Entrepreneurial Spawning from Remote Work

Alan Kwan, Ben Matthies, Richard Townsend, and Ting Xu*

May 17, 2024

Abstract

This paper shows that remote work increases wage workers' transition into entrepreneurship. Using big data on Internet activities, we create novel firm-level measure of remote work. We show that firms with greater increase in remote work during the pandemic are more likely to see their employees subsequently becoming entrepreneurs. This holds both unconditionally and relative to other transitions conditioning on job turnovers. We establish causality using instrumental variables and panel event study. The spawning response is stronger among younger and more educated employees, and the marginally created businesses are not of low quality. The effect is not driven by employee selection, preference change, or layoffs. Rather, remote work increases spawning by providing the time, flexibility, and downside protection needed for entrepreneurial experimentation. We calibrate that at least 9.2% of the post-pandemic increase in new firm entry can be attributed to spawning from rising remote work.

JEL: E32, J22, J24, L26, M13

Keywords: Remote Work, Work-from-Home, Entrepreneurship, New Firm Entry, Labor Market, Experimentation

*Kwan: University of Hong Kong, apkwan@hku.hk; Matthies: University of Notre Dame, bmatthie@nd.edu; Townsend: Univesity of California San Diego, rrtownsend@ucsd.edu; Xu: University of Toronto, tingxu.xu@rotman.utoronto.ca). We thank Jose Maria Barrero for helpful comments. We thank the anonymous data provider for support.

"*While people have always worked nights and weekends to start their own businesses, remote work gives them more time and flexibility to do so and a better hedge against failure.*"

– Vox, "The Rise of the Side Startup", 08/11/2022

1 Introduction

The labor market has experienced a massive shift to remote work in the past few years, catalyzed by the pandemic. In 2023, full days worked from home account for 28% of paid workdays, four times higher than the level in 2019 (Barrero et al., 2023). At the same time, business formation surged during the pandemic and has stayed high (Decker and Haltiwanger, 2023). This paper examines whether there is a link between these two macro phenomena, by testing whether remote work increases workers' transition to entrepreneurship using novel micro data.

Understanding whether and how remote work spawns entrepreneurship is important because the majority of entrepreneurs come from wage employment. Hence, frictions within wage employment may impact labor flows to entrepreneurship, and ultimately growth and innovation (King and Levine, 1993; Decker et al., 2014). As companies and policy makers continue to evaluate the merits of remote work, entrepreneurial spillovers could be an important consideration in their costs-benefit calculations.

Answering the above question, however, is empirically challenging. First, we need to be able to accurately measure remote work at the firm level. However, most available measures are surveys-based and only cover a very limited sample of firms. Second, we also need to observe worker-level transitions into entrepreneurship. Finally, variation in remote work policies across firms is not random, as firms that adopt more remote work friendly policies may also have employees that are more entrepreneurial. This makes it difficult to establish if there is a causal effect.

To overcome the measurement challenge, we exploit big data on Internet activities to create firm-level measure of remote work. The data allows us to classify IP addresses and track anonymized individuals across their work place, home, and mobile devices. It also links individuals to their employers. We aggregate this data across all employees of a firm to obtain firm-level measure of remote work as the percentage of internet activity each month belonging to an remote IP address.[1] By measuring firm-wide remote work rather than individuals' take-up of remote work, we mitigate individual-level endogeneity by exploiting the fact that individuals have limited influence over firm-wide policies. Hence, we estimate an "intent-to-treat" effect.

[1] This measure is extensively validated in (Kwan et al., 2023).

We then link our firm-level remote work measure to employer-employee matched data from LinkedIn, available through Revelio Labs. The LinkedIn data contains the job history of each worker. This allows us to observe transition to entrepreneurship from wage employment, i.e., spawning. We also observe the characteristics of workers and their employers. To mitigate potential truncation bias from stale CVs on LinkedIn, we track spawning activities up till December 2022, even though our LinkedIn data is as of October 2023.

Our baseline cross-sectional analysis focuses on U.S. firms with at least 10 employees in February 2020 (the month before COVID) and their employees as of then.[2] We examine the effect of a firm's change in remote work from 2019 to 2020/21 on the spawning activities of its Feb2020 employees from March 2020 December 2022. We conduct this analysis at both the individual- and firm-level. Our baseline OLS estimates show that a one-std-dev increase in remote work increases the likelihood of entrepreneurial spawning by 8% at the individual level, and the spawning share of a firm's employees by 4% at the firm level. The spawning response is stronger among younger, better-educated, and more senior employees; it is also stronger in younger firms but does not vary with firm size.

We take several approaches to address potential endogeneity of remote work. First, we control for a variety of employee and firm characteristics in our baseline specification, including firm's pre-pandemic spawning share and worker's past founder experience. These two important controls help absorb ex-ante entrepreneurial tendencies at both the firm and worker level.

Second, we use instruments to isolate exogenous variation in remote work. Our primary instrument is a firm's employee commute distance before the pandemic, measured also from our Internet activity data. The idea is that firms whose Workers located father away from the office before the pandemic are more likely to adopt remote work post COVID. We posit that commute distance is idiosyncratic and largely predetermined before the pandemic, thus providing an exogenous source of variation in remote work tendencies. We also use firms' lagged political leaning and local business closure orders during the pandemic as alternative instruments. Our preferred 2SLS estimate shows that a one-std-dev increase in a firm's remote work increases the likelihood of its employees spawning for entrepreneurship by 45% relative to the mean.

Third, we use event studies to compare changes in spawning rates around the pandemic across firms with different remote work tendencies, as proxied by our instruments. We use two samples for this dynamic analysis. The fixed-employees sample tracks a fixed set of employees (i.e., those employed in February 2020) of Feb2020 firms over time regardless of their actual employer. This sample shuts down employee recomposition, and hence rules out the possibility that our results

[2] We exclude firms with fewer than 10 employees as our firm-level remote work measure is less accurate for these firms.

are driven by selection—e.g., remote work increase attracts more entrepreneurial employees and repels less entrepreneurial ones. Our second sample, the all-employees sample, tracks the spawning activities of all employees of Feb2020 firms, allowing for employee compositional changes and thus selection. Both sample generate results similar to our cross-sectional results, though the fixed-employees sample yields larger estimates, suggesting that selection is likely negative. We also find similar results using industry-level remotability (Dingel and Neiman, 2020) as an alternative treatment variable.

