ABSTRACT

Title of Dissertation: Essays in Labor Economics

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This dissertation has as a unifying theme the analysis of labor markets. It includes three papers that analyze labor markets from three different perspectives: the role of firms in workers' careers, the effect of public policies on firms' decisions, and the aggregate labor market effects of individual migration choices. These diverse perspectives show the complexity of labor markets and highlight some ways in which labor markets affect workers, firms and communities.

In the first essay, the objects of study are the career outcomes of workers. More specifically, using Chilean employer-employee data, we provide evidence of the divergent trajectories of workers' careers based on the type of firms they work at. We focus on earnings, periods of employment, and the number of jobs held over the five years after a job transition. Our findings indicate that there is an earnings penalty of 6.7% for joining a startup vs. an established firm. Workers who join a startup have a lower probability of being employed and hold fewer jobs over the five years we follow them.

The second chapter focuses on evaluating the effectiveness of a public policy implemented

at the beginning of the COVID-19 pandemic, a time when mandated quarantines prevented firms from operating, to maintain existing job relationships by providing liquidity in the form of payments to furloughed workers. We leverage a discontinuity in the eligibility for the policy and are able to identify a positive effect on the likelihood of job survival for workers with short tenure. Heterogeneity analysis indicates that the effect was larger in sectors more affected by the pandemic and for more vulnerable workers.

Finally, the third chapter looks at the aggregate labor market effects of experiencing a labor demand shock in a Commuting Zone (CZ) in the US. The main goal of that chapter is to better understand why local labor markets in the US experience a persistent decline in labor force participation following a recession. Using uniquely granular data from the Consumer Credit Panel (CCP), we show that there is a differential migration response to local demand shocks based on the age of individuals. In particular, we find that younger adults increase their in-migration to CZs experiencing a positive labor demand shock. Additionally, while the in-migration response of the retirement-age population also is positive, it is muted and their out-migration response to a positive shock is positive, effectively delivering a negative net migration response to a positive labor demand shock for this age group. The combination of these results indicates that following a positive labor demand shock, local labor markets in the US experience a persistent re-composition of their age structure that leaves them with a higher proportion of young people. On the flip side these results point to the fact that labor markets experiencing a negative demand shock being left with a higher proportion of retirement-age people as the product of the age differentiated migration.

Essays in Labor Economics

by

Nathalie Gonzalez Prieto

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Dedication

To Emilio, Eliana and Andres

Acknowledgments

This dissertation is the culmination of a long journey that was possible thanks to the support of a large group of people. During the six years that it took me to complete my degree, I experienced the standard challenges of finishing a Ph.D. in Economics, the added challenge of having to do so during a pandemic, and the additional consideration of having my first son during my third year and being pregnant while on the job market.

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working with them will guide my research approach in the years to come.

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List of Abbreviations

ACS	American Community Survey
AFC	Administradora de Fondos de Cesantias (Unemployment Insurance Administration Fund)
BEA	Bureau of Economic Analysis
CBC	Central Bank of Chile
CCP	Consumer Credit Panel
CZ	Commuting Zones
ENE	National Employment Survey
EPL	Employment Protection Law
ERS	Economic Research Service U.S. DEPARTMENT OF AGRICULTURE
GDP	Gross Domestic Product
GR	Great Recession
IFE	Ingreso Familiar de Emergencia (Cash transfers)
IHS	Inverse hyperbolic cine
INE	National Statistics Office of Chile
LEHD	Longitudinal Employer-Household Dynamics
LPM	Linear Probability Model
MIGPUMA	Migration Public Use Microdata Areas
MSA	Metropolitan Statistical Area
OLS	Ordinary Least Squares
PUMA	Public Use Microdata Areas
STW	short-term work
UI	Unemployment Insurance
USD	U.S. Dollars
VAR	Vector Autoregression

Chapter 1: Introduction

The three papers contained in this dissertation have as a unifying theme the analysis of labor markets. However, each has a particular angle. In the first chapter, we focus on the individual consequences of career events, having worked at a startup or young firm. In the second, we evaluate a government policy aiming to preserve jobs during the Covid pandemic in Chile. Finally, in the third chapter, we look at the aggregate local consequences of migration decisions in response to local economic shocks on the age composition of the labor force, in the US. The essays contained in this dissertation take alternative perspectives on the labor market. In particular, they highlight the worker perspective, the public policy dimension of labor markets, and finally, the aggregate consequences of individual decisions based on labor market changes.

In Chapter 2, in collaboration with Gonzalo Garcia-Trujillo and Alvaro Silva, we study how working for a startup affects contemporaneous and future labor market outcomes using Chilean employer-employee data. The detail contained in this administrative dataset allows us to compare very similar workers that transition to startups vs workers who transition to established firms. Our findings reveal that joining startup results in a 6.7% reduction in earnings over the next five years, with half attributed to lower average earnings and a half to spending more time out of formal employment. Workers at startups also exhibit a lower probability of being employed and experiencing job-to-job transitions, over the five years after joining the firm. They also

hold fewer jobs. These effects are persistent but vary across worker and firm characteristics. In particular, startups that survive have a smaller earnings penalty, while top-performing startups offer an earnings premium.

In Chapter 3, in collaboration with Macarena Kutscher and Mariano Bosch, we provide evidence of the effects of the employment protection law (EPL) adopted in the wake of the Covid pandemic in Chile using rich administrative data. The EPL allowed Chilean firms to suspend or reduce the working hours of employees in order to preserve the job. We compare the probability of job survival of just eligible workers with that of just ineligible workers before and after the pandemic. We find that for the average low-tenured worker, being eligible implies an increase in the job survival probability of 2.2 p.p. The effect is heterogeneous across sectors and workers' characteristics. The policy was most effective at reducing separations in the Food and Accommodation Services and Arts and Entertainment sectors. We also find that the effect is larger for women than for men. Finally, across income deciles, the effect decreases as the income decile increases. These results indicate that the policy was most effective in sectors more affected by the pandemic and for the most vulnerable workers.

Finally, in chapter 4, in collaboration with John Coglianese, we study the subject of local hysteresis in the US. Hysteresis arises when a short-term shock affects an economy's medium to long-run performance. The consensus is that local labor markets in the US do not exhibit hysteresis (Blanchard and Katz, 1992; Dao et al., 2017). More specifically, recent evidence suggests that age-adjusted labor force participation and employment rates at the state level go back to pre-recession levels following a shock (Cajner et al., 2020; Gonzalez, 2020). However, recessions do appear to produce permanent demographic adjustments at the local level. Using data from the New York Fed Equifax Consumer Credit Panel, where we can follow people over time and have

information on location and age, joined with sectorial GDP at the county level from the Bureau of Economic Analysis (BEA), we look at the migration decisions in response to local economic shocks by age group to understand the process behind these persistent changes in demographics. Our results indicate that prime-age adults decrease their in-migration rate to areas experiencing a recession. In contrast, the out-migration rate of retirement-age individuals increases after a local labor market experiences a positive shock. This result can explain local hysteresis in raw employment and participation rates, not adjusted for demographics, due to the persistent effects of shocks on local demographic composition. These results are consistent with a model in which prime-age individuals are more sensitive to labor market conditions, and retirement-age people are more susceptible to the price of non-tradable goods, such as housing. Age-differentiated migration is a particularly relevant phenomenon because the composition of the labor market affects the business dynamism and the fiscal balance of the local economies.

Chapter 2: Startup Employment and Career Trajectories

This chapter is coauthored with Gonzalo Garcia-Trujillo (Central Bank of Chile) and Alvaro Silva (Economics Department University of Maryland)

2.1 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

[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.

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 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 2.7.

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; made a job-to-job or non-employment-to-job transition after 2012; and who were observed for at least 60 months (five 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

[2]For details, see Section 2.3.

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 a piece due to 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 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, as opposed to a transition that includes a period of non-employment or no 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 leads to 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 2.3. In Section 2.4 we explain our empirical approach and describe the impact of joining a startup on earnings over five years after joining the firm. In Section 2.5, we explore the effect of joining a startup on performance in the job ladder by analyzing subsequent labor market outcomes. In Section 2.6, we study how the earnings effect of section 2.4 differs with workers' and firms' characteristics. Finally, we conclude in Section 2.8.

2.2 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 working at a startup in the earnings paths of workers, painting a clearer picture of the joint dynamics of workers and firms.

2.3 Data

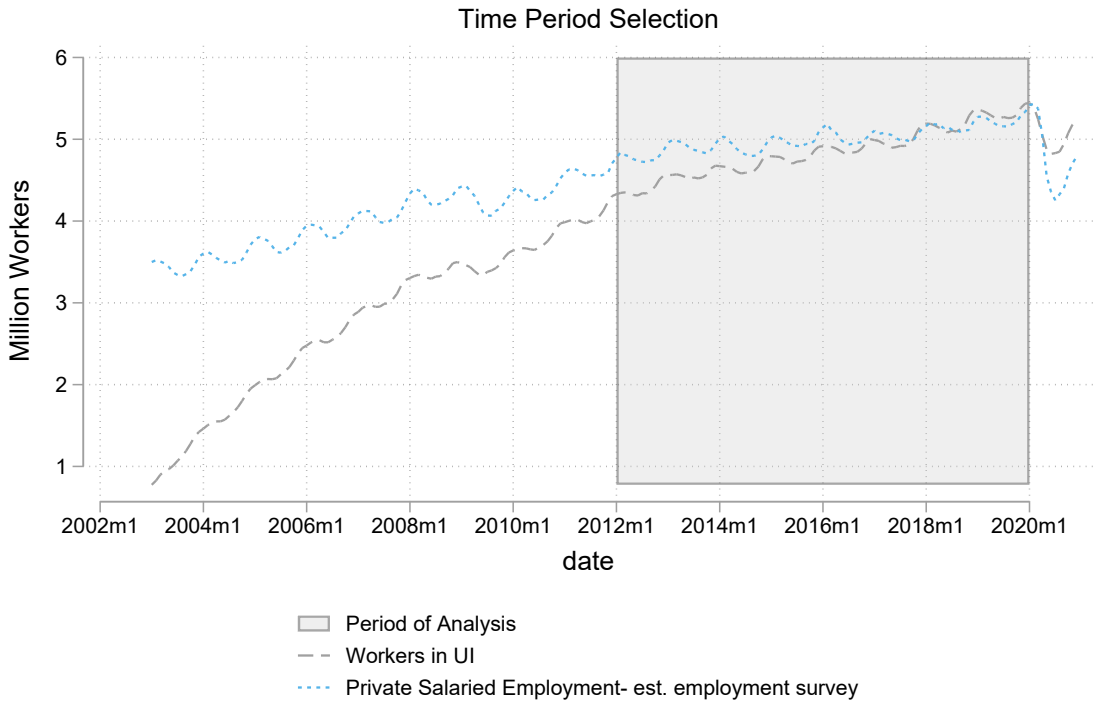
We use data from the UI system in Chile through the Central Bank of Chile (hereafter, CBC). Officials of the CBC processed the disaggregated data from the Chilean Pension Supervisor.[3] The UI data correspond to a matched employer-employee dataset, similar to the LEHD in the United States. Unemployment 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 2.1 shows, a large proportion of private salaried employment was outside the UI system in its early years.

Note on Informality

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 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, what we refer to as non-employment is technically non-formal employment, but given the lack of movement across sectors in Chile, this difference is not very relevant.

[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. Officials of the Central Bank of Chile processed the disaggregated data. All the analysis was implemented by the authors and did not involve nor compromise the Chilean Pension Supervisor.

Figure 2.1: Private Salaried Employment and UI employment



2.3.1 Sample, Variables and Panel Construction

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 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

[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.

tenure differences. Third, including information on tenure only partially addresses this issue. Pastorino (2023) shows that jobs vary in 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.

2.3.1.1 Variable definitions

Startup. Our independent variable of interest is firm age. We define a firm's birth date as the first date at 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.

There are two possible 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 exclude data before 2012, assuming that almost all firms would have hired at least one new worker over the ten-year period from 2002 through 2012. Figure 2.2 shows the share of firms that are startups 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

[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

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

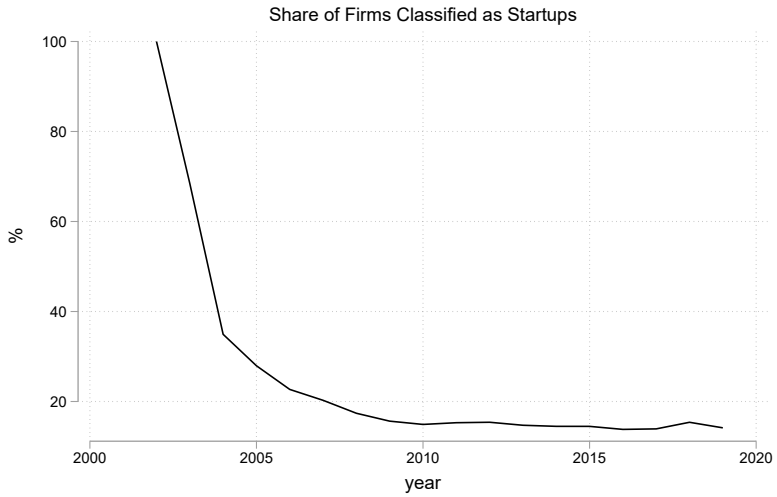


Figure 2.2: Share of firms classified as startups in UI data

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]

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 fewer, by definition, have at least 50% of their workers coming from the same previous employer. For firms with three or fewer workers, we make no adjustments.

[7]By eliminating non-primary jobs, we drop 3% of observations, indicating a low incidence of workers with

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 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 her UI contributions, we assume that she was out of a job and measure her 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 depend on 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 months 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 tran-

multiple jobs. After excluding non-primary jobs, our observation unit is the worker-month.

sition, 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.

2.3.1.2 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 over the five years following 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 the UI system on her behalf.

2.3.2 Summary Statistics

2.3.2.1 Workers

Table 2.1 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

[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, she is not part of the panel because there was not a transition.

Table 2.1: 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 could include months of unemployment, inactivity, retirement, or months prior to entry if the worker entered the formal labor market in 2012.

Table 2.2: 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.

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 anal-

ysis. 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 2.2 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 2.1. However, there is a notable difference; the workers in the balanced panel have lower average age. The difference in the age distribution 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. Note that, transitions to existing firms can come from new jobs or from churning in existing jobs, while jobs in new firms are always new jobs, this share is smaller than the share of net new job creation attributable to startups.

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 for each of these two groups in Table 2.3. 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

[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 2.3: 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.

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. 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 age of the firm to which a worker moves. People who transition to startups are systematically

Table 2.4: 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.

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 2.4 is a table equivalent to Table 2.3 but for the sample of triplets. The main takeaway from this table is that workers' characteristics in both types of firms are similar. Moreover, the characteristics of workers in the balanced panel are similar to the characteristics of workers who transitioned to a startup in the full sample. Even after implementing this sample restriction based on a matching procedure, we find systemic differences in average earnings over the 5 years after the transition.

2.3.2.2 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 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 firm age variable we have available. Figure 2.3 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 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]

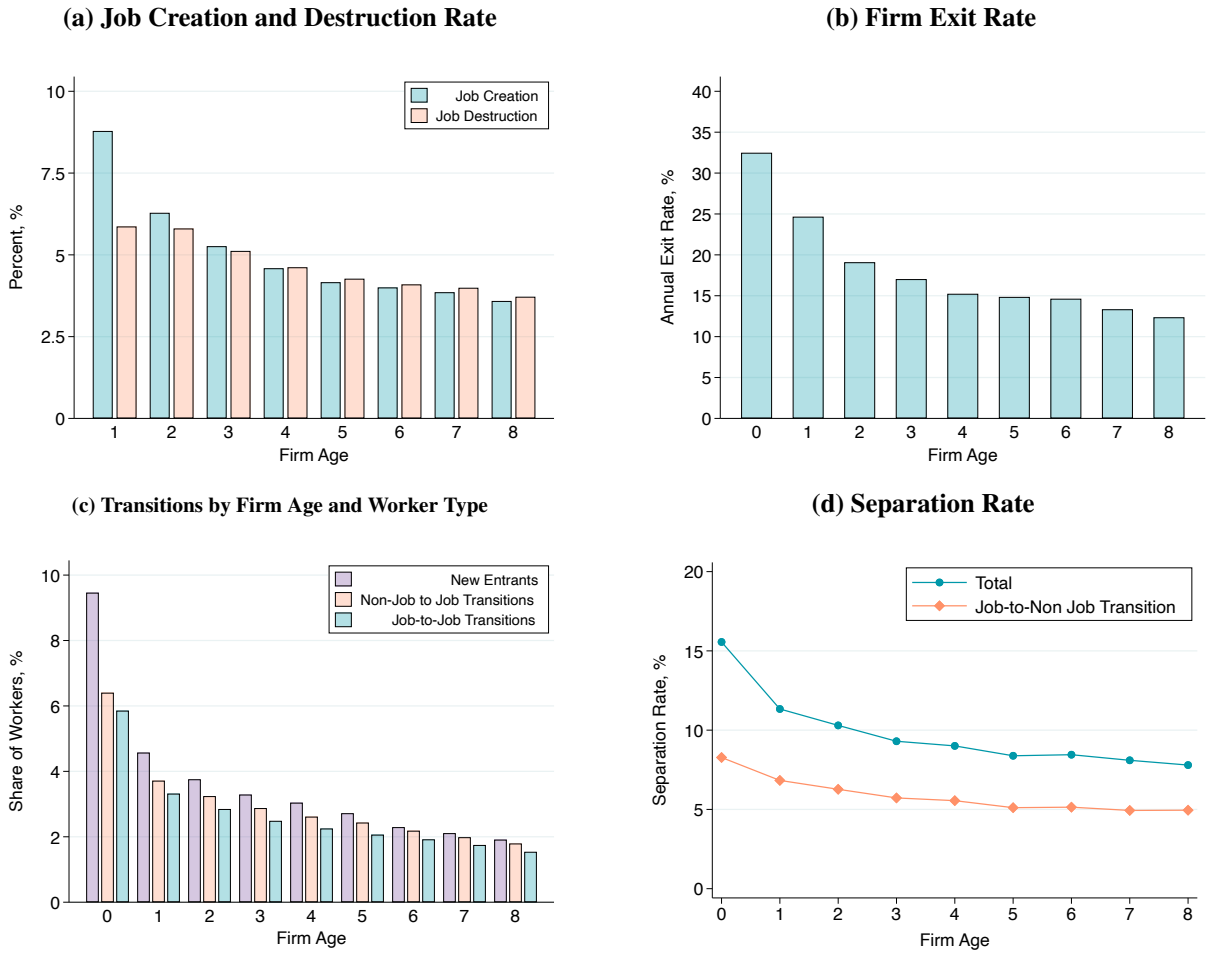
Panel (b) shows that the firm exit rate decreases with age, and in particular, startups have higher exit rates than older firms. Startups have an annual exit rate of around 33%, while firms aged five to eight exhibit exit rates of less than 15%. In panel (c), we decompose transitions into new jobs by firm age and transition type. We classify the transitions into 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 Job Transitions),

[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.

