shows that the earnings effect is highly persistent over time. The fact that both lines are close together at the beginning of the sample indicates that wages represent most of the penalty during that period. After the first year, the two lines start to diverge. This divergence indicates that people who transition to startups are more likely to be out of formal employment from the first year onward than those who transition to established firms. This story is consistent with the result that periods of non-employment contribute to the overall negative effect of transitioning to a startup that we find in Section 5. Finally, notice that the figure also shows that close to half of the effect comes from lower wages, while half of the effect comes from more frequent spells of unemployment.

Figure 4. Earnings Effect Dynamics



Note: This figure shows estimates of β_k in equation 3. The orange estimates use the sample that includes zero earnings periods. The purple estimates show the results excluding periods without formal employment.

4.4 Why do People Move to Startups?

Given the earnings results, a natural question is why people go to startups if they earn less than a worker of similar characteristics that goes to an established firm. The answer to this question is key to the interpretation of our results. We have identified at least three potential answers to this question: search and matching frictions, compensating differentials, and unobserved heterogeneity.

The macro labor literature relies on models with labor search frictions to replicate the general behavior of aggregated labor markets. The general features of a model with search frictions leading to wage dispersion for equivalent workers can be found in Burdett and

Mortensen (1998). The main idea behind models with search and matching frictions is that finding a match between a vacancy and an unemployed worker is costly. In this setting, when a worker gets an offer, she chooses between taking the offer and spending another period looking for a job. Empirical papers that seek to quantify the relevance of search frictions find that they play a predominant role. According to estimates by Taber and Vejlin (2020), search frictions account for up to 29% of wage dispersion in the US.

An alternative explanation is that workers know that they will experience a 6.7% earnings penalty and decide to take the job because they like other features of startup jobs. As we show in the heterogeneity analysis, the penalty almost disappears once we account for the startup's survival. This result does not make sense with the compensating differential view. Both types of startups share similar features being young firms, and it makes no sense for workers of failing startups to be willing to take a higher penalty than a worker of a successful startup.

Finally, a different explanation could be that there are unobserved factors explaining why workers end up at different types of firms and experience different earnings trajectories. To the extent that those unobserved factors can explain why a worker ends up at a startup and why she experiences lower earnings than similar workers who join established firms, we are capturing those effects in the reported parameters. Unobserved heterogeneity is a concern that, by definition, we cannot overcome. Future work could explore the same questions in settings with more information; however, this will always remain a concern when dealing with observational data.

5 Job Ladder Effects

Another way transitioning to a startup can affect workers' career paths is through future performance on the job ladder. Earnings increases often come from job-to-job transitions and climbing the job ladder. Conversely, if the firm closes, the worker can face unemployment, falling off the job ladder. To explore if transitioning to a startup affects this dimension, we look at the employment probability, the probability of experiencing a job-to-job transition, and the total number of jobs over the five years of analysis.

Employment Probability We start by studying the effect of working at a startup on employment probability. We do so by estimating the following linear probability model:

$$E_{it} = \beta_1 + \beta_2 \text{Startup}_i + \lambda_t + \nu_g + \xi_{it}, \tag{4}$$

where E_{it} is a dummy equal to one if the person has positive earnings reported in the UI system in month t and zero otherwise. As before, λ_t is a time-fixed effect, and ν_g is a triplet-fixed effect. Finally, ξ_{it} is an error term.

Our parameter of interest here is β_2 which shows the effect of joining a startup relative to an established firm on the employment probability. Column (1) of Table 6 shows that taking a job at a startup has an average negative effect on the probability of employment over the next five years: it is associated with a decrease in the probability of being employed of two percentage points.

Job to Job Transitions We now look at the probability that the worker experiences a job-to-job transition in any particular month. To this end, we estimate equation 4 with the dependent variable now being a dummy equal to one if the worker experiences a job-to-job transition in period t and zero otherwise. The results in Column (2) of Table 6 show that workers who transition to a startup have a lower probability of experiencing a job-to-job transition. This result is consistent with workers who transition to startups being less able to climb the job ladder and hence having lower earnings. Even though the point estimate is one order of magnitude smaller than the coefficients in the first column, the average of the dependent variable is also one order of magnitude smaller than the probability of being employed and the estimate is highly statistically significant.