The above results are robust to alternative samples, additional controls, and alternative definitions of spawning. Our main analysis excludes firms with more than 5000 employees to mitigate our individual-level analysis being skewed by the largest firms. We show our results are robust to including them. We also show that our results are robust to including additional controls such as workers' age, education, and job role. Our results are similarly strong when restricting to spawnings that happen after an individual formerly leaves her wage employer, suggesting remote work is not only spawning side, part-time entrepreneurship. Lastly, using Census firm entry data, we show that our micro evidence also holds at the aggregate level: industries or locations with greater remote work tendencies saw greater new firm entry post the pandemic.

To what extent are our findings unique to entrepreneurship? It is possible that forces that drive remote workers to entrepreneurship could also drive them to other wage employers or non-employment. To investigate this, we conduct a conditional analysis, restricting our sample to employees that experienced job turnovers, i.e., those have left the Feb2020 employer between March 2020 and December 2022. Examining entrepreneurial spawning among this sample thus test whether remote work *disproportionately* directs workers to entrepreneurship relative to other destinations in the labor market. We find similar results with this conditional analysis. In particular, a one-std-dev increase in ΔRW increases turnovers into entrepreneurship relative to other destinations by 5.4% under OLS, and by 51% under 2SLS. As such, our results do not just capture a general turnover effect of remote work; rather, remote work can uniquely shift workers towards entrepreneurship. Importantly, the conditional analysis also rules out any remaining concerns about data truncation due to stale CV, as we condition our analysis on observing a CV update.

We explore three non-mutually exclusive mechanisms behind our results: 1) preference change, 2) experimentation, and 3) forced entrepreneurship. Under preference change, remote work induces a preference towards flexibility or "a quiet life" with less employer monitoring. If this channel drives our results, we should expect the marginal entrepreneurs to concentrate in low-growth, flexibility-based entrepreneurship, such as self-employment. However, we find the marginally spawned firm is more likely to be an employer, have a website, and receive future VC-backing than the average new firm. This suggests that the marginal entrepreneur tends to be of high quality. This result echoes our prior finding of a stronger spawning response among better educated workers. Additionally, we find that spawning is more likely to go into industries with less remote work than the prior employer, suggesting that preference for remote work is not driving our result.

The experimentation channel posits that remote work spawns entrepreneurs by providing the time and downside protection needed for entrepreneurial experimentation (Kerr et al., 2014). Remote work frees up time by removing commute, increasing productivity, and offering more flexible hours. Such slack time can be used by a worker to develop and experiment with business ideas. Remote work also offers less employer monitoring, which helps to keep a worker's side exploration in "stealth", reducing downside career risks. All these allow a worker to better use her wage employment as a fall back option when exploring entrepreneurship (Gottlieb et al., 2022). If this channel is at work, we should see stronger entry response into industries where experimentation is more valuable, such as those with higher failure risk. We indeed find this when we split spawning by exit probability of young firms in an industry. Additionally, if remote work enables experimentation by relaxing time constraint, we should expect our result concentrates where such constraint is more binding: e.g., high-growth entrepreneurship that requires large time commitment. Our prior heterogeneity result supports this finding.

Finally, our results could reflect forced entrepreneurship, where remote work leads to layoff, and laid off workers subsequently become entrepreneurs out of necessity. We rule this out by showing that our results are similar when restricting to firms that experienced continued employment growth during the pandemic, i.e., those unlikely with mass layoffs. Our analysis conditional on turnovers should also rule out this channel, since layoff should not trigger entrepreneurship more than other turnover outcomes, such as unemployment or job switches. Overall, our evidence is most consistent with remote work spawning entrepreneurship by providing workers the time and protection needed for entrepreneurial experimentation.

We end our paper with a back-of-the-envelope calibration at the macro level. Based on our firm-level estimate, we calibrate that at least 9.2% of the post-pandemic increase in new firm entry can be explained by spawning from remote work. Of course, there can by other channels through which remote work impacts entrepreneurship, such as investment opportunities or local agglomerations. Nevertheless, our paper uncovers a novel link between the two most important economic phenomenon post the pandemic: the rise of remote work and business entry.

Our paper adds to the fast-growing literature on remote work (Barrero et al., 2023). The literature has shown that remote work persisted after the pandemic and is predicted to stay in the long-run, due to both new information learned through the pandemic and better remote work technologies (Barrero et al., 2021b; Aksoy et al., 2022). There is large variations in the adoption of remote work across occupations, geographies, firms, and industries (Hansen et al., 2023; Aksoy et al., 2022). The productivity impact of remote work is largely positive, with some mixed evidence for fully remote (Bloom et al., 2015, 2024; Kwan et al., 2023; Emanuel and Harrington, 2023; Gibbs et al., 2023). Related to entrepreneurship, Han et al. (2021) show that VCs invest in more distant startups during lockdowns, driven by remote technologies. Our paper adds to this literature by uncovering the spillover effect of remote work—entrepreneurial spawning. Our results suggest that the impact of remote work on aggregate productivity may be higher than firm-level estimates. To the extent that employers cannot capture all the positive externalities of entrepreneurial spawning, firms may under-invest in remote work. Policy makers should take such spillover effects into account when designing future labor market policies.

We also contribute to the literature on labor and entrepreneurship. Babina (2020) and Babina and Howell (2024) document entrepreneurial spawning from the financial distress and R&D of established firms. Hacamo and Kleiner (2022) and Bernstein et al. (2024) examine how economic cycles affect individuals' choice between wage employment and entrepreneurship. Hombert et al. (2014) study how downside insurance for unemployed workers affects the quality of new firms started by these individuals. Our paper examines how a new paradigm of work impacts entrepreneurship. We show that remote work provides the safe space needed by workers to experiment with entrepreneurial ideas before they formally take the plunge. As such, our mechanism is related to Gottlieb et al. (2022), who show that job-protected leave mitigates career risks associated with entrepreneurial experimentation. Remote work can be thought of as a flexible form of "job-protected leave".

2 Data, Measures, and Samples

2.1 Data

Firm Internet Activity Data. Our data consists of individual-level internet activity including the user, the URL of each website visited, and timestamps of access. The data also includes information about each user such as the device type, approximate latitude and longitude when accessing the internet, and the company they work for. The data captures a substantial fraction of internet activity, comprising approximately one-fifth of the 4 billion IPv4 addresses in the world. Since websites, as well as some servers and internet-connected devices, are assigned IP addresses, the IP addresses we observe likely comprise an even larger fraction of IP addresses primarily used for human content consumption.