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 2.3: 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

2.4 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.

2.4.1 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:

$$\texttt{ihs}(\text{Earnings}_{it}) = \beta \text{Startup}_i + \phi_s^{\text{init}} + \lambda_t + \rho_\tau + \boldsymbol{\Lambda}' \boldsymbol{X}_i + \varepsilon_{it}, \tag{2.1}$$

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 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 value of the dependent variable 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

[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.

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 controls would mean that the startup coefficient would be affected by the earnings effect in those sectors. In other words, if startups are concentrated in some particular sectors exhibiting wage premia/penalties, without the sector controls, our estimate of γ would 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 2.5 reports the results of the OLS specification with no controls and no 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]

[12] In Appendix A.1.1, 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.1.1.

[13] We calculate the semi-elasticity as $\epsilon \approx 100 * \frac{\texttt{sinh}(\hat{\beta} + \alpha)}{\texttt{sinh}(\alpha)} - 1$ where `sinh()` is the hyperbolic sine function

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.

In column (2) of Table 2.5, 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 2.5: 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 2.1 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 2.2. Column (4) follows the specification in equation 2.2 but restricting to periods with positive earnings. Column (5) adds a contemporaneous sector fixed effect to the specification in column (4).

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.

2.4.2 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 covari-

ates as the OLS does and is, therefore, more flexible. We run the following regression

$$\texttt{ihs}(\text{Earnings}_{it}) = \gamma\ \text{Startup}_i + \phi_s^{\text{init}} + \lambda_t + \nu_g + \epsilon_{it}, \tag{2.2}$$

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 2.5 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.

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. To do this, we first we run the specification described by equation 2.2 but *excluding* periods without formal employment. This gives us an estimate of the portion of the difference due to differences in monthly earnings. The portion due to differences in periods of non-formal employment is then estimated by subtracting the difference due to differences in monthly earnings from the overall difference.

Column (4) of Table 2.5 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 contemporaneous sector 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 are the same.

Column (5) of Table 2.5 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.

2.4.3 Dynamics of the Earnings Effect

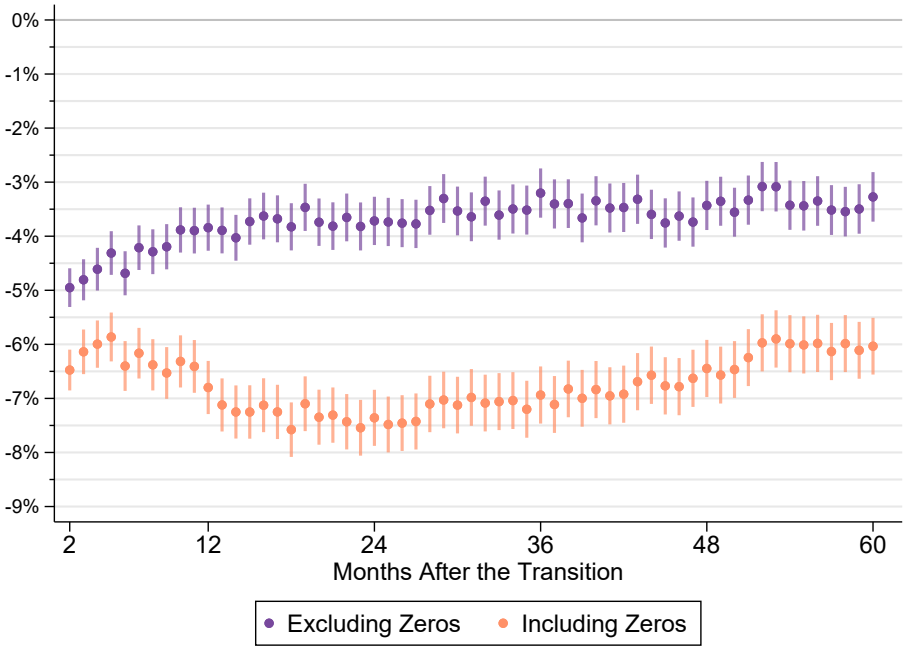
We are now ready to characterize differences in the path of earnings between individuals who transitioned to a startup vs. those that transitioned to an established firm during our five-year period of analysis. To this end, we estimate one regression per time period after the transition, comparing the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

$$\texttt{ihs}(\text{Earnings}_{ik}) = \beta_k \text{Startup}_i + \lambda_t + \nu_g + \varepsilon_{ik}; \quad k = \{2, ..., 60\}. \tag{2.3}$$

This equation is effectively a cross-sectional regression at different engths of time since the initial transition where our panel dictates the time dimension. Figure 2.4 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 2.5. 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 2.4: Earnings Effect Dynamics



Note: This figure shows estimates of β_k in equation 2.3. The orange estimates use the sample that includes zero earnings periods. The purple estimates show the results excluding periods without formal employment.

2.5 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:

$$E_{it} = \beta_1 + \beta_2 \text{Startup}_i + \lambda_t + \nu_g + \xi_{it}, \tag{2.4}$$

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 2.6 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 2.4 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 2.6 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 2.3, 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 and, therefore, could have fewer of jobs over time. To better understand the net effect of these competing forces, we estimate the following equation :

$$\text{NJobs}_i = \delta_1 + \delta_2 \text{Startup}_i + \lambda_t + \nu_g + \varepsilon_i \tag{2.5}$$

Column (3) of Table 2.6 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.

2.6 Heterogeneity

The descriptive statistics in Table 2.3 shows that workers who take jobs at startups and workers who take jobs at established firms are systematically different. By using the triplets

Table 2.6: 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.

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 2.5 then 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.

2.6.1 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:

$$\texttt{ihs}(\text{Earnings}_{it}) = \beta Startup_i + \sum_{j=1}^{J} \xi_j D_j \times Startup_i + \sum_{j=1}^{J} \zeta_j D_j + \nu_g + \lambda_t + \epsilon_{it}, \qquad (2.6)$$

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 2.7 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 by age, the quintile of previous earnings, and the type of transition. 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 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

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

$$\varepsilon_j = 100 \times \left(\frac{\texttt{sinh}(\hat{\beta} + \hat{\xi}_j + \hat{\zeta}_j + \alpha)}{\texttt{sinh}(\alpha)} - 1 \right). \tag{2.7}$$

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 2.7: 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.

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.

2.6.2 Heterogeneous Effects – Firm Characteristics

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

2.6.2.1 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 as of 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 2.6. 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 2.8 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 firm 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 2.8, we report the results for seven selected sectors with a high impact on the average effect.[17] These 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.

[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 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.

2.6.2.2 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.

Table 2.8: 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%.

We look at two ex-post firm characteristics: surviving to 5 years and top performer– condi-

tioning 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 2.8 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 Panel (d) of Table 2.8, 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.

2.7 Why do People Move to Startups?

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

A possible 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 startup coefficients. Notice, however, that in our setting, we have a very rich set of controls. We are comparing workers who joined a startup vs. an established firm who are of the same gender, nationality and age, had similar previous income, changed jobs at the same time, spent the same amount of time out of employment before the transition, and moved to a firm in the same sector. While we cannot entirely rule out unobserved differences between the people who join a startup vs. an established firm, we believe it is less of a threat in our context than in many situations.

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 working at a young firm. This compensating differentials story is at odds with some of the heterogeneity results we show

above, in particular, the one that indicates that conditional on the firm's survival, the penalty of working at a startup is reduced by 80% (from 6.7% to 1.2%), as we show in table 2.8. If we assume that the probability of the business failing is random, then both failing and surviving startups share the similar feature of being young, 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.[18]

Finally, the macro labor literature relies on models with labor search and matching 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 a 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. In our setting, if the worker gets an offer from a startup she has to choose between unemployment and taking the offer. Papers that seek to quantify the relevance of search frictions find that they play an important role in the labor market. According to estimates by Taber and Vejlin (2020), search frictions account for up to 29% of wage dispersion in the US.

2.8 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

[18]This explanation does not hold if firms with an unobserved characteristic that is related to higher work earnings also have a higher survival probability, we can not rule this out by definition.

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.

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 for 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.

Chapter 3: Effect of Employment Protection Policies During the COVID-19 Pandemic on Short Tenure Jobs: Evidence from Chile

This Chapter is coauthored with Macarena Kutscher and Mariano Bosch both at the Inter-American Development Bank.

3.1 Introduction

All over the world, Governments have had to support their population's health, income, and labor market outcomes in the COVID-19 crisis. Latin American countries, particularly, have had to react fast in a challenging context of high labor informality and poverty. Some examples of public policies implemented in the region were monetary transfers, tax reduction, exemption from payment of services and labor subsidies, among others (see Cejudo et al. 2021 for a comprehensive review of income support programs in Latin America and the Caribbean).

Some of the programs implemented aimed to protect existing work relationships and prevent job losses. With numerous social distance measures in place and workplaces closed, the goal was to support household consumption and business survival. Furlough programs that allowed employers to put employees on temporary leave were common throughout the pandemic. In most cases, employees who were furloughed had access to subsidies or unemployment insurance (UI)

benefits.[1]

The value of preserving jobs is that it helps avoid spikes in job losses and that employers and workers avoid costly search, hiring, and training. However, a considerable cost of these types of schemes is that they can prevent the efficient reallocation of workers from non-viable jobs to industries and firms with better medium-term growth prospects (OECD, 2020). If that is the case, these policies postpone rather than prevent dismissals at a high cost for the parties involved.

Despite the fast-growing interest in furlough programs, little is known about their effectiveness. The main challenge in evaluating the impact of these policies stems from the fact that the use of the programs designed by these policies is not random, and most of the time, there are no credible sources of identification. Hence, there is no obvious control group to account for the self-selection into the program take-up. This is especially true for the current recession, as most of the countries extended job-retention policies to every firm and worker (Giupponi et al., 2021).

In April 2020, the Chilean government introduced law 21.227 or the *Employment Protection law* (EPL), which authorized firms to furlough workers or reduce their working hours. The EPL gave access to unemployment benefits to the workers affected by these measures. In a context where ordinarily there are constraints on firms' ability to dismiss workers, the government's goal was to offer relief to workers and firms affected by a reduction in labor demand due to the public health emergency. In this paper, we use rich administrative employer-employee data from the unemployment insurance system to provide evidence of the impact of this policy on job survival. Due to the characteristics of the crisis, most firms and their workers were eligible to apply for the program. However, there were some requirements related to the history of contri-

[1] In many European countries, such as Denmark, the government heavily subsidized furloughs. In the US, workers had access to their UI benefits.

butions to the unemployment insurance (UI) fund on behalf of the worker that we exploit in our identification strategy.

Between April and December 2020, 24% of the firms in the country applied to the program. Most of the applications were from firms of sectors that were especially hit by the pandemic, such as the Accommodation and Food Services (58%) and Arts and Entertainment (49%) sectors. Although the policy was more intensively used by micro firms (1-9 workers), most of the furloughed workers worked at large firms.

A total of 764,964 workers were furloughed during 2020, with workers with more tenure having higher chances of being furloughed. The statistics reported in this study indicate that, within the same firm, the probability of being furloughed was highest for workers with income in the sixth earnings decile. This fact, together with the evidence indicating that workers with higher levels of education have a higher prevalence of remote work, suggests that firms possibly chose to suspend workers with low potential for remote work but with specific human capital. Alternatively, they decided to lay off low-income workers and adopt telework for high-income workers.

A simple counterfactual unemployment rate assuming that firms would have dismissed all their furloughed workers in the absence of the EPL, indicates that, after the first wave, between April and September 2020, the unemployment rate would have been 5.4 percentage points higher on average, implying an average increase in the unemployment rate of 45%.

To estimate the causal effect of the EPL on the probability of survival of a job, we take advantage of the longitudinal dimension of the data as well as the variation in worker's eligibility at the time the law was implemented. In particular, we follow a difference-in-differences strategy comparing job survival probability of eligible jobs (i.e.jobs with more than two months of tenure

and a worker with at least six contributions to the UI system in the last year or jobs with more than three months of tenure) to non-eligible jobs (i.e., jobs with 0-1 month of tenure or with two months of tenure and less than six contributions in the last 12 months) within the same firm, before and after the Coronavirus pandemic. We consider first the entire universe of firms. In the next step, we restrict our analysis to those firms that furloughed at least one employee, with the idea of capturing the effect only in those firms that were most strongly affected by the pandemic. Those firms that filed for the program had to decide which workers to furlough, keep employed or dismiss. The eligibility threshold may have played a relevant role in their decision.

Our results for the full sample indicate that for the average low-tenure job in a firm that participated in the program, being eligible implied an increase in the job survival probability of 2.2 pp. The effect of the law was particularly large in sectors severely affected by the pandemic, like Accommodation and Food Services, where the effect was 16 pp, Arts and Entertainment, 4.7 pp, and trade, 3.19 pp. The coefficients are larger in magnitude when restricting our sample to only those firms that participated in the program.

Our paper contributes to the understanding of the impact of job-preserving programs. Most of the previous evidence comes from the Great Recession. For instance, Abraham and Houseman (2014) exploit cross-state variation in short-term compensation schemes implemented in the US during the Great Recession and estimate that they prevented approximately 22,000 layoffs. Looking at Swiss firms, Siegenthaler et al. (2019) study whether the short-term work (STW) program reduced unemployment in the Great Recession, comparing firms whose application was granted to those whose application was denied. The authors find evidence that STW increased firm survival and prevented a large number of dismissals. Cahuc et al. (2018) also find significant positive results of STW in the French context. They instrument the STW take-up during the

Great Recession using the geographical proximity of a firm to other firms that used STW before the recession. In addition, they exploit the variation in response time to applications across the jurisdictions.

A paper that is more closely related to ours is Giupponi and Landais (2020). They use administrative social security data from Italy during the Great Recession and exploit exogenous variation in short-time work eligibility rules to study the effects of STW on firms' and workers' outcomes. They find large and significant effects on firms' employment at both the intensive and the extensive margin.

There is little evidence of the role of job-preserving policies during the coronavirus pandemic. Recently, Bennedsen et al. (2021) studied the result of a national policy in Denmark, which included publicly subsidized furloughs that allowed workers to keep their jobs through the recession. Using firm-level survey data linked to administrative data, they found that the program was relatively well-targeted: most aid was taken up by firms that experienced severe shocks to their revenue and employment, and the program was effective in preventing layoffs and preserving job matches. To the best of our knowledge, ours is the first paper to analyze this policy in a developing country using a quasi-experimental variation.

Related to temporary layoffs in the Covid context, but closer to the macro literature, a couple of papers have looked at the effects of temporary layoffs on aggregate recovery.Buera et al. (2021) show that having the recall option in a model with labor market and financial frictions makes a recovery faster and more efficient. Gertler et al. (2021) for the US context shows that without the payroll protection program (PPP), unemployment would have been persistently higher: Firms would have recalled far fewer workers from temporary layoff, and more workers on temporary layoff would have ended up in more persistent unemployment. However, papers

that have looked at longer horizons offer a more nuanced result. Autor et al. (2022) find that the PPP's effect on jobs protected was positive but small and that the fiscal cost outweighed workers' earnings approximately 4 times.[2]

3.2 Overview of the country's economic and labor market before COVID-19

The statistics discussed in this section come from the National Survey of Employment (ENE) and correspond to the fourth quarter of 2019, the last quarter before the start of the pandemic.

3.2.1 Overview of the Chilean Labor Market

Chile has a working-age population of 15.5 million and 8 million workers. Even though Chile is a long country, 2653 miles, its economic activity is concentrated in the capital, Santiago, and its metropolitan area, with around 40% of the population in only 2% of the geographical area of the country. The top 3 sectors in terms of share of workers are trade, construction, and manufacturing, with employment shares of 18.1%, 9.4%, and 9.3%, respectively. Regarding the size of firms, 37% of workers work at a firm with ten or fewer workers, 28% work at a firm with 11-199 workers, and 35% work at a firm with 200 or more workers.

Chile has an informal labor sector of around 30% of the total employment. Figure 3.1 characterizes workers from the formal and the informal sectors. Workers with an educational level of high school or less are over-represented in the informal sector with a 73.8% share, compared to a share of 58.9% in the population. About 26% of the population of informal workers have

[2]The implied cost per year of employment retained was $169,000 to $258,000, equal 3.4 to 5.2 times median earnings

at least a higher education degree, compared to 46.6% in the formal sector. Also, as expected, there is a higher share of young workers (less than 30 years old) and old workers (more than 60 years old) in the informal sector relative to the formal sector. This characterization is important because in this paper, we study the effects of the *Employment Protection Law* only on the formal population due to data availability and the fact that the law was implemented through the formal unemployment insurance system.

Table 3.1: Characteristics of formal and informal workers

Variable	Formal workers	Informal workers	Total
% High school or less	53.0%	73.8%	58.9%
% Higher Education and more	46.6%	25.9%	40.7%
% Female	41.5%	44.4%	42.4%
% Less than 30 years old	20.0%	22.2%	20.7%
% 30-59 years old	69.9%	58.2%	66.6%
% more than 60 years old	10.0%	19.6%	12.7%

Note: Source: National Statistics Institute of Chile (INE).

3.2.2 Labor regulation in Chile

The Chilean labor market has two institutions that affect firms' incentives when deciding to retain, dismiss or furlough their workers. If firms choose to furlough their workers, they still have to pay the mandatory contributions, and if they dismiss them, then they may have to pay severance.