Number of Jobs: Job Hopping Two forces are at play behind the effect of transitioning to a startup on the number of jobs a worker has over time. On the one hand, as we showed in Figure 3, startups tend to have higher closing rates than established firms, so a worker who transitions to a startup would have a higher number of employers all else equal. This is coming from the fact that she is more likely to be forced to find another job with a higher probability than if she had transitioned to an established firm. On the other hand, if a worker spends more time in unemployment, she misses opportunities to move up the job ladder

Table 6. Employment Effects

	Employed (1)	Job-to-Job Transitions (2)	Number of Jobs (3)
Startup	-0.0206***	-0.0017***	-0.0690***
	(0.0002)	(0.0001)	(0.0089)
Observations	37,008,780	37,008,780	616,810
Adj. R^2	0.18	0.05	0.22
Y Mean	0.73	0.04	4.57
Time FE	✓	✓	✓
Triplet FE	✓	✓	✓
Initial Sector FE (3 digits)	✓	✓	✓

Note: The first column is a linear probability model of the probability of being employed, each worker has 60 periods of observations starting on the date of the transition. The second column is a linear probability model of experiencing a job-to-job transition; each worker has 60 periods of observations starting on the date of the transition. In the third column, the dependent variable is the number of firms the worker worked at over five years after the transition; each worker has one observation.

and, therefore, could have fewer of jobs over time. To address this question, we estimate the following equation :

$$\text{NJobs}_i = \delta_1 + \delta_2 \text{Startup}_i + \lambda_t + \nu_g + \varepsilon_i \tag{5}$$

Column (3) of Table 6 indicates that, on average, workers who transition to a startup have 0.7 fewer jobs over the next five years than those who transition to established firms.

6 Heterogeneity

The descriptive statistics in Table 3 shows that workers who take jobs at startups and workers who take jobs at established firms are systematically different. By using the triplets approach, we focus on those who ended up at startups and find their counterfactuals in the set of workers who moved to an established firm. Within each triplet, we assume that who ends up at a startup vs. an established firm is random. The coefficients reported in Table 5 are the average treatment on the treated. In this section, we study how the earning effects that we found in the previous section differ across workers' and firms' characteristics.

6.1 Heterogeneous Effects — Worker Characteristics

We first focus on workers' characteristics. In particular, we look at four characteristics: gender, age, the level of earnings before joining the firm, and the type of transition. We use the triplets sample to run the following empirical specification:

$$\texttt{ihs}(\text{Earnings}_{it}) = \beta Startup_i + \sum_{j=1}^{J} \xi_j D_j \times Startup_i + \sum_{j=1}^{J} \zeta_j D_j + \nu_g + \lambda_t + \epsilon_{it}, \tag{6}$$

where D_j denotes a generic categorical variable and J is the number of categories variable j takes minus one. [14]

In column (1) of Table 7 we report the total effect of working for a startup for workers in the different groups, including periods with employment and non-employment. Column (2) excludes periods of non-employment. This exercise allows us to decompose the total effect into earnings while employed and time out of employment.

The results show that there is substantial heterogeneity in the effects when looking along the age, the quintile of previous earnings, and the type of transition dimension. In contrast, the effects seem to be similar for men and women. When looking at age, we find that after age 25, the effects decrease as the workers age. This result is consistent with a model where startups are at the bottom of the job ladder, the first rungs of the job ladder are slippery and therefore being able to find a job at an established firm at earlier ages represents a higher premium (Jarosch, 2023) .[15]

The effects across quintiles of previous earnings have an inverted U-shape. Workers with the lowest previous earnings experience a penalty of 9.3%, the larger penalty across all quintiles. Recall that the average penalty was 6.7%, meaning that workers in the lowest

[14] As before, we calculate the semi-elasticity for each category using the following formula:

$$\varepsilon_j = 100 \times \left(\frac{\texttt{sinh}(\hat{\beta} + \hat{\xi}_j + \hat{\zeta}_j + \alpha)}{\texttt{sinh}(\alpha)} - 1 \right). \tag{7}$$

where $\alpha = \overline{\texttt{ihs}}(\text{Earnings} \mid \text{Startup} = 0)$ is the unconditional average of workers' earnings, over time and across workers, at established firms after applying the ihs transformation.