We obtain the data through a partnership with a data analytics company from the marketing technology space, the "Data Partner". The Data Partner maintains a large network of partnerships with online publishers, focused primarily (but not exclusively) on business content and news. Contributors include thousands of major internet publishers. Most participate anonymously but span a wide range of business functions such as technology, finance, marketing, legal, human resources, manufacturing, science, and general business. Participating publishers contribute to the Data Partnerâs pooled dataset via a technology mechanism which shares information about web content consumption, including the external IP address of the network originating the HTTP request and the URL of content accessed. Overall, the platform aggregates around 1 billion content consumption events per day. From this dataset, the Data Partner performs two steps: (1) it associates visitors with the companies they work for, when possible, and (2) it quantifies the "topics" of the content visitors read about.[3] Our data in this paper are more granular than what is sold commercially by the Data Partner. Accordingly, our access to the data is designed to take special care with respect to confidentiality restrictions—while we observe browsing activity at the individual level, we do not know the identity of any individual persons in the data.

To associate user sessions with the firms they work for, the Data Partner creates a profile through the use of first- and third-party cookies. This enables the publisher, and in turn the Data Partner, to observe when a visitor returns to a website. Over time, the Data Partner infers the association between the profile and their place of employment (Company) through a wide ensemble of industry-accepted methods. For example, user profiles are associated with a Company when visitors use a work email to log into a participating publisher's website. Another example is through IP addresses. That is, if a profile consistently logs onto a publisher website from a work-associated IP address, this gives a strong association the profile belongs to a particular company. The Data Partner also receives data from third-party sources who perform identity resolution of visitors. Through its proprietary processes, the Data Partner assembles these various sources of data and determines whether a reliable association between a profile and a company can be inferred, and when it can be, what that association is.

Crucially, once a visitor has been associated with a company reliably, the visitor is associated with that company even though the visitor may traverse different IP addresses. This is the primary mechanism through which we are able to monitor transitions between different types of IP addresses, and thus whether the employee is remote or not.

A notable limitation of data is that one can only observe internet content in the cooperative. In addition, mappings between users and employers and IP addresses to locations is estimated and imperfect. However, we hope given the large magnitude of available data, idiosyncratic noise in

[3] From these two steps, the Data Partner produces analytics are primarily sold to companies to facilitate sales and marketing—by identifying companies with heightened research interest in a specific business topic, these companies can target likely customers. Participating publishers receive some of the Data Partner analytics in return for providing the data.

classifying individual employers or IP addresses can be mitigated.

Employer-Employee Matched Data. We obtain employer-employee matched data from Revelio Labs, which is underlied by LinkedIn data. Our data consists of the universe of LinkedIn users, their CVs, and employer profile pages up to October 2023. The CV data includes each individual's job history, education, skills, and demographics, among others. Revelio/LinkedIn is used by many other studies (e.g., Agrawal et al. (2021), Chen et al. (2023), and Eisfeldt et al. (2023)), including studies on entrepreneurship (e.g., Hacamo and Kleiner (2022), Jeffers (2024), Bernstein et al. (2024)). This data also gives use firm-level employment size, industry, business description, founding year, and firm website (if any).

One limitation of LinkedIn data is that not all workers are on LinkedIn. However, LinkedIn likely captures the set of workers we are interested in, i.e., those who are *at risk of* spawning. These tend to be knowledge workers or younger workers, who are well captured by LinkedIn. Additionally, workers on LinkedIn overlaps well with those tracked by our Internet activity data, as both sets likely bias towards knowledge workers. Another limitation is potential truncation issues with stale LinkedIn profiles. We discuss how we address truncation concern in Sections 2.2 and 5.

Other Firmographic Data. We supplement Revelio/LinkedIn data with other firmographic databases such as Aberdeen CiTDB and People Data Labs (PDL), which source firm profiles from various sources. These data gives us additional information on firm NAICS, location, founding year, domain, etc.

2.2 Key Measures

Remote Work Measure. Kwan et al. (2023) develops a measure of remote work, RW, which is premised on classifying IP addresses using pre-pandemic data. The classification covers over 760 million IPs, about 20% of possible IPv4s and a likely greater fraction of IPv4s used by *humans* (a large number of IP addresses belong to servers). The IPs are classified into one of four classes: business, residential, VPN, or mobile. The classification is conducted using a two-step approach: first using rules-of-thumb to classify IP addresses, and second using a machine learning model to pick up the remainder unclassified IPs. Importantly, our classification is based on pre-pandemic information. Kwan et al. (2023) provides more details and validation of the classification.

To compute the extent to which a firm is working remotely, we calculate the fraction of the firm's IP traffic during work hours that is originating from a remote IP. We define a remote IP as a VPN, residence, or mobile IP — that is, any IP that is not an office or business address. We compute this fraction at the firm-month and firm-year level.[4] Our RW measure is available from 2019 to 2021, and we are currently extending it to 2022. The RW measure can also be flexibly constructed at the local or industry level.

In the time-series, remote work increases sharply at the start of the pandemic, which we show in Figure 1. Kwan et al. (2023) performs a variety of validation tests. For example, at the county-week level, remote traffic *during the day* increases when SafeGraph reports people going into the office less, with an elasticity of roughly -75%. This elasticity drops during night. They also report industry-level results consistent with Dingel and Neiman (2020). We refer interested readers to Kwan et al. (2023) for validation details.

Given that the level of RW captures some differences in the way companies manage their networks (i.e., mobile phones if they do not offer a corporate Wifi), the level of remote work is not very comparable across firms. We therefore focus on the changes in RW within each firm as our main independent variable. This also makes sure we do not capture any remote work differences across-firms before the pandemic, which may correlate with corporate culture, etc.[5] Specifically, we measure changes in a firm's RW from 2019 to 2020/2021. We define $\Delta RW_{f,2019\to 2020/21} = 0.5(RW_{f,2020} + RW_{f,2021}) - RW_{f,2019}$, i.e., the increase from 2019 to 2020/2021 average. Table A.1 shows the top and bottom industries by $\Delta RW_{f,2019\to 2020/21}$. As expected, IT and professional services had the highest increases in RW, while retail trade, construction, and agriculture had the lowest increase.

Because users are anonymous in our Internet data and cannot be linked to LinkedIn employees, we are not able to measure remote work at the employee level. However, the benefit of firm-level measure is that it is more exogenous than individual-level measure, as an individual employee has limited influence over firm-wide policies. As such, we can think of individual-level RW as "takeup", and firm-level RW as intent-to-treat. Given a firm's RW policy, individual employee's decision to take up is obviously more endogenous, as it may be driven by the person's expected costs and benefit of takeup, which could correlate with her entrepreneurial tendencies.