Mandatory Contributions: Normal times and EPL

Employers and employees are mandated several programs: pensions (10%), health insurance (7%), unemployment insurance (3%), disability insurance (1.26%), and work accident insurance (0.95%). This adds up to 22.2% of taxable earnings. In normal times, of the 3% for

unemployment insurance, 2.4% is paid by the firm and 0.6% by the worker. However, under the EPL, if the worker was furloughed, the firm had to pay the total of the social security contributions except for the work accident insurance, which was temporarily eliminated. This means that if a firm furloughed its workers, it still had to pay 21.3% of the taxable income. [3]

Cost of dismissal

The end of an open-ended contract can take one of four forms: mutual agreement or resignation; termination by cause; fortuitous event or force majeure; or company needs. The company needs category is very broad, including reductions in demand, low productivity of the worker or the firm, firm restructuring, or general economic conditions. When a firm lays off a worker due to company needs, it has to:

1. Give advance notice of 30 days or pay a month of salary in substitution for the 30 days notice

2. Pay any accrued vacation days not used.

3. Make a severance payment equivalent to a month of earnings for each year worked up to 11 years.

The other three causes of termination are not associated with any payments by the firm. Firms that use the EPL are prohibited from dismissing workers under the force majeure provision, forcing them to pay the costs described above.

[3] Source: standard contributions: `https://www.dt.gob.cl/portal/1629/articles-100141_archivo_01.pdf`. Changes under the EPL: `https://www.afc.cl/ley-proteccion-al-empleo/preguntas-frecuentes-empleadores`

3.3 Covid-19 timeline and aggregate changes

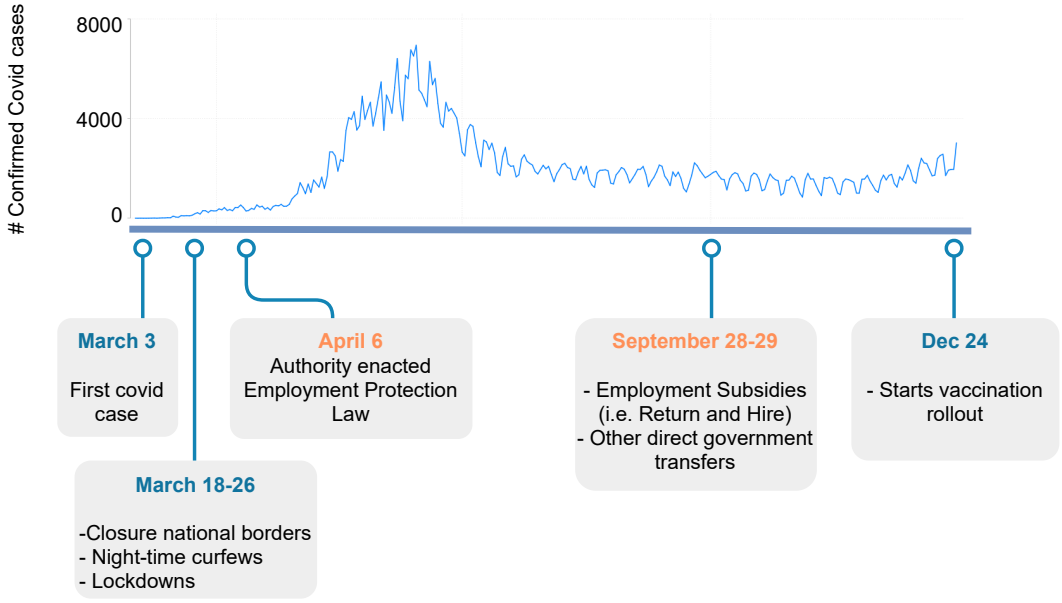
The first confirmed case of COVID-19 in Chile was reported by the health authority on March 3, 2020. As of mid-April, the country had more than 8,000 confirmed cases and ten deaths. To try to mitigate the spread of the virus, the Chilean government implemented several measures, such as the closure of schools and universities, enforced night-time curfews, mandatory use of face masks in public places, and increased controls on national borders, among others. More importantly, it declared lockdowns at the municipality level depending on the number and density of confirmed cases in each municipality.[4]

Regarding the restrictions and lockdown measures imposed on residents, if a municipality (or part of a municipality) was declared in quarantine, people who lived in that area could not leave their homes without legal authorization. Additionally, non-residents were not allowed to transit in quarantined areas. The first lockdown was during the final week of March in 7 municipalities of the Metropolitan Region and a few other cities across the country. Essential businesses were still open during this time (see Appendix B for a description of essential businesses). Across the country, there was substantial variation in the duration of these municipality-level localized lockdowns (Bennett, 2021; Li et al., 2021). By June 2020, the pandemic reached its first peak with 195 daily confirmed deaths and nearly 7,000 positive cases. The number of daily cases began to decrease slowly in the following months, reaching numbers between 900 and 1000 daily cases between October and November 2020, as displayed in Figure 3.1.[5]

[4]The criteria for lockdowns were loosely defined as a function of the number and density (per km2) of infectious COVID-19 cases, increases in case incidence, and health system capacity.

[5]Later in the year, the cases began to increase again. By March 2021, the country experienced a wave even larger than the first one. However, we focus on the first wave between April and September 2020.

Figure 3.1: Timeline



Note: *Source*: Government of Chile.

The pandemic had significant effects on various aspects. The National Employment Survey (ENE) showed a sharp decrease in private salaried employment during the first months of 2020, as shown in Figure 3.2. According to data from the Ministry of Labor, in March 2020, the number of layoffs reached almost 300 thousand people (an annual increase of 38% if we compare with the same month the previous year), while in April, it added another 238 thousand people (a yearly increase of 11.4%). This rise in layoffs has been especially strong in sectors such as Construction, Accommodation and Food Services, Trade, Transportation, and Other Services.

The country experienced a sharp increase in the share of workers working from home starting in March 2020. Figure 3.3 shows the percentage of workers in the formal sector working from home by their educational level. Even though the share of remote work increased across all educational levels, we can see a significantly higher prevalence, after March 2020, among work-

Figure 3.2: Total Salaried Employment

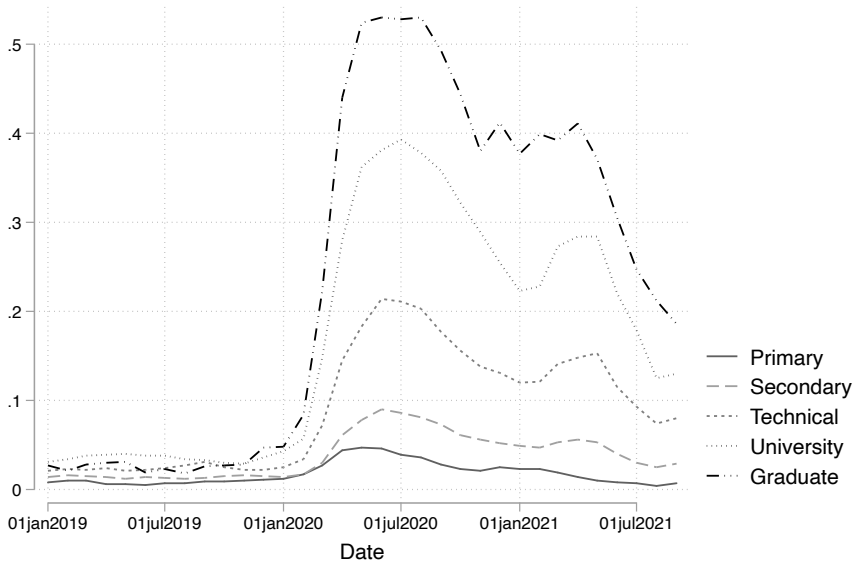
Note: *Source*: National Institute of Statistics of Chile, INE.

ers with at least a higher education. The percentage of the population working from home also varies significantly by economic sector. Figure 3.4 shows that Education, Finance and Insurance, Professional Scientific and Technical, and Information and Communication were the sectors with the highest shares of remote working between June and August of 2020. In contrast, Agriculture, Construction, and Mining were the sectors with the lowest proportions of their workers working from home.

An important adjustment factor in the labor market was the Employment Protection Law (EPL). This law allowed the temporary suspension or reduced working hours of employees of firms whose activities were totally or partially affected by pandemic restrictions, allowing the worker to access UI funds. The program is discussed in more detail in the next section.

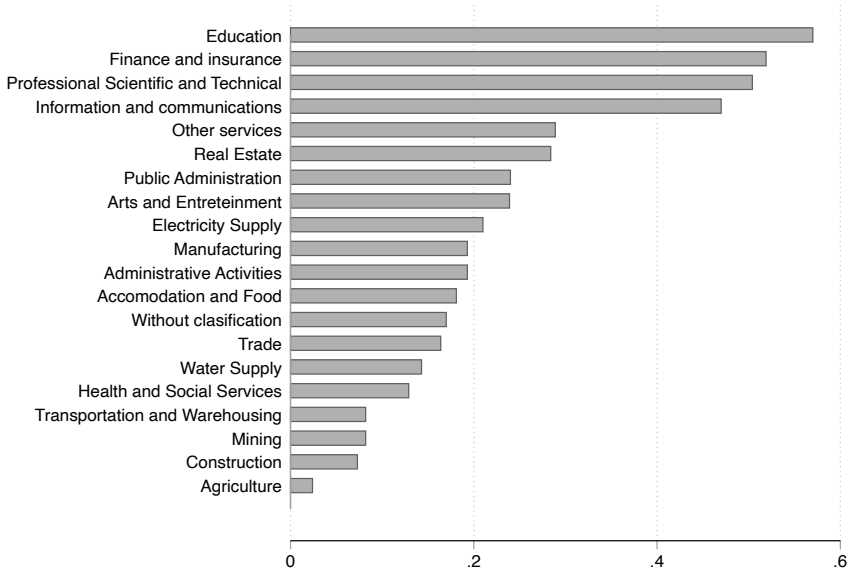
Along with the EPL, the government began to gradually introduce other types of aid, such

Figure 3.3: Share of workers in the formal sector working from home, by educational level



Note: *Source*: National Institute of Statistics of Chile, INE.

Figure 3.4: Share of workers in the formal sector working from home, by economic sector (June-August 2020)



Note: *Source*: National Institute of Statistics of Chile, INE.

as direct monetary transfer, subsidies, etc., as shown in Figure B.1.[6] An important form of government assistance was the *Ingreso Familiar de Emergencia* or IFE, which consisted of a direct monetary transfer and was initially focused on those households that depended mostly on informal income. [7] Between May and October 2020, it reached 1.2 million households and was later expanded to cover a greater percentage of the population.

In September 2020, after the first wave of the pandemic was over, there was a sense of hope that the worst of it was behind us. As a result, lockdown measures were relaxed in line with the guidelines provided by the government, and the government introduced the employment subsidies "Return" and "Hire" (*Regresa y Contrata*, in Spanish). These subsidies sought to encourage the hiring of new workers and the recall of their furloughed workers.

3.4 The Employment Protection Law

On April 6th 2020, the government enacted the Employment Protection Act, whose main objective was to protect family income and support firms and workers with extraordinary and temporary measures. The protection of job stability sought to ensure a quicker labor market recovery as activity rebounded.

The law authorized workers access to UI benefits in exceptional circumstances. In particular, employees were allowed to draw UI benefits in the event of (i) suspension of the employment contract by act of authority -known as mandatory furlough-; (ii) suspension of the employment contract by mutual agreement with the employer -known as voluntary furlough-; or (iii) agreement to temporarily reduce working hours-work sharing.

[6] These policies are described in more detail in Table B.2

[7] The amount depends on the number of people in the household and decreases according to the percentage of income that is formal.

Mandatory furloughs occur in the event of a territorial quarantine, which implies the shutdown of activities in all or parts of the country and prohibits the rendering of services. Employers whose activity was totally or partially affected by the pandemic could also sign a furlough agreement with their workers, known as a voluntary furlough. In addition, the law also allowed employers and employees to agree to a reduction in their working hours of up to 50%, with a proportional decrease in their salaries and social security contributions.

To qualify for the programs described above, employees had to meet certain requirements, in addition to being affiliated with the UI system.[8] To be eligible for furlough (options (i) and (ii)), employees must have 3 continuous contributions registered in the UI in the 3 months immediately prior to the request; or have a minimum of 6 continuous or discontinuous contributions registered in the UI in the last 12 months, provided that the last 2 contributions are with the same employer that is making the furlough request.

To qualify for the work-sharing program, the employee must: (a) have 10 monthly contributions, continuous or discontinuous, with the same employer in case of having an open-ended contract; or (b) have five monthly contributions, continuous or discontinuous, in case of having a fixed-term contract. In addition to the requirements on the workers to be eligible to enter into a work-sharing agreement, the firm should have experienced a reduction in sales of 20% confirmed by the tax authority.[9]

Firms and workers interested in using the programs filed an application with the Ministry of Labor. During the duration of the furlough or work sharing, employers were mandated to

[8] All private sector workers with contracts signed after 2002 are mandated to enroll in the UI system. Independent workers, public employees, officials of the Armed Forces, and retired people are not mandated to contribute to the UI system.

[9] As of October 2019, firms must have experienced a decrease in their average sales in any period of 3 consecutive months that exceeds 20% of their average sales in the same period of 3 months of the previous year.

continue making social security contributions. In the case of furloughed workers, the firm had to pay the full amount of the contributions, including the part that was normally paid by the workers. Under a work-sharing agreement, the contributions were prorated to the proportion of reduced hours, so if the hours were reduced 50%, the contributions were made using as a basis 50% of the taxable earnings. As noted before, firms could not dismiss their workers due to *force majeure* because of COVID. Employment agreements can only be terminated under article 161 (which includes i) *at will* in limited cases and ii) *need of the business*).

The UI payments for workers under the EPL correspond to a percentage of the average taxable earnings in the last three months before the agreement and depend on the type of contract, as shown in Table 3.2. The replacement rate (monthly allowance as a percentage of the worker's taxable income) begins at 70% and decreases every month. Payments are made with the resources of each affiliated worker's individual UI savings account. In the event that the money from the individual account runs out, payments are financed from the Solidarity Unemployment Fund.[10] [11] The replacement rate and the maximum and minimum caps depended on the type of contract, as shown in Table B.1.

Table 3.2: UI payments

	% Taxable Earnings	
Month	Fixed-term contract	Open-ended contract
1	70%	70%
2	55%	55%
3	45%	45%
4	40%	-
5	35%	-
6+	30%	-

Note: Source: Law 21.227

[10]The Solidarity Unemployment Fund is the social security component of the Chilean Unemployment Insurance system and includes contributions by both the State and employers.

[11]The government set up to contribute up to US $2,000 million to the Solidarity Unemployment Fund (Source: Law 21.227), to the extent that the fund required it.

After a few months of operation, the government changed the law. Initially, firms that provided services considered essential (and therefore not forced to cease their activities during mandatory quarantines) could not apply to benefits (i) and (ii). Those firms were only allowed to agree to a reduction of working hours with their employees. In June, however, they were allowed the option of furloughing workers who did not carry out essential activities. In September 2020, the government relaxed some requirements to access the program and increased the UI benefits, as shown in Table B.2. In particular, this amendment allowed suspended workers to receive higher payments and higher replacement rates.

Given this change in the program's requirements in September, which coincides with the introduction of new policies to incentivize the return of the furloughs workers due to the trough in the number of new COVID cases (see Figure 3.1), we evaluate the effectiveness of the EPL up to September. Hence, we focus on the short-term impact of the program.

In Figure 3.5 and table 3.3, we report the monthly unemployment rate for Chile in 2020, along with the *counterfactual unemployment* rate. This *counterfactual unemployment* rate is calculated assuming that all furloughed workers would have been fired in the absence of the policy, as in (Bennedsen et al., 2021). This analysis is likely to overstate the effect on the unemployment rate because employers could have furloughed workers that would not have been fired in the absence of the program.

However, this counterfactual exercise provides a useful upper bound of the potential benefit of the program. We find that for the period April 2020 to March 2021, the average unemployment rate would have been, on average, 3.6 points higher than the actual unemployment rate. The average between April and September is higher at 5.4 percentage points. During July, when the EPL had the highest take-up, the calculation implies a reduction of 6.2 percentage points in the

unemployment rate.