[15] The fact that the effect on the youngest group is the lowest among all other workers is hard to analyze. Workers with previous experience by age 18-24 are less likely to have higher education. Also note that for the 18-24 years old, the proportion of the effect that comes from lower employment probability is smaller than the share that comes from earnings while working at 34.5% vs. an average of 51% for the rest of the age groups.

Table 7. Earnings Effects of Joining a Startup: Worker Heterogeneity

	Panel (a): Gender	
	Total	While Employed
Male	-6.5%	-3.2%
Female	-6.8%	-3.6%
	Panel (b): Age	
	Total	While Employed
18-24	-4.8%	-3.1%
25-29	-9.2%	-5.4%
30-34	-7.4%	-3.5%
35-39	-6.8%	-3.4%
40-44	-5.5%	-2.2%
45-50	-5.1%	-2.5%
	Panel (c): Quintile of Previous Earnings	
	Total	While Employed
Q1	-9.3%	-7.4%
Q2	-7.2%	-4.7%
Q3	-5.7%	-2.5%
Q4	-5.2%	-2.1%
Q5	-6.9%	-3.2%
	Panel (d): Type of Transition	
	Total	While Employed
Non-Job to Job Transition	-9%	-5%
Job-to-Job Transition	-4%	-2%

Note: This table presents the earnings effects of joining a startup versus an established firm over five years after joining the firm separated by worker characteristics at the time workers join the firm.

previous earnings quintile experience almost 1.4 times the average effect. Workers in the fourth quintile of previous earnings experience a penalty of 5.2%, the lowest among all the quintiles and around three-quarters of the average effect. However, the effect is non-linear and increases in the last quintile of previous earnings, with a penalty of 6.9% for those with previous earnings in the fifth quintile, an effect very close to the average.

Finally, we find that the largest earnings penalty comes from workers that had at least one month out of employment before joining the firm, with earnings over five years after the transition 9% lower than their counterparts who transitioned to an established firm. This result is expected because job-to-job transitions are more likely to be voluntary.

6.2 Heterogeneous Effects – Firm Characteristics

We now proceed to explore earnings heterogeneity results that arise from firms' characteristics in an ex-ante and ex-post manner.

6.2.1 Ex-ante characteristics

The first dimensions of firm heterogeneity we analyze are ex-ante i.e. defined before the worker joins the firm. In this section, we focus on two dimensions: firms' size, defined using their number of employees by the time the worker joins, and the firm's main sector of economic activity.

We group firms into five different size categories for the size analysis and then estimate equation 6. Our estimates provide evidence of the percentage difference in earnings between joining a startup of a particular size category compared to an established firm in the same category. Panel (a) of Table 8 shows the results. The first column contains the estimates of the effect considering periods of zero earnings, while the second column restricts the sample to those periods with observed positive earnings.

Compared to an established firm in the same size category, we find that workers who join a startup of fewer than ten workers have higher earnings than those who joined established firms of similar size. This result is consistent with small established firms being negatively selected and small startups having, on average, larger growth potential than their established counterparts. For larger firms, there is a penalty for joining a startup; however, notice that the effect is smaller in absolute value than the average result reported in the baseline. This happens because, in the baseline, we do not control by the firm's size. We know that most firms start small, so for the baseline, our results combine the effect of being young with the effects of being small. We believe that being small is a defining characteristic of a startup.[16]

As an additional ex-ante characteristic, we evaluate the heterogeneity in earnings differences by the firm's main sector of economic activity. In Panel (b) of Table 8, we report the results for seven selected sectors with a high impact on the average effect. [17] These

[16]When we include the size categories as controls in a regression without the interactions between the startup variable and the size category, using the triplets sample and specification, the semi-elasticity of Startup goes down to -1.39%. The difference between this semi-elasticity and the -6.7% of the baseline can be interpreted as the startup effect being largely driven by the different sizes of startups and established firms.