Spawning. We use LinkedIn employment history to measure spawning from wage employment to entrepreneurship. A spawning event is defined as an individual reporting a new job with the following conditions met simultaneously:

[4]we restrict to firms for which we can reliably measure RW. In particular, we restrict to firm-months satisfying the following criteria: 1) in a given month, have 100 work-time observations (9 a.m. to 5 p.m.), where an observation is a session-timestamp-url from an employee of the firm; (2) average at least 1,000 observations per month whether on the weekend, or weekday, during work hours, or otherwise, for at least half of months from 2019 until February 2020, and half of months from March 2020 to end of December 2021; (3) are included in one of our firmographic databases.

[5]In fact, we will control for firms' RW in 2019 in our analysis.

1. The new job is with a company different from the prior employer
2. The individual is within the first five employees of the started firm (ranked by job start date)
3. The job start date is within one year of firm founding date identified by LinkedIn.
4. The job title contains "founder" (including co-founder), "founding", "owner", or "entrepreneur". In the case no employees of the firm has such titles, we use titles "CEO", "partner", or "president".

Although our LinkedIn data is as of October 2023, we track spawning events till December 2022 to mitigate potential truncation bias from stale CV. We use the new job start date as the spawning event date. In some cases, spawning happens before a person formally leaves her current salary job. Such overlap can happen either because side entrepreneurship is permitted by the employer, or because founder retroactively reports the new firm start date after she quits and the startup gets out of the "stealth" mode.

For our cross-sectional sample, we track all spawning events from the Feb2020 firms from March 2020 to December 2022. For our firm-level panel, we track spawning events for each firm-year. Because spawning is low frequency, we multiple both the individual-level spawning dummy and the firm-level spawning share (i.e., fraction of employees that spawned) by 100, for ease of interpreting coefficients.

2.3 Samples

Our firm-level cross-sectional sample starts with all firms on LinkedIn with at least one employee as of February 2020 with non-missing RW measure. We refer to these firms as "Feb2020 firms". To make sure we can reliably measure firm-level RW, we restrict to firms with at least 10 employees as of February 2020.[6] This also makes sure we are capturing entrepreneurial spawning from relatively established firms. Our individual-level cross-sectional sample consists of all US-based employees of these Feb2020 firms employed as of February 2020. We refer to this sample as "Feb2020 employees". To mitigate concern that our individual-level sample is skewed by mega firms, we exclude firms with more than 5000 employees. Our results are robust to including these firms. Our baseline cross-sectional sample has about 13.5 million workers from 136k firms.

We then extend the cross-sectional samples into two firm-year level panels. The first panel tracks the Feb2020 employees over time (2015 to 2022) regardless of whether they were still with the Feb2020 firms. We call this sample "fixed employee panel". The second panel tracks Feb2020

[6] Our results are similar if restricting to firms with at least 5 for 20 employees.

firms over time and include all their employees, not just the Feb2020 employees. We call this sample "all employee panel". Section 3 provides more details on why and how we construct these samples.

2.4 Summary Statistics

Table 1 provides summary statistics for our cross-sectional samples. The mean spawning rate over March 2020 to Dec 2022 is 0.34% across all employees. At the firm-level, the average spawning share over the same period is 0.43%. The difference reflects the fact that larger firms tend of have lower spawning rate (Gompers et al., 2005). The average $\Delta RW_{f,2019\rightarrow 2020/21}$ at the firm-level is 0.13. The median firm in our sample has 27 employees and is 35 years old. The median employee in our sample has a job tenure of 3 year, holds a junior rank (seniority=2), and has a salary of 72K. About 0.2% of the employees have prior founding experience between 2015 and 2019. Table 2 presents the summary statistics for our two firm-level panels. The mean spawning rate is lower than in cross-sectional sample both because we measure annual spawning rate instead of cumulated spawning rate, and because pre-pandemic spawning rates are lower than post-pandemic.

3 Empirical Strategies

3.1 Cross-Sectional Analysis

Individual Level. Our individual level analysis focuses on a cross-section of workers employed as of February 2020. We then track these individuals' spawning activities from March 2020 to December 2022, and relate this outcome to the change in RW of her Feb-2020 employer. Specifically, we estimate the following specification:

$$Spawn_{i,\text{2020-2022}} = \alpha_n + \beta_c + \theta_r + \gamma \times \Delta RW_{f,2019\rightarrow 2020/21} + \lambda \boldsymbol{X_i} + \rho \boldsymbol{X_f} + \epsilon_i \tag{1}$$

In this equation, the dependent variable $Spawn_{i,f,\text{2020-2022}}$ is a dummy equal to one if the individual ever started a new business from March 2020 to December 2022. The key independent variable $\Delta RW_{f,2019\rightarrow 2020/21}$ is the change in the Feb-2020 employer's RW from 2019 to 2020/2021. Specifically we compute firm-year level RW for each year from 2019 to 2021 by averaging the monthly values. We then define $\Delta RW_{f,2019\rightarrow 2020/21} = 0.5(RW_{f,2020} + RW_{f,2021}) - RW_{f,2019}$, i.e., the increase from 2019 to 2020/2021 average.

We include fixed effects for Feb2020 firms' 4-digit NAICS industry (α_n) and county (β_c). We also include a host of individual- and firm-level controls. $\boldsymbol{X_i}$ is a vector of individual-level controls that include job tenure, seniority, and log salary measured as of February 2020, as well as an indicator for past founder experience. $\boldsymbol{X_f}$ is a vector of firm-level controls that include RW in 2019, log employment size in Feb 2020, firm age in 2020, and its entrepreneurial spawning rate in 2019. Importantly, individuals' past founder experience and firms' past spawning rate help absorb latent spawning factors as both the individual and the firm level. We cluster standard error by firm's industry (NAICS 4-digit).

To mitigate the concern that our estimated individual-level effect is skewed by the largest firms, we restrict to firms with no more than 5000 employees as of February 2020. Our subsequent firm-level analysis also addresses this concern by weighting firms equally. To mitigate potential measurement errors in spawning share, we restrict to firms with more than 10 employees. We show robustness to relaxing these size restrictions.

Firm Level. Our firm-level specification is analogous to the individual-level, except that we collapse all individual-level variables to firm-level averages. As such, our dependent variable is the share of Feb2020 employees that later started a business between March 2020 and December 2022, and our firm-level controls now also include the average job tenure, seniority, and log salary of the firm's employees as of February 2020, as well as the average past founder experience of its employees, in addition to the ones specified in Equation 1. Specifically, we estimate the following firm-level specification.

$$SpawnShare_{f,\text{2020-2022}} = \alpha_n + \beta_c + \gamma \times \Delta RW_{f,2019\to2020/21} + \rho\boldsymbol{X_f} + \epsilon_f \tag{2}$$

2SLS. We also estimated a 2SLS version of both the individual-level and firm-level regressions, instrumenting $\Delta RW_{f,2019\to2020/21}$ with three instruments.