Table 3.3: Actual and counterfactual unemployment rate*

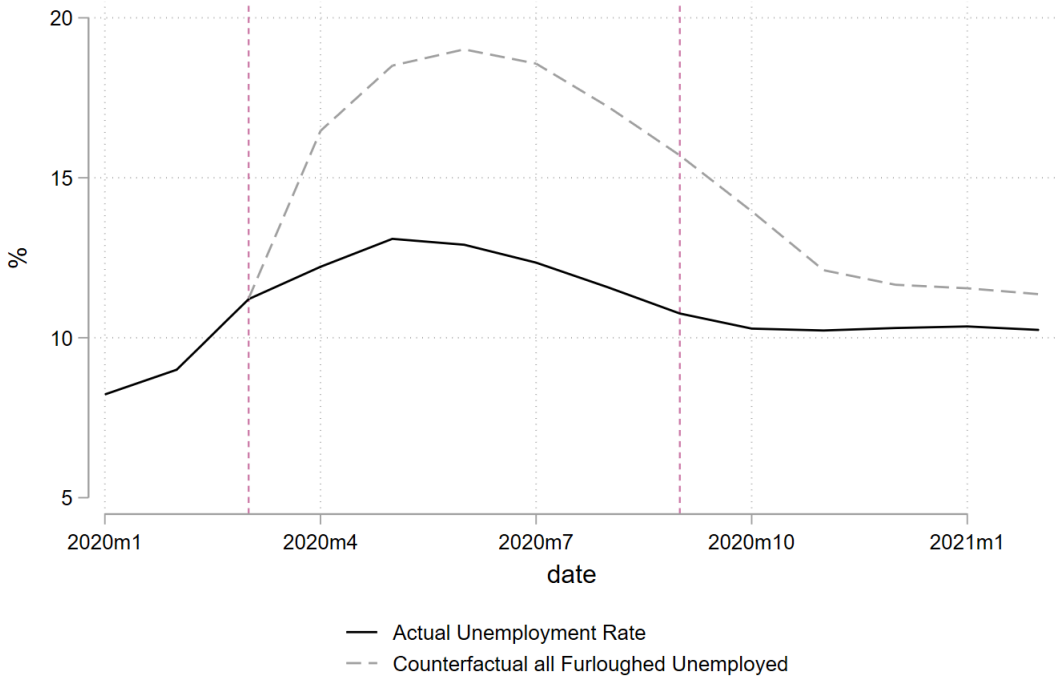
Year	Month	(1) Actual Ur	(2) Counterfactual Ur	(3) Gap
2020	April	12.2	16.5	4.2
2020	May	13.1	18.5	5.4
2020	June	12.9	19.0	6.1
2020	July	12.3	18.6	6.2
2020	August	11.6	17.2	5.6
2020	September	10.8	15.7	4.9
2020	October	10.3	14.0	3.7
2020	November	10.2	12.1	1.9
2020	December	10.3	11.7	1.4
2021	January	10.4	11.5	1.2
2021	February	10.2	11.4	1.1
2021	March	10.0	11.1	1.1
Mean		11.2	14.8	3.6
Mean Apr-Sep		12.1	17.6	5.4

Note: Notes: This *counterfactual unemployment* rate is calculated assuming that all furloughed workers would have been fired in the absence of the policy. Columns (1) and (2) are the estimates plotted in Figure 3.5

3.4.1 Descriptive Statistics on the use of EPL

In this section, we describe the usage of the EPL and provide a first look at the differences at the sector level in the intensity of the policy take-up. Figure 3.6 shows that most of the requests were for the furlough program, while there was a lower take-up of the work-sharing agreement. The reason behind the difference in take-up between both types of programs is unknown, but could be related to the extra requirements imposed on firms to be able to reduce the work hours (discussed above), or due to the nature of the lockdown restrictions, which led some firms to shut down their operations completely instead of partially reducing their output. Due to the relative importance of furloughs, we focus our analysis on identifying the impact of the furlough

Figure 3.5: Unemployment rate in Chile

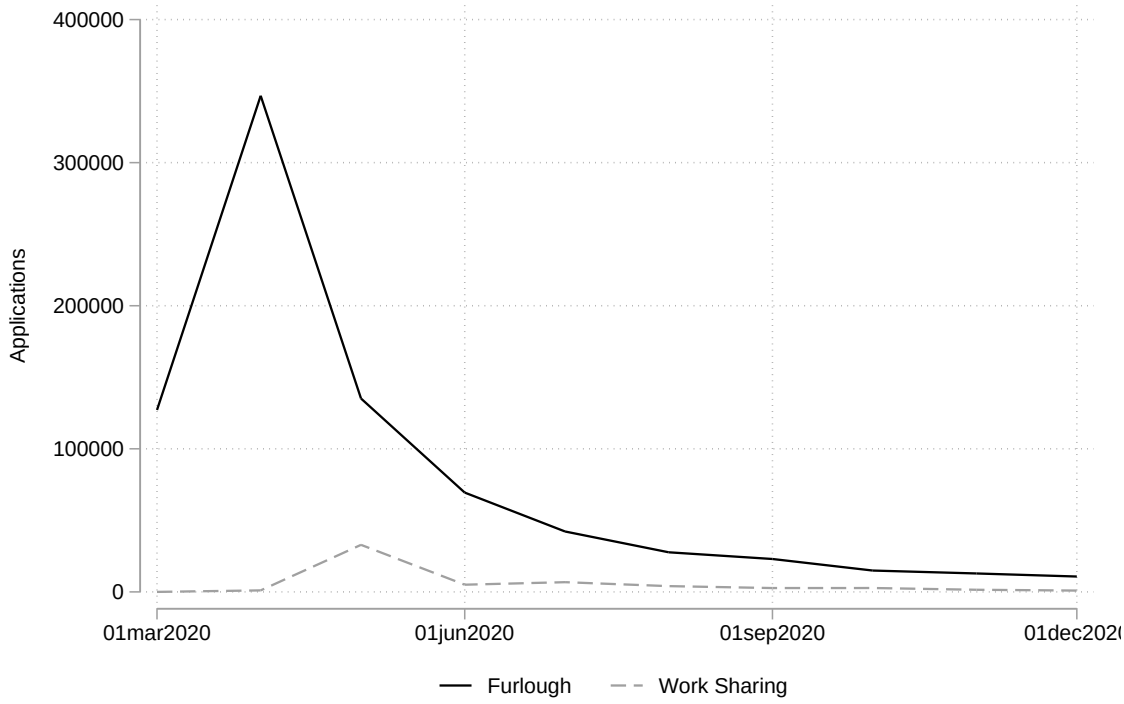


component of the EPL.

Table 3.4 shows the EPL usage by sector. The main takeaways are that the Accommodation and Food Services and the Arts and Entertainment sectors had the largest share of firms using the program and the largest share of furloughed jobs. If we compare the employment growth in each sector with the share of firms using the EPL, we find a negative correlation of -0.74, meaning that the sectors that used the EPL more intensively were the ones that saw their employment decrease the most.

Most (76%) of the firms who used the EPL were micro firms, and only 1% were large firms (column 7). However, as a proportion of the total number of firms in each segment, only 28% of micro firms made furlough requests, the lowest percent across firm size (column 6). Finally, column 8 shows that 31% of furloughed workers work at large firms, even though only 9% of

Figure 3.6: Applications to EPL, by type of benefit



Note: Source: Employment Protection Law, Ministry of Labor, Chile.

jobs in large firms were suspended (column 5). In summary, while the policy was utilized more intensively by micro firms, most of the furloughed workers worked at large firms.

Other determinants that influence the usage of the EPL, aside from the firm sector and size, are the firm's age and the region where the firm is located. As a descriptive exercise, we run a logistic regression to study the determinants of the probability that a firm will use the EPL. Results are displayed in Section B.1. We consider all firms that were active just before the pandemic (in February 2020) and classified as participating in the program firms that suspended at least one worker between March and September 2020. The results of this multivariate analysis corroborate our previous bivariate analysis and indicate that firms with an age of 3 years and higher have higher chances of furloughing a worker, as well as firms located in the Metropolitana

Table 3.4: EPL usage by economic sector

	Pre-pandemic			EPL Usage	
	Share firms	Share jobs	Employment growth*	Share Firms	Share Jobs
Accommodation and	6.59	5.30	-28%	58%	41%
Administrative A	4.95	10.29	-1%	32%	8%
Agriculture	8.13	8.36	0%	7%	2%
Arts and Entrete	0.89	0.64	-24%	49%	39%
Construction	9.13	11.70	-15%	35%	25%
Education	2.37	6.10	-8%	28%	5%
Electricity Supp	0.17	0.37	-1%	16%	6%
Finance and Insu	2.46	6.40	6%	19%	2%
Health and Socia	3.88	3.24	4%	36%	9%
Households as Em	1.07	0.25	-7%	17%	16%
Information and	1.97	2.43	-2%	25%	7%
Manufacturing	7.23	8.72	-5%	36%	15%
Mining	0.38	1.38	-1%	18%	3%
Organizations	0.06	0.04	-3%	11%	8%
Other Services	8.51	5.19	-2%	20%	8%
Professional Sci	7.26	4.53	-5%	29%	12%
Public Administr	0.15	1.66	-1%	5%	0%
Real Estate	2.21	0.79	-8%	27%	18%
Trade	21.67	15.57	-7%	36%	20%
Transportation a	9.91	6.31	-6%	24%	11%
Water Supply	0.77	0.66	1%	8%	3%

Note: Source: Employment Protection Law, Ministry of Labor, Chile.
Note: Employment growth is calculated between September 2019 and September 2020, to account for seasonality factors.

and Tarapacá regions.

The disruptive effects of the pandemic are related to the ability of firms to operate under lockdown conditions. At a first glance, sectors that report a higher share of remote work tend to use the furlough program less intensively.

Firms must decide if they want to participate in the program, and if they do, which workers to furlough, keep employed, and fire. Hence, we now look inside those firms and investigate which workers were more likely to be furloughed. We have information about workers' characteristics, such as gender, age, and monthly earnings from the administrative data. Additionally,

Table 3.5: EPL usage by firm size

	Pre-pandemic				EPL Usage			
	Share firms (1)	Share jobs (2)	P.P. change firms* (3)	P.P. change jobs* (4)	% $jobs^a$ (5)	% $firms^b$ (6)	% firm $usage^c$ (7)	% jobs $usage^d$ (8)
Micro 1-9	82%	15%	1.1%	0.8%	24%	28%	76%	26%
Small 10-49	14%	19%	-0.8%	-0.2%	19%	41%	19%	27%
Medium 50-199	3%	18%	-0.2%	-0.3%	12%	37%	3%	16%
Large 200+	1%	48%	-0.1%	-0.3%	9%	39%	1%	31%

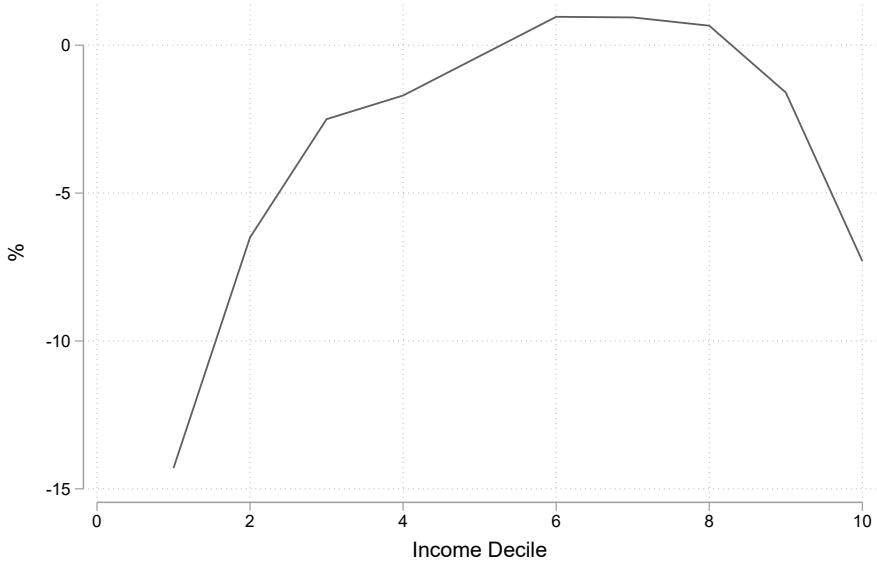
Note: Source: Employment Protection Law, Ministry of Labor, Chile.
Notes: *Changes in share are calculated between September 2019 and September 2020, to account for seasonality. [a]: Corresponds to the number of furloughed jobs by firm size over the total number of jobs by firm size.[b] Corresponds to the number of firms that used the EPL, by size, over the total number of firms by size. [c] Corresponds to the number of firms using the EPL by size over the total number of firms using the EPL. [d]: Number of furloughed jobs by size, over the total number of furloughed jobs.

given that we have access to the entire payroll history, we can build the tenure of each work relation.

We found an interesting pattern across the wage distribution. More precisely, as Figure 3.7 shows, the probability of furlough has an inverse u-shape. The likelihood of being furloughed increases as the wage increases up to the sixth decile; from the seventh decile onwards, the probability of furlough declines as the wage decile increases. The increasing part of the relationship between wages and the likelihood of being furloughed is consistent with firms being more willing to dismiss workers on the lower end of the earnings spectrum than those with earnings around the median.

An explanation behind this behavior could be that workers with lower wages are easier to replace than those with wages at the median. Thus, if the firm has to choose between dismissing or furloughing these workers, it will likely decide to preserve the relationships that are harder to replace. The decreasing pattern after decile seven could be explained by workers working

Figure 3.7: Likelihood of furlough across earnings



Note: Source: Authors' calculations based on official data of Unemployment Protection Law from Ministry of Labor, Chile.

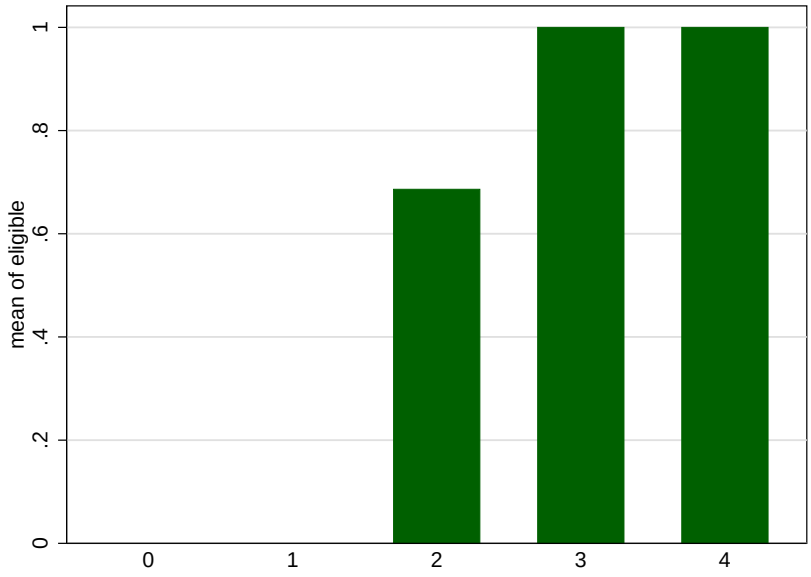
remotely at the higher end of the earnings distribution. For workers who can work from home, the choice set of the firms might be larger; they can choose to keep them active and have them work from home, furlough them, or dismiss them. The analysis also coincides with the statistics of the National Statistics Institute of Chile. For example, Figure B.3 in the Appendix displays the share of temporarily laid-off workers in the formal sector who were receiving salary or earnings between 2019 and 2021, by educational level, which proxies for workers under the EPL. The figure confirms that formal workers with a University or a Graduate degree were less likely to be furloughed than workers with primary, secondary, or technical education.

The fact that workers with high earnings are less likely to receive payments from the EPL is good news for the equity implications of the program. However, those in the lower-income decile are the ones least likely to benefit from the program as well. This is an argument supporting complementary measures to mitigate the loss of income induced by employment loss, such as the

program *Ingreso Familiar de Emergencia*, that supported vulnerable households with insufficient informal or formal income that have been affected by the pandemic.

Another relevant dimension when choosing who to furlough within a firm is tenure. Those with higher tenure are more likely to have firm-specific capital. In addition, given the severance payment in Chile, the longer the tenure, the higher the dismissal cost. Hence, as tenure increases, the firms have incentives to furlough instead of dismissing the workers. Importantly, due to the program requirements, there is a structural discontinuity in the first months of tenure. Workers must have at least two months of tenure in their jobs to be eligible for UI payments, as shown in Figure 3.8.

Figure 3.8: Share of Eligible by Tenure in Months



3.5 Data description

We use administrative data from the institution that administers the pension system in Chile (*Administradora de Fondos de Cesantias*- AFC) on the universe of employer-employee matches

and social security payments in the private sector in Chile from 2016-2021. The data includes detailed information of workers' characteristics such as gender, year of birth, nationality, earnings history, type of contract, and participation in social and training programs. It also includes information on firms' characteristics such as labor-force composition, location, industry, and number of workers. Information is collected from the employer and must be submitted within 10 days of the hiring date to the AFC.

We also have information on all the employer-employee requests to the Employment Protection Law, either for furlough or work-share. The dataset contains information on the date of request, type of benefit, status (approved, rejected, pending), the reason for rejection, start and end date of the agreement, and number of payments, among others.

Finally, we use information from the National Statistics Institute (INE), which, as part of its regular survey on wages and labor costs, begining April 2020, includes questions associated with the pandemic situation. In particular, they have asked employers how many of their workers are using the Employment Protection law and how many are working remotely, among other questions. The survey is representative at the level of the economic sector (ISIC Rev. 3), and by firm size category.

For the rest of the analysis, we focus on the period from April 2020 until September 2020. There are several reasons behind evaluating the effects of the policy through September 2020. First, in September, the government relaxed the lockdown measures and enacted policies to promote the recall of furloughed workers, and the hiring of new ones as the first wave of the pandemic was over. Second, the government made substantial changes to the EPL at that time, as described in section 3.4. And third, the take up of the policy was concentrated in the first wave of COVID. For the second wave, the government shifted its efforts to direct payments to people. More de-

tails about the direct transfers the government distributed and their growing size over time can be found in figure A1.

Our outcome variable is the job status by September. If we do not see UI contributions for at least one of September, October or November, we classify a job as lost. We discuss the implications and the rationale behind our sample selection in more detail in the next section.

3.6 Identification Strategy

There are several challenges to identifying the causal effects of employment protection programs. In our setting, the policy was implemented at the national level, precluding geographic identification strategies. A naive comparison between the survival probability of jobs that were furloughed and jobs that were not will not give us the actual effect of the policy. Jobs that were furloughed could be different from jobs that were not furloughed in several relevant dimensions. The differences could be related to sector characteristics, firm characteristics or worker characteristics.

In this section, we discuss our methodology to overcome these limitations. We take advantage of the longitudinal dimension of the data as well as the variation in workers' eligibility under the Employment Protection Law based on job tenure and previous contributions to the UI system to offer evidence of the effects of the program on the probability of job survival. In particular, we follow a difference-in-differences strategy comparing job survival probability of eligible workers to non-eligible workers, before and after the Coronavirus pandemic.

The difference-in-differences strategy eliminates any existing time-invariant differences between the groups. For instance, if the trial period of workers is three months, the probability of

job survival will likely be higher for those above the eligibility threshold because they have already passed the trial period. This identification strategy removes that systematic difference and prevents us from attributing the effect of having passed the trial period, for example, to the policy itself.

Given the evidence presented in Section 3.4.1, it is very likely that the pandemic affected high and low-tenure workers differently. Therefore, we restrict the analysis to workers just below and above the eligibility threshold. Hence, the identification relies on the assumption that eligible and non-eligible jobs with tenure under five months would have had a similar turnover in the absence of the EPL.

Our estimating equation is a Linear Probability Model (LPM) of the form:

$$Survives_{jft} = \beta_1 Eligible_{jf} + \beta_2 Post_t + \beta_3 Eligible_{jf} \times Post_t + \gamma_f + u_{jft} \tag{3.1}$$

where $Eligible_j$ is a dummy variable identifying jobs that met the eligibility requirement in April of each corresponding year.[12] We use data from April of each year because of the seasonal component of the share of workers with short tenures. $Post_t$ equals one if the year is 2020 and zero between 2015-2019. We only include jobs with tenure under five months.