[17]We report the effects for sectors that represent more than 5% of the first transitions in the panel. The

results have three main messages. First, there is a significant degree of heterogeneity in the earnings effect of joining a startup by sector of economic activity, with the semi-elasticity ranging from -2.01% in Construction to -11.87% in Transportation and Storage. Second, all sectors show a negative effect of working for a startup on total earnings. Third, the share of the effect that comes from earnings while employed varies widely across sectors. On one end of the spectrum, we find that in the "Agriculture, Forestry, and Fishing" sector, 96% of the difference in earnings comes from lower wages at young firms. At the other end of the spectrum, we find that the "Food and Accommodation Services" sector has a negative effect on total earnings but that earnings while working are slightly higher in young firms relative to established firms. This is consistent with the negative effect on total earnings coming from more periods out of employment for workers who join startups.

6.2.2 Ex-post characteristics

A second possibility is that of ex-post heterogeneity. Ex-post differences refer to firms' characteristics that change over time after the worker joins the firm.

We look at two ex-post firm characteristics: surviving to 5 years and top performer–conditioning on surviving. We define a firm as a survivor to five years if it reports a contribution for at least one employee five years after entering our sample. Panel (c) of Table 8 shows that most of the average penalty comes from firms that do not survive to age five. For workers who joined failed firms, earnings are 20.27% lower than for the average worker who joined an established firm. Once we focus on workers transitioning to firms that survive to age five, the average worker who transitioned to a startup has a minor penalty on earnings of -1.32% relative to the average worker who joins an established firm. These heterogeneous results support the idea that compensating differentials cannot be the principal reason for a worker to move to a startup. In other words, a worker would not choose a failing firm over a surviving one for non-pecuniary reasons.

We now turn to the top performers' results. We define top performers based on firms' size at age five relative to the age-sector size distribution. More precisely, a firm is a top performer if it is in the 90th percentile of the size distribution in its sector at age five. In

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sectors are sorted by the share of workers who transition to those sectors at the beginning of the panel. Construction has the largest share (19.6%), which is due to this sector's high turnover.

Panel (d) of Table 8, we show that workers who joined a top-performer startup, relative to the average worker who joined an established firm, have earnings that are 44% larger. Notice that for workers who joined top-performer startups, the premium is largely due to an increase in the employment probability. The earnings while employed are higher but only by 5%. In contrast, joining non-top performer startups entails earnings penalties of -23.98%. These results highlight that although some workers may experience wage premiums by joining startups (those going to top performers), most workers do not have such luck, i.e., they experience earning penalties.

Our results in this section suggest that workers experience wage penalties when joining a startup relative to an established firm. This effect is heterogeneous across workers' and firms' characteristics. While some workers may actually have higher earnings from joining a startup, our results suggest that this is not true for the average worker nor for a significant fraction of the workers' distribution.

7 Conclusion

In this paper, we study the effect of working at a startup on workers' earnings and on their performance on the job ladder over the five years following a worker's transition into a new job. Using Chilean matched employer-employee administrative data, we find that those who take a job at a startup earn 16.3% less on average over the next five years than those who take a job at an established firm. However, after considering selection, this difference reduces to -6.7%, implying that a significant part of the observed difference in earnings comes from sorting. When decomposing the earnings effect, we find that 3.5 percentage points of the overall five-year effect come from lower average earnings while employed. The remaining 3.2 percentage points arise from more frequent or more prolonged periods of non-employment associated with taking a job at a startup. When looking at the dynamics of the average effect on earnings, we find that initially, the effect comes mainly from lower wages, but after the first year, the employment margin kicks in, with workers who join startups having a higher likelihood of being out of employment. This result likely reflects the high exit rate of startups. Moreover, we find that the average negative effect of taking a job at a startup on earnings is highly persistent and remains even five years after the transition.

Table 8. Earnings Effects of Joining a Startup: Firm Heterogeneity

Ex-ante Characteristics		
	Panel (a): Firm Size	
	Total	While Employed
One employee	1.04%	3.42%
Micro 2-9	0.17%	3.67%
Small 10-24	-1.23%	0.85%
Medium 25-199	-2.47%	-1.40%
Large 200+	-2.72%	-1.40%
	Panel (b): Sector	
	Total	While Employed
Construction	-2.01%	0.06%
Wholesale and Retail Trade	-9.19%	-5.49%
Admin. and Support	-6.85%	-3.66%
Manufacturing	-11.44%	-6.89%
Agriculture, Forestry and Fishing	-6.36%	-6.09%
Transportation and Storage	-11.87%	-7.92%
Accommodation and Food Services	-3.22%	0.58%
Ex-post Characteristics		
	Panel (c): Survival	
	Total	While Employed
Non-survivor by 5yo	-20.27%	-13.19%
Survivor by 5yo	-1.32%	-2.02%
	Panel (d): Top-performers	
	Total	While Employed
Non-Top performer by 5yo	-23.98%	-12.12%
Top performer by 5yo	44.20%	5.49%