Our first instrument, $Commute_i$, is firm-level commute distance of employees measured in 2019 before Covid, calculated using our internet activity data. The intuition of this measure is that firms whose employees live farther from the office face higher costs of commuting. After the onset of the pandemic when remote work first became a consideration for many firms, we posit that, all else equal, firms with greater commute distances were more likely experiment with remote work. For example, consider two identical firms in Manhattan located across the street from one another. If one firm's employees live in Connecticut with a two hour daily commute and the other's live in Manhattan with a 20 minute daily commute, during pandemic when both firms consider remote work policies, the first firm is more likely to implement such policies. We posit that, within the same city, commute distance across firms is idiosyncratic and largely predetermined before the COVID-19 pandemic, thus providing an exogenous source of variation in the propensity to work from home.

We construct a measure of commute distance at the firm-level for all US firms in our sample using the internet activity data. We leverage two key features of the data: first, our data is sufficiently granular that we observe the web browsing of each individual employee in every firm; second, meta data associated with each IP address allows us to observe the approximate location of each worker whenever they access the internet. We compare each employee's location during nonworking hours with their location during working hours (Monday through Friday between 9 a.m. and 5 p.m.). This allows us to calculate the approximate commute distance from each employee's home to the office for each firm in the United States.[7] We calculate the haversine distance between the approximate home location and the office location for each user session in each firm during 2019. For each firm, we compute the 25th and 75th percentiles of the commute distance across all sessions during 2019. We calculate the average commute distance of the middle 50th percentiles to obtain $Commute_i$, the average commute distance of employees at firm i. Kwan et al. (2023) provides more details on the validation of this instrument, including validation using SafeGraph data.

There are two potential concerns with using commute distance as an instrument. First, commute distance varies by firm geography. To address this issue, we normalize our commute distance measure within geography. This allows us to compare the commute distance of firms within the same city. We further include county fixed effects in our analysis. Second, commute distance could be correlated with worker characteristics. In one of our panel analyses, we fix employee composition to directly address this concern. Our cross-sectional analysis also control for pre-Covid firm size and remote work, which capture the differential ability to remote work prior to the pandemic, as well as firms pre-Covid spawning rate and workers' prior founder experience, which capture employees' underlying entrepreneurial tendencies. These controls mitigate the residual correlation between commute distance and firm types that is not going through remote work.

Our second instrument, $DemShare_i$, is firm-level share of Democratic contributions measured before 2020. We use the Federal Election Committee database on political contributions, which reveals the employer of the contributor. We ask Revelio Labs to apply their name matching algorithm, returning us a link between the political contributor and the Revelio Labs firm, if possible. For each firm, we calculate $DemShare_i$ as the fraction of political contributions to a Democrat from 2010 to 2018. We interpret an absence of observed contributions by an employee to be zero. Note that many firms have no employee contribution records so this instrument is missing for 47% of the firms in our sample. This instrument satisfies the relevance condition because the literature has found robust evidence that Democratic leaning individuals or institutions are more likely to practice social distancing during Covid. Hence, we expect that Democratic leaning firms are more likely to adopt remote work during the pandemic. Because we have county fixed effects, this instrument

[7] We do not observe any personal identifying information about any employee. We also do not observe precise locations of employee residences – we observe only the approximate neighborhood of each employee.

does not pick up geographic variation in political ideology, which may correlate with local business opportunities; rather, it captures variations in firm's tendency to adopt remote work *within the same location.*

One concern with the instrument is that a firm's political leaning may correlate with its employees' unobserved entrepreneurial tendency. Controlling for employees' past entrepreneurial activities and firm's past spawning rate helps absorb such tendencies. Further, Engelberg et al. (2023) shows that Republicans are more likely to start a new business than Democrats, even after conditioning on a variety of social-demographic variables. This means that, if anything, this residual correlation (after conditioning ex-ante entrepreneurial tendencies) would only bias us downward, making it harder for us to find a positive effect.[8]

Our last instrument is county-level business closure measures during COVID. We obtain local business open and closure orders from Spiegel and Tookes (2021). We then compute the average fraction of time over 2020 to 2021 that businesses were closed in a county, taking into account both full and partial closures. Specifically, Spiegel and Tookes (2021) categorizes four levels of business open measures: medium risk open, high risk open, higher risk open, and highest risk open. We assign a weight of 50%, 33.3%, 16.7%, and 0% to these levels respectively to compute the average closure time. This instrument satisfies the exclusion condition because these measures were installed by local politicians, partly in response to the severity of the local pandemic situation. In other words, the instrument isolates "forced" remote work changes. However, the downside is that this instrument is only at the county level and requires us to drop county fixed effects.

3.2 Firm Panel Analysis

We take two approaches to our panel analysis at the firm-level. The first approach fixes the set of employees and allow their employers to change and be different from their Feb2020 employers. The second approach fixes the set of firms and allow for compositional changes in employees. In both approaches, we estimate a firm-year-level DID based on the following equation:

$$SpawnShare_{f,t} = \alpha_f + \beta_t + \theta \times Treat_f \times Post2020_t + \epsilon_{f,t} \tag{3}$$

[8]One concern is that the 2020 election may shift Democrats towards entrepreneurship and Republicans away from it, due to changes in economic expectations (Engelberg et al., 2023). This should not affect our cross-sectional results, because in the cross-section Republicans are still more likely to start a business than Democrats, even during Democratic administration. This could, however, affect our DID estimates where we exploit changes in spawning rate. However, our subsequent DID analysis actually shows that the reduced form effect of *DemShare* is the stronger in 2020 (pre-election) than in 2021 and 2022 (post-election).

, where f indicates the Feb2020 firm, $Post2020_t$ indicates years$\geq$ 2020, and $Treat_f$ is a continuous treatment variable that is either ΔRW_f, $Commute_f$, $DemShare_f$, or $BizClosure_f$ (all standardized). The dependent variable is the share of the Feb2020 employees that started a new business in a particular year. We include firm fixed effects (α_f) for the Feb2020 firms and calendar year fixed effects (β_t). Standard errors are clustered by firms' NAICS 4-digit industry.

We also estimate a dynamic version of the baseline DID based on the following equation:

$$SpawnShare_{f,t} = \alpha_f + \beta_t + \sum_{t\neq 2019}^{2015\rightarrow 2022} \theta_t \times Treat_f \times \mathbb{1}(Year = t) + \epsilon_{f,t} \tag{4}$$

We omit 2019 as the base year. This specification will also tests whether the identifying assumption that firms with different changes in RW trended similarly before 2020 is likely to hold.