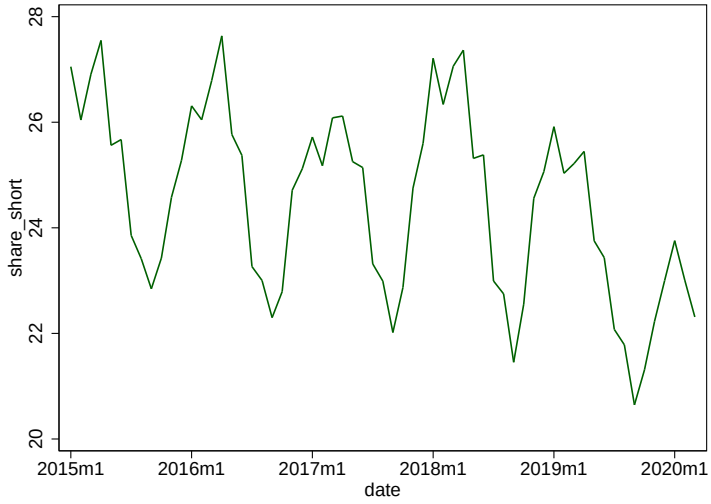
We classify a job as surviving if it existed in April 2020 and had contributions to the UI system in September, October, or November 2020. This means that we include active jobs and potentially furloughed jobs. In our baseline specification, we exclude jobs that ended due to quits or the end of fixed-term contracts. The rationale behind this exclusion is that the EPL was

[12]Note that we built the eligibility using the same requirement for 2015-2019, even though for those years, there was no option to furlough workers.

intended to reduce layoffs due to the negative demand shock, not voluntary separations. [13] We include firm fixed effects to account for turnover differences at the firm level. Notice that since our observations are at the job level, including firm fixed effects does not restrict our sample to surviving firms. We use variation from both surviving firms and non-surviving firms when estimating our coefficients.

In Figure 3.9 we show that there is a big role for seasonality in the share of eligible jobs in the economy. We suspect that this is related to the high season in the summer. January and April are consistently the months with the highest share of workers with tenure under five months. However, notice that regardless of the season, jobs with low tenure are very common in the Chilean economy. The share of short-tenure jobs is always above 21%. This means that even if the effect that we are identifying is local, it is a very relevant effect.

Figure 3.9: Share of low tenure jobs, by month



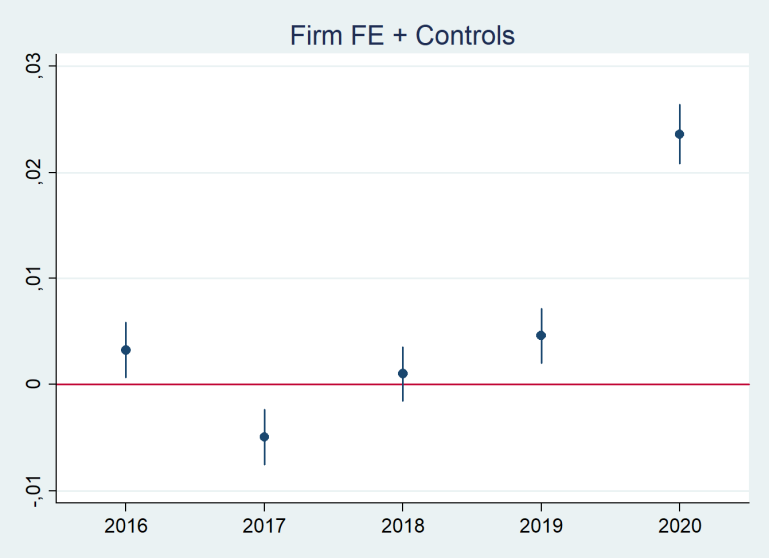
Note: Source: Authors' calculations based on EPL official data from the Ministry of Labor, Chile.

A crucial assumption of the difference-in-differences strategy is that we expect to observe

[13] In Table B.5, we show the results of our baseline specification, including quits. The overall results are significant but qualitatively smaller than the baseline results.

parallel trends in baseline outcomes across eligible and non-eligible jobs. In other words, it implies that both groups, eligible and non-eligible, would have followed a parallel trend in survival probability in the absence of the pandemic. We visually inspect the pre-pandemic dynamics of the treatment and control groups in Figure 3.10. We can see that before 2020, the difference in survival probabilities between eligible and non-eligible jobs is close to zero, whereas, in 2020, that difference increased significantly.

Figure 3.10: Pre-trends



Note: Note: In this figure, we report the coefficients of the interaction of eligible and each year fixed effect on the survival probability of a job. We use a linear probability model with firm fixed effects and controls for age and gender. The sample is restricted to jobs with tenure under five months.

3.7 Results

Table 3.6 shows the results of equation 3.1 for different samples. The relevant coefficient is the interaction of $Eligible \times Post$, which captures the effect of the EPL on the survival of jobs under five months of tenure. All regressions control for the age and gender of the worker and

include firm fixed effects. In column 1 we report the results for all sectors of the economy. For low-tenure jobs, being eligible increases the probability of survival by 2.2 percentage points.

Columns (2)-(5) report the results for different sectors of the economy. The impact of the EPL seems to be larger in sectors that were more affected by the pandemic, such as the Accommodation and Food Services sector. In that specific sector, being eligible to be furloughed increases the probability of the job surviving by 16 percentage points. This effect makes sense as this sector has a low incidence of remote work as previously discussed.

Table 3.6: Effect of the EPL on the survival of jobs

	(1) All sectors	(2) Trade	(3) Manufacturing	(4) Acomm. and Food	(5) Arts and Entr.
Eligible=1	0.0551***	0.0496***	0.0776***	0.0573***	0.0244***
	(127.15)	(42.85)	(51.31)	(31.91)	(4.68)
Post=1	-0.0206***	-0.0370***	0.0102***	-0.171***	-0.0921***
	(-25.51)	(-15.56)	(3.38)	(-40.23)	(-8.22)
Eligible=1 × Post=1	0.0224***	0.0319***	-0.00918*	0.160***	0.0465**
	(19.82)	(9.77)	(-2.25)	(27.99)	(3.15)
N	5479953	747666	443866	371015	31597

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Note: The table contains the coefficients of equation 3.1 for different samples. We include only jobs with less than 5 months of tenure. The dependant variable is a dummy variable equal to one if a job that existed in April survives up to September. We define the survival dummy as one if the same combination of firm and worker is recorded in the unemployment insurance records in September, October, or November. All Samples exclude quits, include firm fixed effects, and control for age and gender. We include data from 2015 to 2020. The eligible variable is equal to one if the job had three months of tenure or if the worker had at least six contributions to the UI system in the last 12 months. 67% of jobs with tenure of two months are eligible.

In Table 3.7 we repeat the analysis but only for the sample of firms that furloughed at least one worker in April 2020. The idea is to capture the impact of the program on those firms that were most impacted by the pandemic crisis. As expected, the effect of the EPL on the survival

probability of low-tenure jobs is higher, as being eligible increases the probability of survival by 4.5 percentage points. We find a similar pattern when we look at different sectors.

Table 3.7: Effect of the EPL on job survival - Sample of Firms that furloughed at least one worker

	(1) All sectors	(2) Trade	(3) Manufacturing	(4) Acomm. and Food	(5) Arts and Entr.
Eligible=1	0.0629***	0.0435***	0.0659***	0.0712***	0.00421
	(72.91)	(20.43)	(19.88)	(29.19)	(0.56)
Post=1	-0.0389***	-0.0756***	0.0161*	-0.219***	-0.248***
	(-22.44)	(-14.78)	(2.18)	(-40.11)	(-11.67)
Eligible=1 × Post=1	0.0446***	0.0609***	-0.00141	0.225***	0.0902***
	(19.19)	(9.08)	(-0.14)	(30.49)	(3.58)
N	1359126	200844	87063	178798	13485

t statistics in parentheses

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Note: The table contains the coefficients of equation 3.1 for different samples. Only jobs with less than 5 months of tenure and that are in firms that furloughed at least one worker in April are included. The dependant variable is a dummy variable equal to one if a job that existed in April survives up to September. We define the survival dummy as one if the same combination of firm and worker is recorded in the unemployment insurance records in September, October, or November. All Samples exclude quits, include firm fixed effects, and control for age and gender. We include data from 2015 to 2020. The eligible variable is equal to one if the job had three months of tenure or if the worker had at least six contributions to the UI system in the last 12 months. 67% of jobs with tenure of two months are eligible.

In the Appendix, we include 3 robustness exercises. Table B.4 shows the results following alternative specifications for our baseline sample. Overall the effects are economically and statistically significant under any of the specifications included. We find robust evidence of a positive effect of the EPL on job survival in the short term.

3.7.1 Heterogeneous Effects: Worker Characteristics

In this section, we explore whether the effect of the EPL differs by worker characteristics. We look at the effect across income decile, gender, and age. Figure 3.11 shows the coefficients

of the interaction $Eligible \times Post$ that we get from running the specification in equation 3.1 by income decile. The main message of the Figure is that the effect seems generally to be decreasing with earnings decile. The coefficients are ordered in three distinct groups: with the first and the second decile in the low earnings group, the third to the sixth deciles in the middle earnings group; and the seventh to the tenth deciles in the high earnings group. The impact of the EPL on the short-term survival probability of low-tenure jobs is larger for jobs in the low-income group, with an average of 5 percentage points of additional survival probability due to the policy. Next, the EPL has an average effect on the survival probability of 3 percentage points for jobs in the middle earnings group. Finally, the EPL did not seem to have an effect on jobs in the high earnings group.

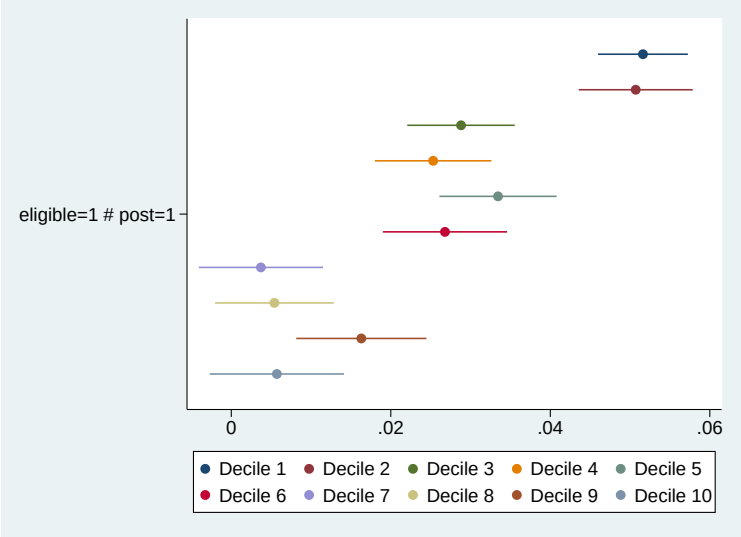


Figure 3.11: Effect of EPL by Income Decile

Note: Note: This figure reports the coefficient of the interaction $Eligible \times Post$ when we estimate equation 3.1 separately by income decile. For all the estimations we restrict the sample to jobs with less than 5 months of tenure. See notes of Table 3.6 for more details.

Turning to gender, in the first two columns of Table 3.8 we find that the EPL had a larger effect on women than on men. The EPL increased the probability of survival by 3 percentage

points for women and 1.8 percentage points for men, a 67% difference in the effectiveness of the policy by gender.

Table 3.8: Survival in September of April Jobs: Heterogenous Effect

	(1) Female	(2) Male	(3) 18-35	(4) 35+
eligible=1	0.0666***	0.0500***	0.0541***	0.0547***
	(91.41)	(90.34)	(90.26)	(83.43)
post=1	-0.0358***	-0.0129***	-0.0152***	-0.0308***
	(-28.47)	(-11.97)	(-13.41)	(-25.68)
eligible=1 × post=1	0.0295***	0.0184***	0.0222***	0.0260***
	(15.57)	(12.63)	(13.86)	(15.63)
N	2,052,335	3,333,148	2,912,093	2,459,030

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Finally, we asked whether there were differences in the effect of the EPL for younger or older workers. We divided the sample at age 35 and estimated equation 3.1 the older and younger groups separately. The results are included in the last two columns of Table 3.8. The coefficients are not statistically different.

3.8 Cost and Benefit Analysis

One of the features of this program is that most of the money used for the benefits came from UI funds. The UI system in Chile has two components, an individual account, and a solidarity fund. The source of the UI benefits is first the individual account and then the solidarity fund. During the pandemic, the government committed up to USD $2 billion to the solidarity fund.

Making a cost-benefit analysis is complex in this setting. First, in terms of the cost, the main challenge is to think about the relevant counterfactual. In the absence of the EPL, some of

the workers that ended up benefiting from the law would have been dismissed, but others would have kept their jobs. Given that our analysis focuses on workers with short tenure, it is likely that they possess little to no specific human capital, so the firm, in the absence of the law, would likely have preferred to dismiss them when facing a mandatory quarantine. If workers would have been dismissed in the absence of the law, then it is reasonable to think the law had zero cost, in that dismissed workers would have drawn similar benefits as furloughed workers.

Defining the benefits in this context also is challenging. Our results indicate that one of the benefits of the law is to increase the probability of job survival by 2.2pp for short-tenure jobs. The benefit of the policy is going to depend on how much longer the working relationship survives, and we cannot quantify this given that by the end of the policy, the pandemic was still in place. If a job ended after the policy ended, we cannot say whether it ended because of the absence of the policy or because of the pandemic. A useful exercise, to be performed in future research, to get a sense of the persistence of the policy's effects would be to keep track of the eligible and non-eligible workers defined at the onset of the policy and verify if at different horizons, their outcomes are similar or diverge.

3.9 Conclusions

COVID-19 has presented a great challenge to governments around the world. Various OECD countries have made use of job retention schemes. In this paper, we analyze the effectiveness of the job retention policy implemented in Chile during the pandemic, the Employment Protection Law. First, we perform a descriptive analysis. We construct a counterfactual unemployment rate assuming that all furloughed workers would have been dismissed in the absence

of the EPL. We estimate that between April and September 2020, the unemployment rate would have been 5.4% higher on average, with the largest effect found in June at 6.2%. We then described the firms and the workers that benefited from this program. The program usage varied significantly across sectors, with those most affected by the pandemic participating more intensively. Most of the firms who used the EPL were micro firms, although most of the furloughed jobs were from large firms.

Firms must decide which workers to furlough, keep employed, or fire. Hence, we look inside the firms that furloughed at least one of their employees and show that the relationship between the probability of being furloughed and the earnings decile has an inverted u shape. The fact that workers with high earnings are less likely to receive payments from the EPL than workers in the median is good news for the equity implications of the program. However, those in the lower-income decile are the least likely to use the program. This argument supports complementary measures to mitigate the loss of income induced by employment loss.

Second, we seek to estimate the causal effect of the EPL on preserving jobs. One of the main challenges of identifying this effect is that furloughed workers can be systematically different than non-furloughed workers. We take advantage of the longitudinal dimension of the data as well as the variation in workers' eligibility to the EPL to offer evidence of the effects of the program on the probability of job survival. We follow a difference-in-differences strategy comparing the job survival probability of eligible workers to non-eligible workers within the same firm before and after the Coronavirus pandemic.

A main caveat is that if there is a differential impact of the pandemic for groups above and below the threshold, our coefficient might be biased. Hence, we restrict our analysis to low-tenure workers just above and below the eligibility requirement. We also exclude jobs that do not

survive due to quits or because of the end of the contract.

Our results indicate that for the average low-tenure job in a firm that participated in the program, being eligible implies an increase in the survival probability of 2.2 percentage points. The effect of the law was particularly high in sectors severely affected by the pandemic, like the Accommodation and Food Services sector, where the effect was 16 percentage points, the Arts and Entertainment sector with 4.5 percentage points, and the Trade sector with 3.2 percentage points. The effects are higher when we restrict the sample to those firms that at least furloughed one worker. The EPL had larger effects in preserving jobs in the lower income deciles and for women. This means that more vulnerable workers benefited the most from this policy.

The reported effect corresponds to an estimate of the effect of the program at the beginning of the pandemic, after the first wave of infections. This result should not be generalized since, as other authors have pointed out, policies that seek to preserve the employment relationship tend to be more effective when crises are temporary and less effective when they are persistent or permanent (Giupponi et al., 2021).

Chapter 4: Lingering Effects of Recessions: Age Differentiated Migration

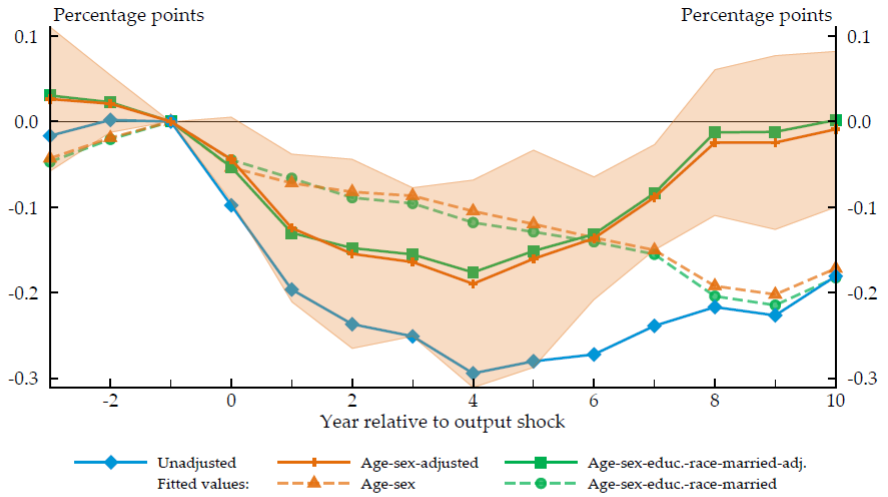
This chapter is coauthored with John Coglianese at the Federal Reserve Board

4.1 Introduction

Whether temporary shocks can have persistent effects is a first-order policy question. For example, suppose there are long-lasting consequences of transitory drops in demand. In that case, Central Banks should place more importance on preventing deviations from potential output and less importance on keeping inflation under check.