Note: Panel (a) and (b) present the earnings effects of joining a startup versus an established firm in the same size/sector over five years after joining the firm. Panel (c) and (d) present the earnings effects of joining a startup versus an average established firm over five years after joining the firm. All semi-elasticities are significant at the 1%.

Our results on the subsequent performance on the job ladder show that those who transition to a startup have, on average, (i) a two percentage point lower probability of being employed in a subsequent month, (ii) hold fewer jobs, and (iii) have a lower probability of experiencing a job-to-job transition, relative to those who joined an established firm over the next five years after the transition. These job ladder results are consistent with our earnings results. They indicate that part of the penalty of joining a startup comes from spending more time out of formal employment and experiencing fewer moves up the job ladder.

We provide further evidence of the heterogeneity of earnings effects. Across worker characteristics, workers with previous earnings in the first quintile experience a larger penalty than any other quintile, 9.6%. After age 25, the earning penalty of joining a startup decreases with age. This result is consistent with the literature that finds that early experiences in the labor market have more pervasive effects on workers' labor market performance.

Looking at firms' characteristics, we find a large degree of heterogeneity across size categories and sectors of economic activity. When evaluating ex-post characteristics, we find that the negative earnings difference comes mainly from non-surviving startups. Joining a surviving startup only represents a penalty on total earnings of 1.32%. Finally, we find that top-performer startups have a premium over the average established firm of 5.49% in earnings while employed and an even larger effect on the probability of being employed, with a total earnings effect of 44.2%, when compared to the average worker who joins an established firm.

The interpretation of our results as causal relies on the conditional independence assumption. In other words, they can be interpreted as causal as long as we have included all relevant confounders when estimating the effect of working at a startup. This means that if there is an unobservable variable related to wages that explains why a worker makes his first transition to a startup instead of to an established firm, the estimated coefficients would also capture the effect of that unobserved variable on earnings. The treatment and control workers we are comparing have the same demographic characteristics, labor history, and previous earnings. Still, some moved to a startup and others to an established firm. We use alternative estimators to check the robustness of our results. Our robust findings indicate that there is a negative earnings effect of taking a job at a startup.

Previous literature on firm dynamics shows that startups are the engine of business dynamism and economic growth in multiple contexts. Our results highlight that when considering the consequences of failed entrepreneurial endeavors, policymakers should also consider that the effects of such adverse outcomes go beyond the owners of the companies and have at least medium-term consequences on workers' career trajectories. When facing budget constraints and choosing between supporting existing businesses with liquidity constraints vs. promoting entrepreneurship, policymakers should keep in mind that existing firms, on average, offer better career outcomes for their workers.

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Appendix

A Alternative Transformations: Inverse Hyperbolic Sine and Natural Logarithm

In this appendix, we show the scale we chose for the inverse hyperbolic sine and study the robustness of our main results to using a logarithm transformation.

A.1 Inverse Hyperbolic Sine

For the earnings regressions, we wish to obtain the semi-elasticity of earnings from working at a startup vs. an established firm. Generally, the procedure to get this semi-elasticity is to apply the logarithmic transformation to the earnings variable before estimating the regression. However, the logarithmic transformation is not defined at zero. We want to include non-formal employment periods in our analysis. Therefore, zeros play a relevant role in our setting.

To include zeros when estimating a semi-elasticity, the standard approach in the literature is to use the inverse hyperbolic sine transformation:

$$\texttt{ihs}(x) = \ln(x + \sqrt{x^2 + 1}).$$

It is evident from this formula that $\texttt{ihs}(.)$ is defined at zero. However, as pointed out by Aihounton and Henningsen (2021), the units of the earnings variable matter when using $\texttt{ihs}(.)$. Figure A.1 shows that the $\texttt{ihs}(x)$ approximates X, the 45-degree line, for values smaller than one, and approximates $\ln(x) + \ln(2)$ for values larger than two. Note that $\lim_{x\to 0} \texttt{ihs}(x) = 0$ and $\lim_{x\to\infty} \texttt{ihs}(x) = \ln(x) + \ln(2)$.