Fixing Employees but not Employers. Our first approach focuses on a fixed set of employees employed as of February 2020. We then track their entrepreneurial spawning from 2015 to 2022, regardless of whether they were still with the Feb2020 employer. We link all their spawning activities to the remote work policies of their Feb2020 employer, even if they did not spawn from the Feb2020 firm. By fixing the composition of employees of Feb2020 firms, this approach effectively removes individuals' selection into these firms based on unobserved characteristics. It also differences out individuals' latent spawning tendencies using their spawning events from other employers before or after their Feb2020 employer. To this end, we obtain a balanced panel of individual-years from 2015 to 2022. We then collapse this panel to the Feb2020-firm-year level. We estimate Equations 3 and 4 on this panel, where the dependent variable is the share of the Feb2020 employees that started a new business in a particular year, regardless of whether the individual was still with the Feb2020 firm. We refer to this sample as the "fixed-employees" sample.

Fixing Employers but not Employees. Our second approach fixes the set of employers instead of their employees. We track the spawning events by all employees of Feb2020 firms from 2015 to 2022, not just those employed in February 2020. As such, we allow for compositional changes in employees. However, different from the first approach, we only track spawning events from the Feb2020 firms, not from any firm. Specifically, we obtain a sample of individual-years based on the employment spells of all employees of the Feb2020 firms from 2015 to 2022. We define spawning year as the minimum of the new business start year and job end year.[9] We then collapse this panel to the Feb2020-firm-year level. We estimate the same specifications in Equations 3 and 4, expect

[9]As such, for our panel analysis, the spawning year is the business start year for businesses started during the employment spell with the Feb2020 firm, and is the job end year for those occurring after the employment spell. We do not track businesses started more a year away from the job end year.

that the dependent variable $SpawnShare_{f,t}$ is the share of employees spawning from the Feb2020 firm rather than from any firm, and the employee set is time-varying rather than fixed at February 2020. We refer to this sample as the "all-employees" sample.

By allowing for employee recompositions, this approach accommodates the possibility that part of the effect of remote work on entrepreneurial spawning is through selection: firms with generous remote work policies attract and retain employees that are innately more entrepreneurial. However, a priori, the selection effect could also go the opposite: firms with generous remote work policies retain employees who prefer flexibility or a quiet life, while firms quickly bringing employees back to office lose such type of employees, who then start new businesses for flexibility reasons.

4 Main Results

4.1 Cross-Sectional Results

Table 3 presents the individual-level cross-sectional result. Column 1 reports the OLS result based on Equation 1. We find that workers who experienced greater increase in remote work during COVID are significantly more likely to leave their employer and start a new business between 2020 and 2022. In particular, a one-std-dev increase in ΔRW increases the spawning rate by 8% relative to the mean. The control variables all exhibit sensible signs. In particular, firms that had more remote work pre-COVID, smaller firms, and younger firms are more likely to have their employees spawning for entrepreneurship post-COVID; so are firms that had higher spawning rates in 2019, a control we include to absorb unobserved employee spawning tendencies. In terms of employee characteristics, those with shorter job tenure, higher seniority, higher salary, and prior founder experience are more likely to leave for entrepreneurship. These effects are consistent with determinants of entrepreneurial spawning documented in prior literature (e.g., Gompers et al. (2005), Babina et al. (2023), Babina and Howell (2024)).

Column 2 of Table 3 presents the 2SLS result, where the instrument is firm-level average commute distance before COVID. The instrument is strong in the first stage, with a F-stat of 35. The instrumented coefficient indicates that a one-std-dev increase in ΔRW increases workers' spawning likelihood by 120%. This magnitude is much larger than the OLS estimate, but within the range of 2SLS/OLS ratios surveyed in Jiang (2017). It is also consistent with the presence of confounders that could bias OLS estimate downward relative to 2SLS estimate. Fore example, companies with greater ΔRW during COVID may offer better non-wage amenities or greater job flexibility, hence better retaining their employees and reducing spawning.

We find similar results with our two other instruments, firm-level Democratic contribution share (*DemShare*) and county-level business closure measures during COVID (columns 4-6). The F-stats are 25.2 and 9.4, respectively. Note that our instruments will be stronger in subsequent firm-level analysis due to overweighting of larger firms in individual-level samples. Although we cannot rule out the possibility that there may still exist confounders that violate the exclusion condition of our instruments, the consistent results across our three instruments after conditioning on a variety of controls greatly reduces any remaining endogeneity concerns.

Table 4 presents our firm-level cross-sectional results. We collapse both LHS and RHS variables from individual-level to the firm-level. As such, the dependent variable is the percentage share of employees spawned between 2020 and 2022, and individual-level controls are now firm averages. Relative to the individual-level specification, the firm-level specification weighs each firm equally. We continue to find similar results with smaller magnitudes than individual-level results. For example, based on columns 1 and 2, a one-std-dev increase in ΔRW increases firm-level spawning share by 4% under OLS, and by 45% under 2SLS when the instrument is *Commute*. The results based on the two other instruments are similar, and are both smaller in magnitude than the corresponding individual-level estimates. Importantly, the F-stats of the instruments in the first stage are much higher at the firm-level than those at the individual-level, suggesting that the larger 2SLS coefficients are unlikely to be driven by weak instruments.

4.2 Firm Panel Results

Table 5 presents the firm-level DID results estimated based on Equation 3. In Panel A, we fix the set of employees employed as of Feb2020, and link their Feb2020 employers' remote work changes to these individuals' spawning activities over time, irrespective of which employers they were with. We estimate this on a balanced panel of Feb2020 firms from 2015 to 2022. The dependent variable is the spawning share (times 100). We find that firms with a greater increase in remote work during COVID had a higher spawning rate post 2020 than pre 2020. This holds whether we measured increases in remote work directly or through our instruments. We also find similar result using industry-level remotability (Dingel and Neiman, 2020)—the extent to which jobs in an industry can be done remotely—as an alternative treatment. In particular, firms with a one-std-dev higher ΔRW had a 5.6% higher spawning rate post 2020 than pre 2020. This effect is 9.1%, 10%, 12.5%, and 16% when we proxy remote work increase through *Commute*, *DemShare*, *BizClose*, and *Remotability*, respectively. Notably, because this sample fixes the set of individuals and track their spawning over time regardless of their employers, the results are not driven by selection of employees into firms. In other words, our results cannot be driven by the possibility that firms that increased remote work during COVID also attracted employees that later became entrepreneurs, or lost less entrepreneurial employees.