The general consensus in the literature is that there is no macro hysteresis in US unemployment rates (Blanchard and Katz, 1992; Dao et al., 2017). That is, local unemployment rates return to their pre-recession levels within a couple of years following the recession's end. However, the story is more complicated when looking at participation and employment rates in local labor markets. There is a persistent decline in employment and labor force participation rates in local areas experiencing recessions. However, this decline stems from changes in local demographic composition, not from a decline in individual labor force attachment. Using state-level variation, Cajner et al. (2020) find that the demographically-adjusted labor force participation rate takes eight years to recover following a recession but eventually goes back to its pre-recession level. As Figure 4.1 shows, this result does not hold for the raw labor force participation rate, unadjusted

Figure 4.1: Labor Force Participation Recovery- Raw series and Demographic Adjustments



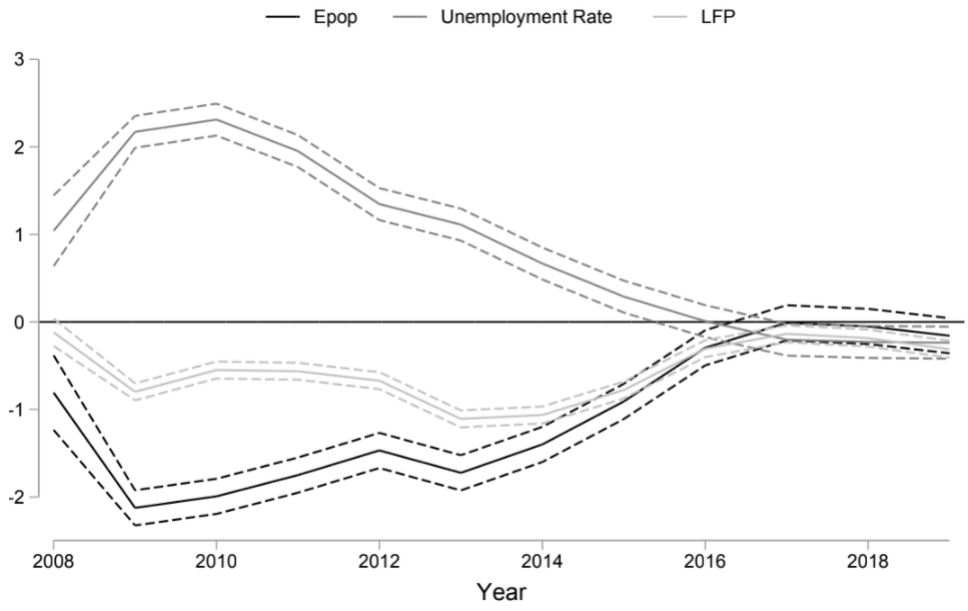
Note: Each line shows the estimated coefficients from Equation 1 using the specified adjusted, unadjusted, and fitted-value LFPR as the outcome. The band around the orange solid line shows a 95% confidence interval, based on standard errors clustered by state. Coefficients are normalized to show the effect of a temporary -1% shock to GSP growth in year 0. F-statistic: 149.1. Regressions control for state and year fixed effects and are weighted by population.

Source: Cajner et al. (2020)

for demographic shifts. Using a different methodology, Gonzalez (2020) finds a similar result for unemployment, labor force participation, and employment rates after the Great Recession (GR) using state-level variation, as shown in Figure 4.1. Cajner et al. (2020) show that states affected by a negative output shock experience a decrease in the population share of prime-age adults even 10 years after the shock. Since prime-age adults have the highest labor force participation rate among all age groups, this means that areas affected by a negative shock are left with a higher share of people with lower labor market attachment.

The main goal of this paper is to understand the process through which local labor markets are left with a persistently smaller share of prime-age individuals following a negative shock. Using Commuting Zones (CZ) to define labor markets, we study migration patterns by age in response to different growth rates of economic activity induced by changes in national sectoral

Figure 4.2: Recovery Path of Labor Market Indicators after the GR



Note: This graph shows the difference in labor market indicators between states severely affected by the GR and those mildly affected by the GR. States are classified in their corresponding group using the 2009 residuals from Blanchard and Katz (1992) VAR system. These indicators are adjusted for changes in states' demographic composition. Source: Gonzalez (2020)

gross domestic product (GDP) growth as a possible mechanism behind hysteresis. [1]

Reverse causality is the main challenge in identifying the migration response to an increase in GDP. Individuals' migration decisions are potentially affected by the state of the local economy, which in turn is potentially affected by migration decisions via changes in labor supply. In other words, if individuals are basing their migration decisions on the health of the labor market, a lower level of GDP growth could deter them from moving to a specific area, a reduction in *in-migration*, or lead them to leave, an increase in *out-migration*. If enough people make this decision, the labor supply of the destination could be reduced, which could negatively impact GDP growth.

To overcome this endogeneity challenge, we use an instrumental variable approach using a shift-share instrument based on sectoral GDP. The idea behind this instrument is to identify changes in local GDP growth stemming from national changes in industry-level demand. To the extent that the industrial composition of a CZ reflects structural characteristics of the local labor market, this strategy allows us to identify demand shocks.

Our findings suggest that the net migration —net migration = in-migration - out-migration— rate of prime-age adults increases when a CZ experiences positive GDP growth, while retirement-age individuals decrease their net migration in response to a negative GDP shock. When looking into the source of the net migration response, we find that the response of prime-age individuals comes mainly from an increase in **in-migration** to areas that experienced a positive shock, whereas the retirement age group experiences a reduction in its **out-migration** rate from areas that experience positive GDP shock.

[1] As the measure of local economic activity, we use CZ GDP. We built this measure using county-level data from the Bureau of Economic Analysis and the crosswalk of counties to 2000 delineation of CZ provided by the Economic Research Service of the U.S. Department of Agriculture.

Related Literature

Following the categorization proposed by Jia et al. (2022) in their recent review on US internal migration, this paper is related to literature that studies migration and labor market adjustments. This literature started with Blanchard and Katz (1992), a seminal paper that uses a VAR model on state-level relative employment growth, employment rates, and participation rates to characterize the labor market after a demand shock. They find that the unemployment rate recovered quickly for the period 1972-1990 and conclude that migration was the main equilibrating force after a local recession. Dao et al. (2017) in a follow-up paper, using an alternative identification strategy and additional data sources, including a direct measure of net migration, find that most of those who lose their jobs after a local recession end up in the unemployment pool for the first couple of years. Migration kicks in later and acts as an equilibrating force in the long run, so their results are consistent with the core results in Blanchard and Katz (1992). However, migration has lost power as an equilibrating force in recent decades. This result is in line with other studies that have shown that in recent decades there has been a decrease in the convergence of labor markets across the US (Amior and Manning, 2018; Austin et al., 2018; Ganong and Shoag, 2017) and that there is a negative secular trend in migration, particularly related to work-related moves (Molloy et al., 2011, 2017). Another recent contribution to this literature is Hershbein and Stuart (2021). They use the reduction in the employment rate in the first two years after a national recession to quantify its effect at the MSA level; for example, their measure of the impact of the GR is the reduction in the employment rate in 2007-2009. They find that more affected MSAs experienced permanent declines in their employment rate. Cadena and Kovak (2016) is one of the few papers that looks at heterogeneity in the migration response to local recessions

based on demographic characteristics. They find that, among low-skill workers, Mexican-born immigrants are more responsive to employment changes induced by the GR than native workers. This finding underscores the importance of paying close attention to the heterogeneous migration response of individuals in different demographic groups. We contribute to this literature by analyzing the migration response for different age groups and characterizing the aggregate impact of individuals' decisions on local labor markets. Finally, Cajner et al. (2020) using state-level data, find that there is a long-lived negative effect on local labor force participation rates after a recession. Still, on average, the demographically-adjusted labor force participation rate returns to its pre-recession level after eight years. The demographic adjustment is key to their findings. The authors find that the share of prime-age workers in areas more adversely affected by a recession decreases permanently and assert that this is likely the result of an increase in the out-migration of prime-age workers. On the structural side, Monras (2020) finds that in-migration is the relevant margin affecting population growth after a local economic shock and develops a general equilibrium model with multiple locations around that stylized fact.

Our contribution to the literature is threefold. First, we use the Consumer Credit Panel, a better-suited data set to understand the migratory response at a granular level. Second, we look directly at the response in the gross migration flows by age group and find that local recessions induce a reduction in the in-migration rate of prime-age adults and a reduction in the out-migration of retirement-age individuals. This result explains how labor markets affected by a recession are left with a lower participation rate via a higher share of retirement-age individuals. Third, we use Commuting Zone (CZ) level variation. CZs are geographical units closer to a labor market than states. Using CZs as our unit of analysis allows us to include and study within states across CZ migration. This is a relevant difference for two reasons. The gravitational relation between local

labor markets in regards to migration documented in the US since Zipf (1946) implies that the distance is a strong predictor of moving flows between areas and labor markets within the same state are likely to be closer to each other than labor markets in different states. Using data from the American Community Survey (ACS), we find that 40% of cross-CZ migration occurs within states. Additionally, a recent paper by Wilson (2022) finds that there is a discontinuity in the migration probability around state borders. This means that by missing the intra-state migration, state-level analyses ignore a relevant proportion of migration decisions.

In the next section, we describe the empirical strategy, including some of its weaknesses and possible avenues for improvement. In section 3 we describe the data, including how we built the shift-share instrument at the CZ level. Section 4 includes the results using weighted averages of national 2-digit-level GDP growth rates to instrument the CZ GDP growth rate. In section 5, we link the empirical results on migration by age group to the demographic change documented in previous literature. Finally, in section 6, we conclude.

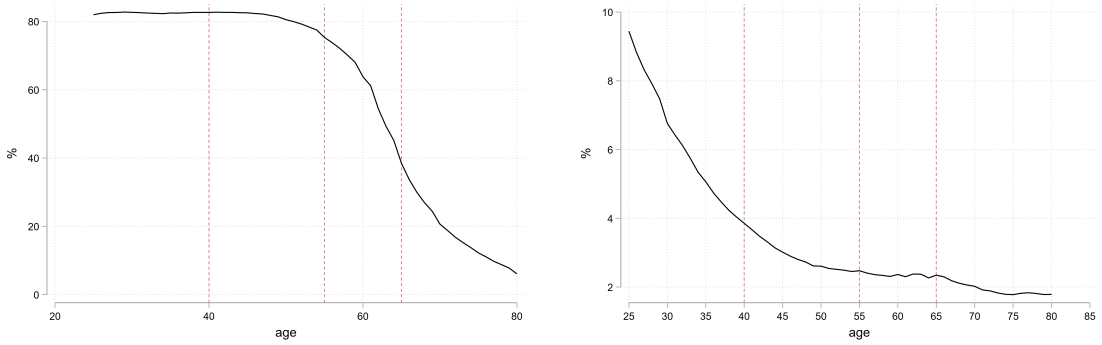
4.2 Labor Force Participation and Migration Over the Life Cycle

In this section, we document the average migration and labor participation rates by age using data from the American Community Survey (ACS) between 2005-2019. The labor force participation rate is the share of the population that is either working or looking for a job. The migration rate is the number of people that migrate across CZs as a percentage of the population of that age group in the destination CZ. It is a well-known fact that the likelihood of migration decreases with age. Early papers like Sjaastad (1962) and Schultz (1978) talk about a consensus in the literature on this issue. More time to take advantage of differential economic conditions, in-

creasing opportunity, direct and psychic cost, and cohort selectivity effects have been mentioned as potential explanations for this established fact. More recently, Kaplan and Schulhofer-Wohl (2017) documented that it continues to be true that the likelihood of migration decreases with age, with younger workers being more likely to migrate than older workers.

If a relevant reason for moving is the state of the economy, the fact that younger workers migrate more implies that young people are more sensitive to the local labor market conditions than older people. This difference alone could explain why areas affected by a negative economic shock are left with a larger proportion of individuals with a lower labor market attachment.

Figure 4.3: Participation and Migration Profiles by Age



Source: Authors' calculations using ACS microdata from 2005-2019. The migration rate refers to the annual cross-CZ migration rate. A person is classified as a migrant if she moved to a PUMA with no overlapping CZs as the PUMA of origin. CZs are defined using 2000 delineations as described in Appendix C.

The graph on the left shows that labor market attachment remains high and stable for the prime age population (25-54). After age 55, labor force participation declines steeply. After age 65, the labor force participation decrease decelerates, but participation continues declining until age 80, the last age reported in the graph. On the right-hand side figure, it is evident that migration decreases with age. Additionally, we can see that the slope of the migration profile is not constant. The migration profile has a convex shape. It decreases rapidly between the age of 25 and the mid-40s, and then it levels out above 2 percent up to 65 years of age. After age 65,

migration rates decrease and stay constant below 2 percent up to age 80. These patterns indicate that a decrease in the net migration of prime-age individuals would likely be associated with a decrease in the overall labor force participation and employment rates.

4.3 Empirical Specification

4.3.1 One-sided regressions

Identifying the migration response after a local economic shock is not trivial. The main identification threat is reverse causality. This challenge stems from the fact that people may decide to move to a location based on the economy's health, and at the same time, the economy's health might be affected by migration decisions.

We compare the sensitivity of the migration rates to changes in destination GDP growth across three age groups $A = \{25-39,\ 40-64,\ 65+\}$. The coefficients of interest are β_k^A, the change in the migration rate induced by a change in GDP. . More specifically, we allow for the effect to take time by specifying the structural equations of the form

$$MR_{c,t+k}^A - MR_{c,t-1}^A = \beta_k^A \Delta g_{c,t} + \mu_c^A + \mu_t^A + \xi_{c,t}^A \tag{4.1}$$

where MR is either the net-migration, the in-migration or the out-migration rate, $A = \{25-39,\ 40-64, 65+\}$, $\Delta\hat{g}_{c,t}$ is the predicted GDP growth based on the industrial structure of CZ c in time t, and μ_c and μ_t are CZ and time fixed effects respectively. Our goal is to characterize the migration response across age groups to understand the process through which areas affected by a negative labor demand shock are left with a higher share of individuals with lower labor

market attachment. Based on this specification, we build an impulse response function with the coefficients of β_k^A over different values of k. β_k is interpreted as the effect of a change in the GDP growth rate over the k periods after the demand shock.

The bias of estimating equation 4.1 by ordinary least squares (OLS) depends on the right-hand side variable. The sign of the bias for the net and gross migration is the same. If GDP growth and in-migration are positively related in both directions, meaning that as GDP growth increases, the in-migration rate increases, and as the in-migration rate increases, the GDP growth increases, the sign on the bias of the OLS estimator is positive. The structural relationship between out-migration and GDP growth is likely to be in the opposite direction, meaning that as GDP grows, out-migration is likely to decline. As out-migration increases, GDP is likely to shrink. In this case, the bias of the OLS estimator is negative. [2]

To overcome this challenge, we use the change in local GDP growth induced by changes in local demand. We do this by instrumenting the GDP growth rate with the GDP growth predicted by the industrial structure of the CZ. More precisely, we define the predicted CZ-level GDP growth as the sum of the shares of local GDP in each sector in 2004 multiplied by the national growth rate of that sector in each subsequent year.

$$\Delta g_{c,t}^{*} = \sum_{s} \omega_{c,s}^{2004} \Delta g_{s,t} \tag{4.2}$$

where $\omega_{c,s}^{2004}$ is the share of sector s in nominal GDP in CZ c in 2004, and Δg_t is the national real GDP growth of sector s in period t. The intuition behind this instrument is that it captures the effect on local growth of changes in the national demand for the goods that are produced

[2] For details on the sign of the bias in a system with reverse causality, see Basu (2015)

intensively in the CZ. A decrease in the local GDP growth rate coming from a decrease in demand signals a less favorable environment for workers. This is a shift-share instrument, also known as a Bartik instrument. The literature has traditionally used this type of instrument using employment, not GDP. We use GDP for two reasons: 1) The dynamic behavior of GDP displays less persistence than that of the employment rate. We want to characterize the dynamics of the migration response, so this feature makes GDP growth a better alternative 2) The relationship between employment and migration is mechanical, making the reverse causality concern more salient. The first-stage regression is thus:

$$\Delta g_{C,t} = \alpha \Delta g^*_{C,t} + \lambda_t + \lambda_C + \nu_{C,t} \tag{4.3}$$

where λ_t and λ_C are fixed effects for time and CZs, respectively. For the instrument to be valid, it must be relevant and exogenous. The argument for the instrument's relevance is that a decrease in the national demand for the goods produced intensively by the CZ will translate into a decrease in local demand for labor and, therefore, a decrease in GDP growth. As shown in Table 4.1, GDP growth is strongly related to predicted GDP growth. As pointed out by Goldsmith-Pinkham et al. (2020), by using a shift-share instrument, we are also implicitly either assuming the exogeneity of the shares or the exogeneity of the shocks. In this case, we rely on the assumption of the shares being exogenous. Concretely the identifying assumption is that differential exposure across CZ to common national-level shocks leads to differential changes in the migration rates. In other words, we take the shares as reflecting long-term structural characteristics of the CZ that make them more or less susceptible to changes in the national level industry labor demand. To make this assumption more sensible, we use the industry shares of a period before the data we use in

Table 4.1: First Stage

VARIABLES	(1) $\Delta g_{c,t}$
$\Delta g^*_{c,t}$	1.235***
	(0.0274)
Observations	6,900
R-squared	0.410

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Includes year-fixed effects and CZ-fixed effects. Annual data from 2005-2019.

our estimates.

4.3.2 Origin-Destination Specification

One of the advantages of having access to a large data set is that we can look at the origin-destination migration flows. When someone is deciding if they will move and where they will move to, they consider the state of their current location and the state of potential destinations. In other words, the migration decision is likely based on the relative state of the origin and each potential destination. By looking at the one-sided regressions described in the previous section, we are assuming that, in the case of out-migration (in-migration), the state of the potential destinations (origin) is constant. Since people always have the option to stay without paying a moving cost, not considering the two-sided dynamic of the migration decision is a relevant omission. Using data from Brazil, Borusyak et al. (2022) found that results based on a one-sided standard regression provide a misleading picture of the determinants of migration elasticities. We move to a specification and data structure that allows us to characterize the in-migration response of each age group to local economic cycles, keeping the characteristics of the origin fixed. We

implement this idea by including an origin $\times$ time fixed effect.

$$IMR^{A}_{o,d,t+k} - IMR^{A}_{o,d,t-1} = \beta^{A}_{k}\Delta g_{d,t} + \gamma^{A}_{o,t} + \gamma^{A}_{o,d} + \epsilon^{A}_{o,d,t} \tag{4.4}$$

Symmetrically, when we estimate the effects of local economic cycles on out-migration, we include a destination$\times$ time fixed effect.

$$OMR^{A}_{o,d,t+k} - OMR^{A}_{o,d,t-1} = \beta^{A}_{k}\Delta g_{o,t} + \tau^{A}_{d,t} + \tau^{A}_{o,d} + \upsilon^{A}_{o,d,t} \tag{4.5}$$

This specification allows us to overcome the shortcoming highlighted by Borusyak et al. (2022) by directly controlling for the dynamics of the origin/destination. Since the endogeneity concern operates in the same way in the origin-destination specification, we also instrument the local growth rate with the predicted growth rate using the industrial structure of the CZ in 2004 and national growth rates at the industry level.