Given this scale sensitivity of the $\texttt{ihs}$ transformation, Aihounton and Henningsen (2021) propose a procedure to adequately choose the scale, somethign we label R^2-criteria in what follows. The R^2-criteria suggests that researchers should pick the units of measurement from the higher R^2 regression where each regression uses a different scale. We implement such a procedure in our setting as shown in Table A.1. We use the estimated coefficients to compute

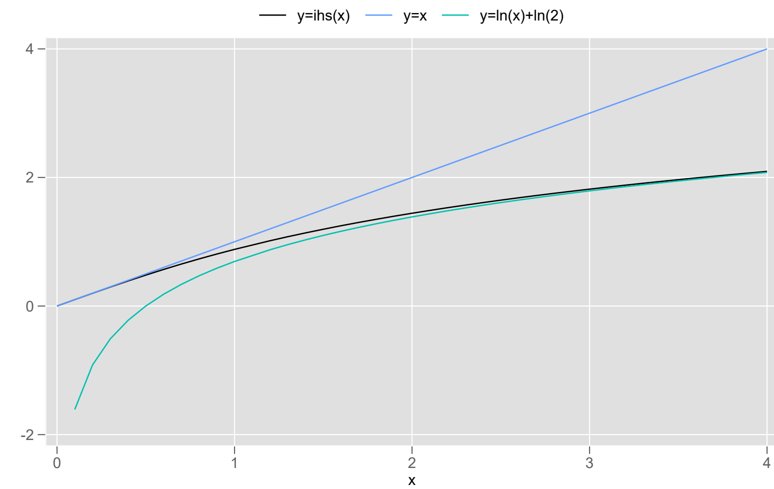

Figure A.1. Inverse Hyperbolic Sine Transformation and Units

the following semi-elasticity:

$$\hat{\varepsilon} = 100 \times \frac{\texttt{sinh}(\hat{\alpha} + \hat{\beta})}{\texttt{sinh}(\hat{\alpha})} - 1, \tag{8}$$

where α corresponds to the constant parameter, the average wage across workers in established firms, and β is the coefficient of interest on the startup dummy. The semi-elasticity ranges from 27.8% to 6.7%. Using the R^2-criteria, our baseline is column (5). Note that the semi-elasticity (R^2) decreases (increasing) across columns until it stabilizes after column (5). Based on these results we choose 10^-9 as the appropriate scale for our estmates.

Table A.1. Baseline Results with Alternative Scales using the Inverse Hyperbolic Sine Transformation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Startup	-0.32601***	-0.27784***	-0.18151***	-0.08351***	-0.00286***	-0.00003***	-0.00000***	-0.00000***	-0.00000***
	(0.0021)	(0.0018)	(0.0010)	(0.0004)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Implied Elasticity	-0.278	-0.243	-0.166	-0.087	-0.067	-0.067	-0.067	-0.067	-0.067
P-value elst.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	36391967	36391967	36391967	36391967	36391967	36391967	36391967	36391967	36391967
Adj. R^2	0.21	0.21	0.23	0.33	0.52	0.52	0.52	0.52	0.52
Scale	10^{0}	10^{-1}	10^{-3}	10^{-5}	10^{-7}	10^{-9}	10^{-11}	10^{-13}	10^{-15}

Note: This table shows different specifications of our main regression (column (3) in Table 5) where we vary scale of the dependent variable.

A.2 Natural Logarithm

In this subsection, we study the robustness of our results by using an alternative transformation to earnings that allow us to include zeros. First, we replicate Table 5 using the natural logarithm instead of the `ihs` and adding the first percentile of earnings to the zeros so that the natural logarithm is defined for the periods of non-employment. The results are in Table A.2. We note our main result is qualitative robust: similar workers who join startups have lower earnings than those who join established firms. Additionally, the 50% split of the penalty between lower earnings while working and more periods out of formal employment is also robust to this transformation. The main difference between these results and our baseline in Table 5 is that the magnitudes in Table A.2 are larger.