We find similar results in Panel B of Table 5, when we allow for employee recompositions in our firm-level panel. Specifically, we track the spawning activities by *all* employees from firms that existed in February 2020, regardless of whether these employees were at these firms in February 2020. Different from the sample in Panel A, this sample allows for the possibility that employees' selection on firm's remote work policies drive some of our results. We find results largely similar to Panel A. In particular, firms with a one-std-dev higher *Commute* experienced a 7.8% increase in spawning rate post 2020 relative to pre 2020. This effect is 11%, 7.8%, and 9.4% when we proxy for RW increase with *DemShare*, *BizClose*, and *Remotability*, respectively. However, we do not find an effect with the direct ΔRW measure. The smaller effects in Panel A relative to Panel B suggests that employee selection induces a negative bias in our results: increases in remote work tend to attract employees that were less entrepreneur, while losing more entrepreneurial employees. This is consistent with the notion that less entrepreneurial employees tend to prefer a "quiet life", while more entrepreneurial employees prefer in-person interactions. We discuss this more in Section 6.

We visualize the dynamics of the above DID results in Figures 4 and 5 presents the results. Figure 4 shows that the effects are strongest in 2020 and 2021 but declines significantly in 2022, except when treatment is measured with *Commute*. This is likely due to a weaker first-stage effect of our treatment variables on the level of RW in 2022, which we will verify after obtaining 2022 RW measures.[10] Nevertheless, it seems that employees' pre-pandemic commute patterns have a persistent effect on RW levels well into 2022. Importantly, we observe largely parallel trends between treatment and control groups before 2020, especially in the 2 to 3 years leading up to the pandemic, lending support to our identification assumption. We observe similar effects for the all-employee panel in Figure 5.

4.3 Robustness

We investigate the robustness of our main results in this section.

Including the largest firms. Our baseline analysis restricts to firms of employment size between 10 and 5000. Panel A of Table A.2 shows the results for all firms above 10 employees.

Additional controls. Table A.3 demonstrates the robustness of our cross-sectional results to including additional controls. In particular, we additionally control for individuals' age and education as of February 2020, and in individual-level sample, fixed effects for their job roles in February 2020. Specifically, we infer a person's age based on his/her undergraduate degree year, and in case it's missing, high school finish year. For education, we control for whether the individual has a graduate degree and whether her undergraduate degree is from a top-100 school based on the

[10] Another possibility is potential truncation in observing spawning events on LinkedIn in 2022.

Times higher education ranking. We dummy out individuals without any education information. Table A.3 shows that the results remain similar.

Spawning before vs after departing wage job. About one-third of the spawning events in our sample happen before the worker formally leaves her wage employment job. This can happen either because side entrepreneurship is permitted by the employer, or because founder retroactively reports the new firm start date after she quits and the startup gets out of the "stealth" mode. If our results are all driven by side entrepreneurship, i.e., entrepreneurship activities while holding a full-time job, it may call into question the quality of the spawned business, as well as whether there is any career risk associated with transition to entrepreneurship from wage employment. To check this, we split our dependent variable by whether the spawning event happens before or after departure from the wage job, and rerun our main analysis in Table A.4. We find that the response is stronger for spawnings that happen after quitting than those happening while employed. Although experimentation with entrepreneurship could start before a firm is formally launched, this finding alleviates the concern that remote work only drives side, part-time entrepreneurship.

4.4 Heterogeneity

Next, we explore the heterogeneity in our baseline cross-sectional results. We interact ΔRW with various employee and firm characteristics (all measured as of February 2020), and visualize the estimates through graphs. Figure 2 shows heterogeneity across employee characteristics. We find that younger and better educated employees have a much stronger spawning response to remote work than older and less educated employees. In particular, workers below 33 (median age in our sample) are five time more responsive than those above 33 (Panel A). Those with a graduate degree are three times more responsive than those without, and those with a BA from top-100 school are four times more responsive than those without (Panel B). These results are consistent with the finding in Bernstein et al. (2022), where they find that young and skilled individuals are most responsive to local entrepreneurial opportunities. Interesting, there is a non-linear heterogeneity along seniority (Panel C). Medium-ranked employees are least responsive to remote work, while lower-ranked employees have stronger responses. However, the most senior employees at the executive level have the strongest response, being 3 to 4 times more responsive than all other employees.[11]

Figure 3 explores heterogeneity across firm types. We find that conditional on a firm's remote work policies, the spawning response of its employees does not depend on firm size, yet depends strongly on firm age. In particular, employees of firms less than 10 year old are four times more responsive to remote work increase than employees of older firms. This finding is consistent with the

[11] Revelio categorizes jobs into seven seniority levels: 1. entry level, 2. junior level, 3. associate level, 4. manager level, 5. director level, 6. executive level, 7. senior executive level.

"Fairchild view" of entrepreneurial spawning in Gompers et al. (2005), where young firms prepare employees for entrepreneurship by educating them about the process and exposing them to relevant networks. We find that, not only are young firms more likely to have a higher spawning rate in the baseline, their employees are also more responsive to workplace arrangements that push them towards entrepreneurship.

5 Is it Unique to Entrepreneurship?

One may argue that some of the forces that drive the effect of remote work on entrepreneurship may also apply to worker turnovers in general, including turnovers into other wage employment or unemployment. To assess the extent to which our results are unique to entrepreneurship, we condition our individual-level analysis on those experiencing job turnovers (including turnovers into entrepreneurship) and examine whether remote work *disproportionately* directs individuals into entrepreneurship relative to other labor statuses. Specifically, we restrict to individuals who left their Feb2020 employer between March 2020 and December 2022. We then rerun our individual-level cross-sectional analysis on this subsample. This conditional analysis makes sure that we are not capturing a general job turnover effect; rather, any significant effect reflects mechanisms unique to entrepreneurship.

There are two additional benefits of this analysis. First, by conditioning on observing turnovers, i.e., the individual updating her CV on LinkedIn, it addresses any concerns with truncation issues in LinkedIn data. Second, this analysis helps rule out the interpretation that our results are driven by forced entrepreneurship, where remote work increases entrepreneurship by increasing layoff. This is because laid off workers should not flow into entrepreneurship more than into unemployment or other wage employment, particularly during an economic downturn (Pugsley and Èahin, 2019).[12]

Table 6 presents the result of this conditional analysis. We find that, conditional on workers leaving their jobs, those more exposed to remote work increases are more likely to pursue entrepreneurship relative to remaining non-employed or wage employed. This effect holds both in OLS as well as in 2SLS. For example, based on columns 1 and 2, a one-std-dev increase in ΔRW increases turnovers into entrepreneurship relative to other destinations by 5.4% under OLS, and by 51% under 2SLS. These effects suggest that the mechanisms through which remote work spurs entrepreneurship is somewhat unique to the economics of entrepreneurship. We explore this more in the next section.