4.4 Data description

We use data from the New York Fed Equifax Consumer Credit Panel (CCP) to measure migration rates by age groups at the CZ level, county-level population estimates from the Census Bureau, and county-by-sector GDP data from the Bureau of Economic Analysis (BEA) to build the GDP growth and the instrument. For the regression estimates, we used data from 2005 to 2019, and for the sectoral shares of GDP, we used data from 2004. Below is an overview of each data source and the variables we use.

4.4.1 CCP

The CCP is built using individuals' credit reports. It includes anonymized information on age, census block of residence, and financial indicators. The CCP is a nationally representative 5% sample of adults in the US with Equifax credit report data (core sample) and those living in their address. The longitudinal dataset has information starting in 1999 and information up to the second quarter of 2022. As noted by DeWaard et al. (2019), the CCP allows detailed cross-sectional analysis of migration within the US that no public dataset provides, while being consistent with the ACS, the most common data source for migration data in the US. For this project, there are two main advantages of working with the CCP rather than the ACS. First, the CCP contains detailed information on the origin and destination of migrants at the census block level. In contrast, the ACS only reports the geographic data at the PUMA (Public Use Microdata Areas) or MIGPUMA (Migration Public Use Microdata Areas) level, sometimes pooling more than one county together. This means that we can directly measure cross-CZ migration flows. Second, the CCP's sample size is more than three times larger than the ACS. This point is very relevant for the empirical analysis of this paper, given that we require a sample large enough to be representative of age groups within small geographic units.

The main limitation of the CCP is that it is a sample of US residents with a social security number and a credit history. According to Brevoort et al. (2016), 11% of American adults are credit invisible or lack a credit report. The probability of being credit invisible is higher for young, elderly, minority, and low-income consumers. We do not consider this to be a severe drawback since our interest is not focused on financially disadvantaged people. However, it is important to consider this when thinking about the results' external validity. We define migrants

as those whose recorded addresses are in a different CZ from the year before. This means we are comparing the reported CZ in period t with the reported CZ in period t-1.[3] We define the in-migration rate for each age group in a particular CZ as the total number of people in that age group who report moving into that CZ in the last year, divided by the number of people in that age group living in the destination CZ in that year. Similarly, the out-migration rate is the number of people in a particular age group who reported moving out of the origin CZ in the last year, divided by the number of people in that age group in the origin CZ that year.

4.4.2 BEA

Starting in December of 2020, the BEA publishes annual sectoral GDP by county. The data starts in 2001 and is provided in current dollars and in chained dollars of 2012. To build the shares needed for the instrument, we used nominal GDP in 2004. We aggregate the sector/county level GDP to sector/CZ level by using the county-to-CZ crosswalk provided by the ERS and described in Appendix C. The BEA suppresses some cells to avoid disclosing sensitive information, with suppression more prevalent in small counties. To deal with this issue, we use aggregated sectors to ensure the shares add up to one. This process implies that the shares reflect a collection of sectors for some counties. We calculate the equivalent national-level growth rate for all the aggregated sectors necessary to build the CZ predicted GDP growth.

[3] Equifax has a proprietary algorithm to define the place of residence. We do not have access to the algorithm. Still, from the manuals, we know that address identification relies on several pieces of information, including billing addresses and, if the individual has a mortgage, the address of the mortgage.

4.4.3 Descriptive Statistics

Tables 4.2 and 4.3 contain the summary statistics of the relevant variables. The first thing to note is that migration rates decrease with age. This is a well-documented fact in line with Figure 4.3.

Table 4.2: Summary Statistics: One-Sided Regressions

	mean	sd	p25	p50	p75	min	max
IMR 16+	3.4	1.5	2.3	3.1	4.1	0.0	20.5
IMR 25-39	5.7	2.1	4.2	5.4	6.8	0.0	50.0
IMR 40-64	2.6	1.4	1.6	2.3	3.2	0.0	25.9
IMR 65-80	2.2	1.3	1.3	1.8	2.7	0.0	26.7
OMR 16+	3.4	1.3	2.5	3.1	3.9	0.0	263.0
OMR 25-39	5.8	2.0	4.4	5.4	6.6	0.0	262.5
OMR 40-64	2.6	1.1	1.9	2.4	3.0	0.0	314.0
OMR 65-80	2.2	1.1	1.6	2.0	2.6	0.0	251.9
Observations	10605						

Source: New York Fed Equifax Consumer Credit Panel, BEA, authors' calculations.

Table 4.3: Summary Statistics: Origin-Destination Regressions

	mean	sd	p25	p50	p75	min	max
IMR 25-39	0.010	0.100	0.000	0.000	0.000	0.000	33.333
IMR 40-64	0.005	0.049	0.000	0.000	0.000	0.000	15.385
IMR 65-80	0.003	0.045	0.000	0.000	0.000	0.000	22.727
IMR 16+	0.006	0.046	0.000	0.000	0.000	0.000	16.190
OMR 25-39	0.010	0.104	0.000	0.000	0.000	0.000	33.333
OMR 40-64	0.004	0.044	0.000	0.000	0.000	0.000	9.091
OMR 65-80	0.003	0.061	0.000	0.000	0.000	0.000	100.000
OMR 16+	0.006	0.047	0.000	0.000	0.000	0.000	5.869
Observations	7151850						

Source: New York Fed Equifax Consumer Credit Panel, BEA, authors' calculations.

Additionally, notice that the average migration rates in the origin-destination data structure are two orders of magnitude smaller than in the one-sided data. This comes from the fact most of

the O-D pairs have no migration flows while most of the CZ have migration flows. In other words, there are always some people coming or leaving a CZ, but it is not the case that we see people moving to all CZ but to a very reduced set of CZ. This is a fact documented in Sprung-Keyser et al. (2022), where the authors find that the radius of economic opportunity for Americans is very narrow. In other words, people from a particular CZ tend to move to a small set of CZ.

4.5 Results

This section includes the results for the one-sided regressions and the origin-destination regressions. The main message is that the results are robust to the specification used. We find that after a local labor market experiences an increase in GDP fueled by national demand, there is a migration response that changes in magnitude and, in some cases, in direction for the different age groups we analyze. The differential migration response by age group is consistent with places that experience GDP shocks experiencing persistent effects via the composition of their demographic composition. The results show that younger people have a larger migration response than older people; this is consistent with the fact that younger people generally have a higher migration probability and with the fact that retirement age with a lower reliance on labor market income responds less to labor market events. Young (26-39) and middle-aged (40-64) people have a positive in-migration response to positive output shocks in both specifications. Even though smaller, the out-migration response of retirement-age people works in the opposite direction. In other words, retirement-age individuals increase their out-migration rate after a positive shock.

4.5.1 One-Sided Specification

Figure 4.4 summarizes the results of the local projections on the different migration rates by age group over a ten-year horizon. We include the response of the migration rate to an increase in GDP— a local economic expansion—induced by an increase in national demand. From top to bottom, the results are ordered by age group, from younger to older, and the final panel of the figure includes the effect for all age groups.

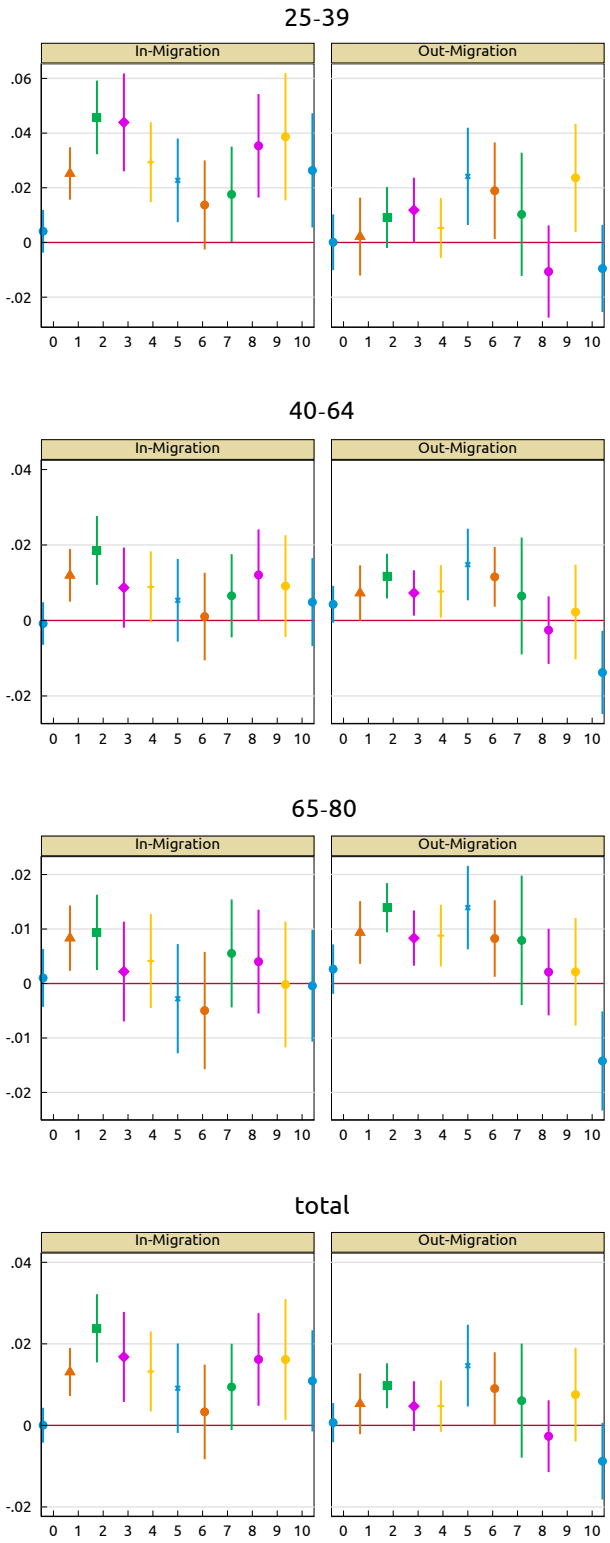
According to the one-sided results, the in-migration rate of all age groups increases with local economic expansion. However, as can be seen by comparing the scale of the Y-axis of the figures, the response is larger for people in the younger age groups. In other words, younger workers are more attracted to areas experiencing an economic expansion, while the retirement age individuals respond at a lower scale. On the other hand, we find that out-migration significantly increases after an economic expansion, only for retirement-age individuals. A muted in-migration and positive out-migration responses indicate a negative net migration response for retirement-age individuals.

If the results are symmetric, this would translate into a decrease in the net migration of young and middle-aged individuals and an increase in the net migration of retirement-age individuals in areas that experience a local economic contraction.

4.5.2 Origin-Destination Specification

As Figure 4.5 shows, the results using the origin-destination specification are similar to the one-sided regression results. Young and middle age people increase their in-migration to areas that experience an economic expansion. The main difference between the one-sided and

Figure 4.4: One-sided regression results



Source: New York Fed Equifax Consumer Credit Panel, BEA, authors' calculations.Each coefficient comes from a different regression of the form: $MR^A_{c,t+k} - MR^A_{c,t-1} = \beta^A_k \Delta g_{c,t} + \mu^A_c + \mu^A_t + \xi^A_{c,t}$ using as an instrument for the GDP growth the predicted GDP growth using a Bartik instrument with industry detail at the two digits level. We use the total population to weigh the results.

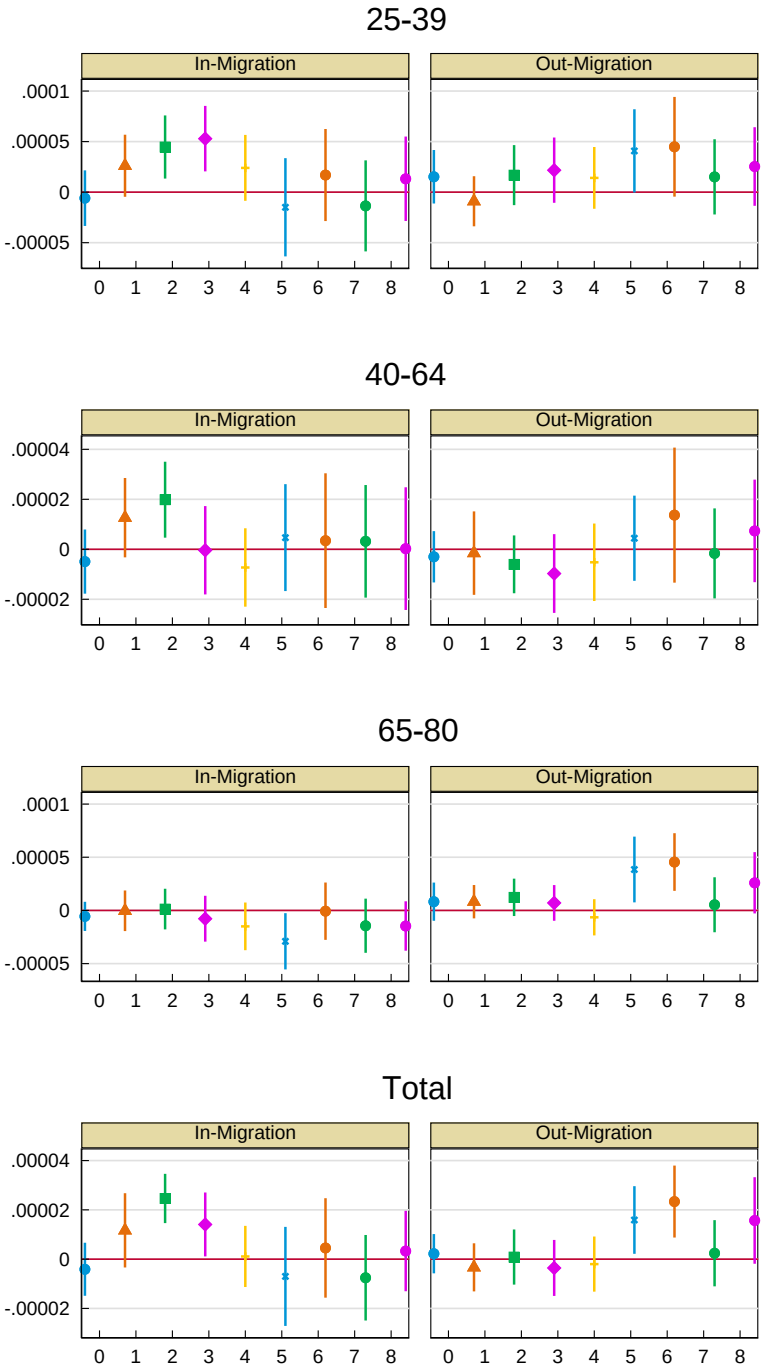
the origin-destination results is that according to the origin-destination results, retirement-age individuals do not increase their in-migration to expanding labor markets. This could be because there are pushing factors that lead retirement-age individuals to leave their respective origins. After those push factors enter in effect and the retirement age individuals are choosing where to move, areas that are experiencing an economic expansion could be appealing because of increasing amenities or because they want to be closer to younger relatives that are pulled to those areas. This effect goes away when we account for the conditions in origin by including the origin$\times$ year fixed effect.

The overall results after we account for the state of the origin (destination) when analyzing in-migration (out-migration) indicated responses in the migration elasticities that are consistent with the change in the demographic composition of areas that experience an unexpected change in their labor demand. This is a novel and interesting result. Most of the migration results in the literature are one-sided, and when we expand the dataset and control for the two-sided nature of the migration decision our main results hold up.

4.6 Conclusion

Previous literature has shown that local labor markets that experience economic shocks have persistent changes in their demographic composition. Local areas that experience a negative economic shock are left with a higher share of the population above 65. In this paper, we offer novel evidence of the process through which this demographic change takes place. Using detailed data on the universe of US residents with a credit history from the CCP, we show that young adults are more responsive to local economic developments than middle-aged adults and

Figure 4.5: Origin-destination regression results



Source: New York Fed Equifax Consumer Credit Panel, BEA, authors' calculations. Origin-Destination regressions. We instrument destination (origin) GDP with the predicted destination (origin) GDP and control by origin (destination)-by-year FE. The predicted GDP growth is a Bartik instrument with industry detail at the two digits level using the industry weights of 2004 and the national growth rates. All specifications include origin-destination-pair FE.

retirement-age adults. Additionally, after a positive GDP shock, we find that the relevant adjustment margin for young adults is in-migration. Young people move more into CZs that experience a positive demand shock. In comparison, the most relevant margin of adjustment for retirement-age individuals is out-migration. Retirement age people increase the out-migration from places that experience a positive increase in demand. In addition to implementing one-sided regressions — in (out)-migration regressions with the destination (origin) GDP as the main variable of interest—we implement a novel exercise with an origin-destination data structure that allows us to consider the state of the CZ of origin and destination simultaneously. Our results are robust to this identification strategy.

Our results are consistent with younger individuals having a stronger incentive to move where the labor market is performing better. While retirement-age individuals– who are less likely to be active participants in the labor market– place more weight on the price of non-tradable such as housing when making moving decisions.

These results are in line with the broader literature that studies the lack of convergence in labor markets in the US Amior and Manning (2018); Austin et al. (2018); Ganong and Shoag (2017) and explain a mechanism behind local hysteresis- the persistent effect of transitory local shocks. Notice that to quantify the proportion of the reduction in labor force participation explained by age-differential migration, we would need to perform an additional empirical exercise. In future research, we expect to estimate the direct effect of a GDP shock on CZ labor force participation to quantify the relevance of the mechanism proposed in this paper.

We identify three open questions. First, it would be relevant to identify whether or not the results we find are symmetric to positive and negative shocks. This is particularly relevant because migrating is an expensive process, so it makes sense that the elasticities of migration differ

between good and bad times. Second, given that migration in the US occurs mainly between a reduced subset of CZ, as documented by Sprung-Keyser et al. (2022), we would like to quantify the difference in the migration elasticities between highly connected CZs and the rest of the country. Finally, it would be interesting how the elasticity changes across other individual characteristics such as ethnicity, education, or income.