As a second robustness exercise, we collapse the time dimension of our main sample. We compute the total real earnings over the 60 months after starting a new job and count the number of months when the worker had positive earnings. More precisely, if we label real earnings at time t for worker i as W_{it}, we have that total real earnings over the 60 months for worker i, W_i, is

$$W_i = \sum_{\tau=1}^{60} W_{i\tau}.$$

Moreover, we define the number of months that worker i is employed in our period over the 60 months window, N_i, as

$$N_i = \sum_{\tau=1}^{60} \mathbb{1}_{i\tau},$$

where $\mathbb{1}_{i\tau}$ is an indicator function that takes the value of one if worker i was employed at time τ and zero otherwise. Based on this two variables, we calculate the average earnings ($\overline{W}_i^T$) and the average earnings while employed ($\overline{W}_i^E$) as follows

$$\overline{W}_i^T = \frac{W_i}{60}, \tag{9}$$

$$\overline{W}_i^E = \frac{W_i}{N_i}, \tag{10}$$

where the main difference in the equations above is that equation (9) considers periods of non-employment to estimate the average, while equation (10) does not.

Using this cross-sectional sample, we re-estimate our previous results. Table A.3 shows that our main qualitative result is robust in such a setup. As in Table A.2, magnitudes are slightly larger than those reported in Table 5. The split between periods of employment and non-employment also differs in this case, where now periods of non-employment only accounts for 30% of the total effect.

These results suggest that our main result in the paper is not particular to the `ihs` transformation we employ in the main text and provide sounding evidence that workers that move to a startup do, on average, earn less than those who move to an established firm.

Table A.2. Earnings Effect of Working at a Startup- Ln and imputation

	Earnings ≥ 0			Earnings > 0	
	(1)	(2)	(3)	(4)	(5)
Startup	-21.90***	-10.27***	-9.66***	-5.15***	-4.81***
	(0.0404)	(0.0370)	(0.0444)	(0.0286)	(0.0278)
Observations	163,319,316	163,319,316	36,391,967	25,838,819	25,838,819
Adj. Within R^2	0.134	0.134	0.001	0.001	0.001
Time F.E.	✓	✓	✓	✓	✓
Transition Date F.E.	✓	✓			
Controls		✓			
Triplet F.E.			✓	✓	✓
Initial Sector F.E. (3 digits)		✓	✓	✓	✓
Contemporaneous Sector F.E.					✓

Note: This table reports results using the logarithm of earnings. Startup is a dummy variable that takes the value of 1 if the firm the worker joined at the beginning of the panel was a startup and zero otherwise. We have a balanced panel. We follow each worker for 60 periods after the first transition starting in 2012. Notice that since we use previous earnings as a matching variable, we only include workers with previous experience. To include missing employment periods, we impute each worker with earnigns equivalent to that of the 1st percentile. Column (1) estimates equation 1 including both transition date fixed effects and time fixed effects. Column (2) adds as controls gender, country of birth, age, date of transition, and a dummy to indicate if the transition was a job-to-job transition or a non-job-to-job transition. In addition, it also includes an initial sector fixed effect. Column (3) follows the specification in equation 2. Column (4) follows the specification in equation 2 but restricting to periods with positive earnings. Column (5) adds a contemporaneous sector fixed effect to the specification in column (4).

Table A.3. Earnings Effect: Collapsed Regression

	Earnings ≥ 0			Earnings > 0
	(1)	(2)	(3)	(4)
Startup	-17.81***	-7.99***	-10.14***	-7.11***
	(0.2001)	(0.1577)	(0.2220)	(0.2184)
Observations	2,768,106	2,768,106	536,182	536,182
Adj. R^2	0.02	0.42	0.41	0.41
Transition Date F.E.	✓	✓		
Controls		✓		
Triplet F.E.			✓	✓
Initial Sector F.E. (3 digits)		✓	✓	✓

Note: This table presents result where the dependent variable is the logarithm of average earnings. Column (1) to (3) consider average earnings including periods of non-employment ($\overline{W}_i^T$). Column (4) only considers period of positive earnings ($\overline{W}_i^E$).