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 `ihs(.)` is defined at zero. However, as pointed out by Aihounton and Henningsen (2021), the units of the earnings variable matter when using `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 $\lim_{x \to \infty} \texttt{ihs}(x) = \ln(x) + \ln(2)$.

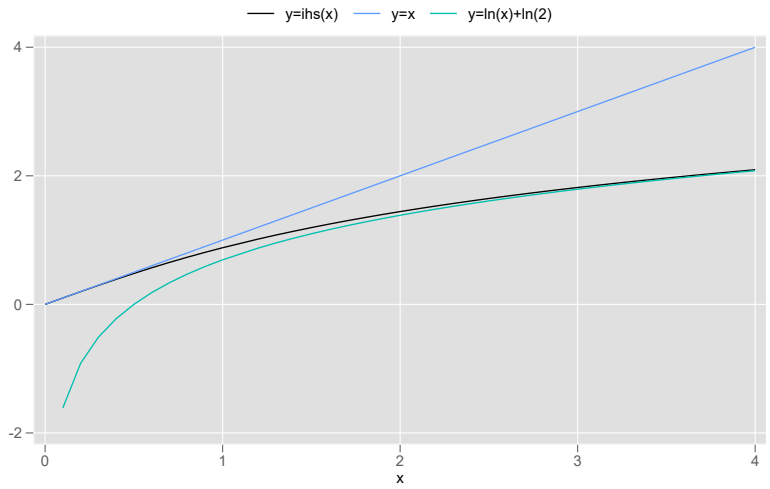


Figure A.1: Inverse Hyperbolic Sine Transformation and Units

Given this scale sensitivity of the $\texttt{ihs}$ transformation, Aihounton and Henningsen (2021) propose a procedure to adequately choose the scale, something 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 the following semi-elasticity:

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

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 estimates.

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 2.5) where we vary the scale of the dependent variable.

A.1.1 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 2.5 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 2.5 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 these two variables, we calculate the average earnings ($\overline{W}_i^T$) and the average earnings while employed ($\overline{W}_i^E$) as follows

$$\overline{W}_i^T = \frac{W_i}{60}, \tag{A.2}$$

$$\overline{W}_i^E = \frac{W_i}{N_i}, \tag{A.3}$$

where the main difference in the equations above is that equation (A.2) considers periods of non-employment to estimate the average, while equation (A.3) 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 2.5. The split between periods of employment and non-employment also differs in this case, where now periods of non-employment only account 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 earnings equivalent to that of the 1st percentile. Column (1) estimates equation 2.1 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 2.2. Column (4) follows the specification in equation 2.2 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)		✓	✓	✓

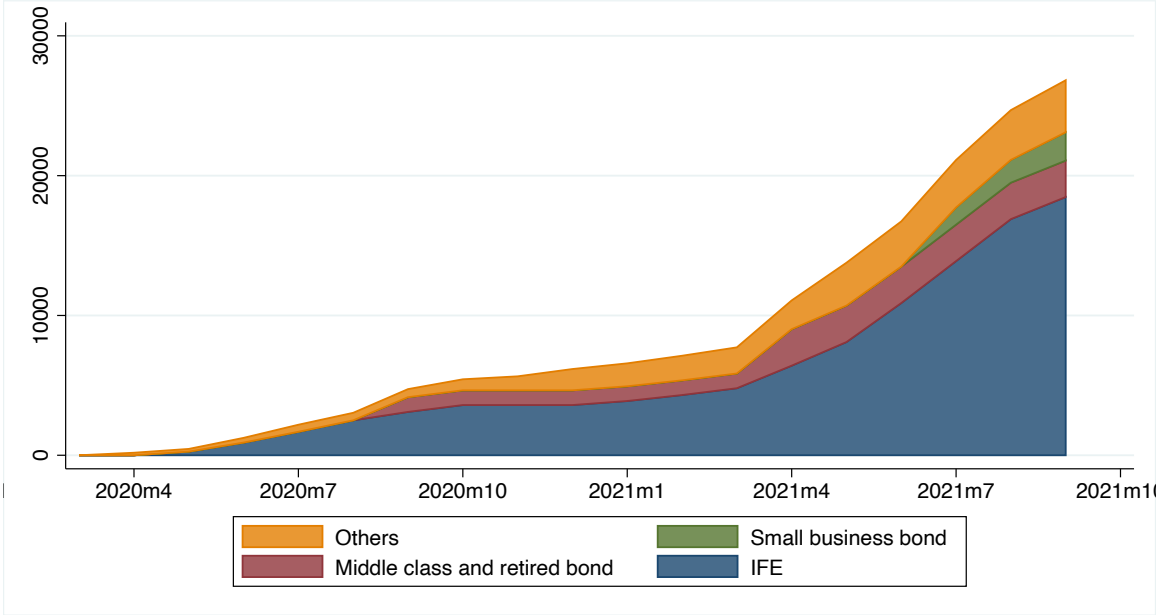
Note: This table presents results where the dependent variable is the logarithm of average earnings. Columns (1) to (3) consider average earnings including periods of non-employment ($\overline{W}_i^T$). Column (4) only considers periods of positive earnings ($\overline{W}_i^E$).

Appendix B: Essential Businesses during the Coronavirus Pandemic

The Government declared as *Essential Services* those businesses that perform critical functions and are intended to provide vital services, exercise political authority, maintain the safety of citizens, and maintain an economic base during an emergency. The categories considered were:

1. Health: Health facilities and any business that provide good or services to health facilities, including hotels for quarantines and nursing homes
2. Emergency services
3. Public utility services: including provision of water and energy, airports, gas stations, banks, telecommunications services, data centers, delivery companies and repair service for infrastructure among others
4. Food and essential trade: Supermarkets, corner stores, bakeries and agricultural companies
5. Transportation
6. Security: companies providing security services to buildings and other properties
7. Communications: TV channels, radio stations, newspapers, and online journalism
8. Education: in connection with ethical shifts and IT support for educational institutions

Figure B.1: Evolution of cumulative amount of direct transfers from the Government (MMUS $)



9. Others: hotels with guests or with confirmed reservations, activities that by their nature cannot be stopped and whose interruption generates an alteration to the operation of the country duly determined by authorities

Figure B.2: Summary of main support policies for families and businesses during the pandemic

program name	Description	Target audiences	Requirements	Number of beneficiaries	application date	Amount delivered
Law 21242	Interest-free loans and a subsidy for independent workers, depending on the income bracket.	Independent workers who receive income taxed in accordance with article 42 No. 2 of the Income Tax Law	-Having delivered a service rendered payment for at least three months in the last year (between April 2019 and March 2020), or six months in the last two years, and - drop in income by at least 30% in the last year	-	May 2020 – November 2020	-
Employment subsidies: contract line	It seeks to encourage the hiring of new workers, and their maintenance in their jobs.	Companies and potential workers	Companies and potential workers must meet several requirements, for details see Link	As of 09.27.2021: 842,891 people Source: link	Started in September 2020	Budget: US$ 2,000 million (distributed with line returns and protects) Source: Link
Employment subsidies: line returns	It is a benefit to the employer for each worker whose contract was suspended by LPE and is reinstated to work.	Companies directly and workers suspended indirectly	Suspended companies and workers must meet certain requirements, for details see Link	As of 09.27.2021: 150,219 people	Started in September 2020	Budget: US$ 2,000 million (distributed with line returns) Source: Link
Employment subsidies: line protects	In a subsidy of $200,000 for each child under 2 years of age that the beneficiary worker has in their care. It lasts 3 months, with possible extension of up to 6 months.	Employees or workers, dependent or independent, who take care of children under two years of age, and who do not have the right to a nursery guaranteed by their employer.	Specific requirements: Link	As of 09.27.2021: 38,507 people	Started in September 2020	Budget: US$ 2,000 million (distributed with line returns and protects) Source: Link
Labor IFE	It corresponds to a monthly payment that is delivered to all people who start a new employment contract	Unemployed people	-Have a new employment contract. -Monthly income less than or equal to 3 Minimum Wage. -Having been unemployed during the month prior to the start of the new contract. Detail requirements: Link	-	Until March 2022	-

Table B.1: Payments from Solidarity Unemployment Fund

Month	% Taxable earnings	Max. cap ($CLP)	Min. cap ($CLP)
A. Workers with fixed-term contract			
1	70%	625.956	225.000
2	55%	513.038	225.000
3	45%	419.757	225.000
4	40%	373.228	200.000
5	35%	-26.476	175.000
B. Workers with open-ended contract			
1	50%	466.398	225.000
2	40%	373.118	200.000
3	30%	326.478	175.000

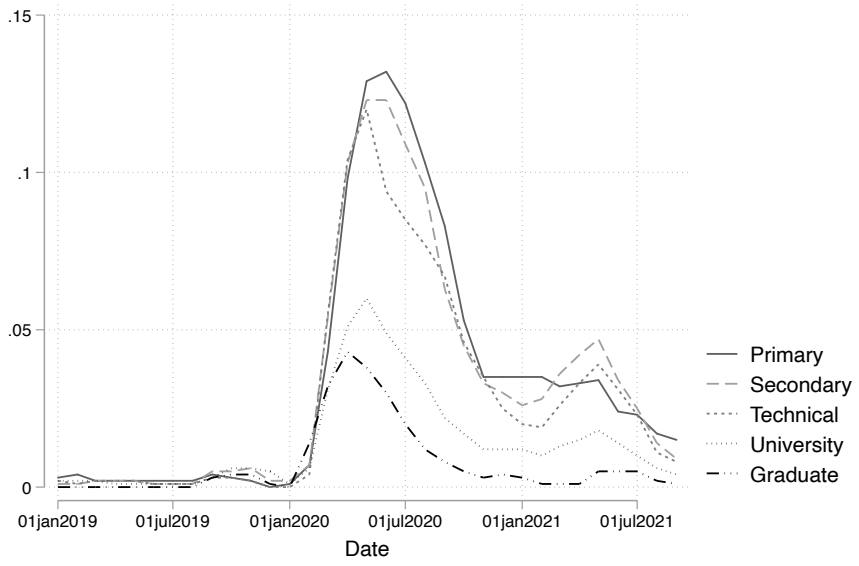
Note: Source: Law 21.227 $CLP stands for Chilean Pesos

Table B.2: New UI payments scheme, since September 2020

	Individual Account	Solidarity Fund		
Month	% Taxable Earnings	% Taxable Earnings	Max. cap	Min. cap
1	70%	70%	652.956	225.000
2	55%	55%	513.038	225.000
3	55%	55%	513.038	225.000
4	55%	55%	513.038	225.000
5	55%	45%	419.757	225.000
6+	50%	-	-	-

Note: Source: Law 21.263

Figure B.3: Share of workers on temporary layoff, receiving salary or earnings, in the formal sector, by educational level*



Source: National Institute of Statistics of Chile, INE.

Note: * We refer as temporary layoffs workers to those who are currently furloughed, or unemployed but were given a date to return. We consider only those temporary layoff workers who were receiving salary or earnings, as this proxy for being furloughed under the EPL. University is equivalent to an undergraduate degree in the US. Graduate means the worker has a postgraduate degree

B.1 Probability of using the EPL

Table B.3: Determinants of the probability of using the EPL, at the firm level

Number of Firms	368,103	
Average probability of EPL usage	0.30	
	Predictive Mg	**Delta-method Std. Err.**
Accommodation and Food	0.60	0.00
Administrative Activities	0.30	0.00
Agriculture	0.08	0.00
Arts and Entertainment	0.50	0.01
Construction	0.34	0.00
Education	0.25	0.00
Electricity Supply	0.15	0.01
Finance and Insurance	0.18	0.00
Health and Social Services	0.37	0.00
Households as Employers	0.18	0.01
Information and Communications	0.23	0.00
Manufacturing	0.35	0.00
Mining	0.18	0.01
Organizations	0.11	0.02
Other Services	0.20	0.00
Professional Scientific and Technical	0.28	0.00
Public Administration	0.04	0.01
Real Estate	0.27	0.00
Trade	0.36	0.00
Transportation and Warehousing	0.25	0.00
Water Supply	0.09	0.01
Age Category		
less than a year	0.25	0.00
1-2 years	0.29	0.00
2-3 years	0.29	0.00
3+ years	0.32	0.00
Size		
Micro 1-9	0.28	0.00
Small 10-49	0.40	0.00
Medium 50-199	0.38	0.00
Large 200+	0.39	0.01
Selected Region		
Atacama	0.18	0.01
Valparaíso	0.26	0.00
Metropolitana	0.35	0.00
Tarapacá	0.38	0.01

Note: Notes: The coefficients come from a logit model. The outcome variable is a binary indicator that takes the value of 1 if the firm furloughed at least one worker between March and September 2020. Due to the large number of independent variables, we only report the coefficients for selected regions (those with the lowest and higher coefficients).

B.2 Additional differences in differences samples and specifications

Table B.4: Survival in September of April Jobs: Alternative Specifications

	(1) No controls	(2) Sector FE	(3) Firm FE	(4) Firm FE+ Controls
Eligible=1	0.0880***	0.107***	0.0571***	0.0551***
	(202.53)	(248.90)	(131.74)	(127.15)
Post=1	0.0101***	0.0183***	-0.0194***	-0.0206***
	(12.54)	(23.28)	(-24.05)	(-25.51)
Eligible=1 × Post=1	0.0130***	0.00905***	0.0219***	0.0224***
	(11.04)	(7.88)	(19.32)	(19.82)
N	5630224	5623236	5479953	5479953

t statistics in parentheses

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Note: Note: The table contains the coefficients of equation 3.1 for different samples. We include only jobs with less than 5 months of tenure. The dependent variable is a dummy equal to one if a job that existed in April survives up to September. We define the survival dummy as one if the same combination of firm and worker is recorded in the unemployment insurance records in September, October, or November. All Samples exclude quits. The controls we include are age and gender. We include data from 2015 to 2020. The eligible variable is equal to one if the job had three months of tenure or if the worker had at least six contributions to the UI system in the last 12 months. 60% of jobs with a tenure of two months are eligible.

Table B.5: Survival in September of April Jobs: Including Quits

	(1) All sectors	(2) Trade	(3) Manufacturing	(4) Acomm. and Food	(5) Arts and Entr.
Eligible=1	0.100***	0.116***	0.118***	0.105***	0.0757***
	(259.69)	(107.77)	(87.44)	(63.22)	(14.77)
Post=1	0.0130***	0.00990***	0.0360***	-0.126***	-0.0714***
	(18.43)	(4.58)	(13.83)	(-32.38)	(-6.48)
Eligible=1 × Post=1	0.00382***	-0.0148***	-0.0220***	0.0966***	0.00483
	(3.72)	(-4.79)	(-5.97)	(18.09)	(0.32)
N	7961031	1028505	642383	478940	42751

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Note: The table contains the coefficients of equation 3.1 for different samples. We include only jobs with less than 5 months of tenure and firms that furloughed at least one worker in April. The dependent variable is a dummy variable equal to one if a job that existed in April survives up to September. We define the survival dummy as one if the same combination of firm and worker is recorded in the unemployment insurance records in September, October, or November. All columns include firm fixed effects and control for age and gender. We include data from 2015 to 2020. The eligible variable is equal to one if the job had three months of tenure or if the worker had at least six contributions to the UI system in the last 12 months. 60% of jobs with a tenure of two months are eligible.

Appendix C: Commuting Zones as Unit of Analysis

Following the recommendations in Molloy et al. (2011) we use CZ as the geographic unit of analysis. CZs are geographic units of analysis intended to reflect more closely the local economy, the area where people live and work. More specifically, CZ are defined using commuting flows from the 2000 census. The strength of the commuting ties between two counties is defined as

$$T_{ij} = \frac{c_{ij} + c_{ji}}{min(w_i, w_j)}$$

where c_{ij} is the number of commuters from i to j and w_i is the total number of workers in i. Then a clustering algorithm is applied for the average linkage that starts by grouping the county pair with largest value of T_ij and subsequently forms clusters of interrelated counties. The final set of CZ is defined such that the average value of T_{ij} is above 0.02. Using this methodology in 2000 the research sector of the department of agriculture defined 709 CZs of which 690 are in the continental US. In contrast to MSAs, another geographic unit defined using commuting patters, CZs do not need to be around a metropolitan area. This means that CZs include rural areas as well and hence cover all the US. Using CZ in contrast to using state-level data to identify migration allows me to capture migration within states across CZ. Within-state migration is a large share of CZ migration, representing, on average, between 2005-2018 40% of CZ migration.

C.1 Commuting Zones in the American Community Survey

This appendix is relevant to the statistics included in the motivation of the paper. The in-migration and out-migration rates used in the regressions come from the CCP so we are able to clearly identify the county of origin/destination and hence do not have to deal with this issue.

Using the information on PUMA (Public Use Microdata Area) delineations and population shares from the CENSUS retrieved from the Missouri Census Data Center we built the PUMA-CZ crosswalk needed to calculate the cross-CZ migration rates. In cases where the county-group/PUMA crosses CZ boundaries, the crosswalks assign respondents to CZs in proportion to the share of the county-group/PUMA population that falls within each CZ, using county-level population counts from the Census Bureau to generate the shares. In other words, a single PUMA can be assigned to multiple CZs. An additional complication arises from the fact that for migrants, the location of origin is reported at the MIGPUMA (Migration Public Use Microdata Area) level. MIGPUMAs are groups of one or more PUMAs. When Counties are split into various PUMAS, MIGPUMAS group those PUMAS together. To build the CZ-level variables, we use two strategies:

1. A person is classified as a CZ migrant if she moves from a MIGPUMA with no overlapping CZ with the destination PUMA. This is a conservative way of building the migration variable since it is possible that people moving to a PUMA that does not fully overlap with the CZ are moving to a different labor market.

2. To build aggregates at the CZ level we use the product of the individual's weight provided by the Census with the share of the population in the PUMA that belongs to the CZ. This

means that every person in the PUMA counts towards all the CZ in the PUMA. To keep the total of number of people consistent with the total population we multiply the weight by the share of the population in each PUMA that belongs to a CZ.

PUMA delineations change approximately every ten years. Between 2005-18 there were two PUMA delineations, the ones based on the 2000 census applied from 2005 to 2011 and the ones from the 2010 census applied from 2012 to 2018. We are using the annual ACS files, and according to the available documentation, these data are representative of areas with populations over 65,000. WE exclude CZ that at any point in the sample had a population below 65,000. We perform the current statistical analysis with the 455 CZ above the population threshold.

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