[12]Pugsley and Èahin (2019) shows that startups and young firms are more pro-cyclical than incumbent firms.

6 Potential Mechanisms

We explore the mechanisms through which remote work spurs entrepreneurship in this section. We identity four non-mutually exclusive mechanisms: 1) employee selection 2) preference change 3) experimentation, and 4) forced entrepreneurship. Overall our evidence points towards the dominant role of the experimentation channel, where remote work gives employees downside protection and, possibly, the slack time needed for entrepreneurial experimentation.

Employee selection. Both ex-ante and ex-post selections on employee types could explain our results. Under ex-ante selection, firms that increased remote work more during COVID were already matched to employees with greater spawning tendencies before COVID. Under ex-post selection, increases in remote work by firms attract new employees who are more entrepreneurial, or drive away existing employees who are less entrepreneurial.

Our IV analysis and dynamic analysis with firm fixed effects help rule out the ex-ante selection story. Our dynamic analysis with fixed employee composition rules out ex-post selection story. It is also worth noting that, theoretically, the selection effect can also go the opposite way. High-RW firms could attract unambitious employees who prefer flexibility and a "quiet life", or lose entrepreneurial employees who prefer social interactions (we discuss preference change next). In fact, the larger effects found when fixing employees than when allowing employee recomposition in Section 4.2 points towards this negative selection. As such, employee selection cannot explain our results.

Preference change. One explanation of our results is that remote work increases workers' preference for flexibility or a "quiet life", which entrepreneurship may offer. This, however, should only predict spawning into low-growth, hobby-based self-employment that offers these non-pecuniary benefits (Schoar, 2010). In contrast, high-growth, transformational entrepreneurship is time-consuming and requires founders' full commitment. Our heterogeneity result in Figure 2 shows that the spawning response is much stronger for younger, better-educated, and higher-ranked employees, who are unlikely to pursue subsistence or flexibility-based entrepreneurship.[13]

We further directly examine the quality of the marginally spawned new businesses. To this end we split our spawning events by the quality of the started business. We use three quality measures 1) initial employment, 2) whether the business has a website, and 3) whether the business received

[13] This channel should also predict that spawning tends to happen after remote work stops (i.e., bringing employees back to office), rather than during remote work, because remote work itself can substitute flexibility-based entrepreneurship and help high-RW firms retain employees with such preferences.

VC backing.[14] We then compare the effect of remote work on high- versus low-quality spawning. If the preference channel drives our results, we should observe a much stronger response in low-quality spawning than in high-quality spawning.

Table 7 presents the result, where we split the spawning outcome by each of our three quality measures. The bottom row reports the percentage effect of a one-std-dev increase in ΔRW relative to outcome mean. We find that remote work as a stronger effect on spawning into employer business than into non-employer business (Panel A). Similarly, spawning effect is stronger for businesses with a website than those without (Panel B). Importantly, we observe a much stronger spawning response of VC-backed firms than non-VC backed firms, with the former being 3 to 4 times larger than the latter. These results suggest that remote work does not spawn primarily low-quality businesses that offer entrepreneurs more flexibility or other non-pecuniary benefits; rather, a majority of the marginally spawned businesses are of high quality.

Finally, the remote work level of the spawned business itself could speak to whether the spawning event is preference based. Due to their nascency, we do have enough data to accurately measure the exact RW of the spawned businesses. However, we can proxy it using their industry's average RW at the time of founding. If preference for flexibility drives our results, we should see employees spawning into industries with the same or higher levels of remote work than the original employer (upshift in RW), rather than lower levels (downshift in RW). Table A.5 split spawning events based on whether they represent an upshift or downshift in RW. We find that the effect concentrates in spawning with an upshift in RW. In contrast, downshift-RW spawnings have insignificant or even negative responses to remote work. However, we caveat that we do not observe the precise RW level of the spawned firm.

Taken together, these evidence suggests that preference changes is unlikely to explain the positive effect of remote work on entrepreneurial spawning. We next explore non-preference-based channels.

Experimentation Remote work could provide workers the time and "stealth" needed to experiment with entrepreneurship without risking their current career. First, remote work frees up time by saving on commuting time and increasing productivity, which reduces actual work hours.[15] Remote

[14] We define initial employment as the maximum employment in the initial two years of a business' life. We observe a firm's website URL from its LinkedIn page and restricts to independent business URLs that are not hosted on social media (e.g., Facebook), e-commerce platforms (e.g., Etsy), or Google (i.e., Google site). We identify VC-backed as firms that can be matched to the VC-backed universe in Crunchbase as of 2024.

[15] Barrero et al. (2023) report that the average daily savings in commuting and grooming time is 65 minutes for American workers. The literature typically finds that hybrid arrangement (i.e., WFH some days of the week) increases worker productivity, while fully remote arrangement less so, though the lower productivity are often offset by savings on commute time. See detailed review by Barrero et al. (2023).

work also frees up time by increasing flexibility: employees can work on their side project during lunch breaks or lulls of their job. This slack time gives workers the opportunity to develop and tinker with entrepreneurial ideas (Agrawal et al., 2018). Remote work can also provide downside protection for experimentation. By offering employees more private space and less monitoring by the employer, remote work reduces the likelihood that one's side project is discovered by her employer, which may negatively impact her career.[16] All these together allow an employee to better use her current job as a fallback option while exploring entrepreneurship, which was less feasible while working in office. From this perspective, this channel is similar to the career risk channel in Gottlieb et al. (2022), where job-protected leave increases workers' experimentation with entrepreneurship by providing downside protection. Here, we can think of remote work as a flexible form of "job-protected leave".

If the downside protection channel is at work, our result should be stronger in industries with a higher risk of failure, as the option value to experiment is higher in these industries. Table 8 explores such heterogeneity. Similar to Table 7, We split the dependent variable into spawning into high-risk vs low-risk industries, based on the probabilities of failure by young firms (age$\leq$5) in U.S. Business Dynamic Statistics (BDS). We find that remote work induces significantly more spawning into industries with higher failure risk than those with lower failure risk. This holds for both OLS and 2SLS specifications, and the differences are statistically significant. This result supports the downside protection channel.

If remote work enables experimentation by providing more time and flexibility, our results should be stronger for entrepreneurship that requires greater time commitment, such as innovative, high-growth startups. Entrepreneurship that is already flexible and less time-consuming, such as selling crafts on Etsy or running an Airbnb, should respond less to remote work, as they could be done even with an in-person job. Our prior findings that the marginally spawned firm is more likely to be high quality and in industries with lower RW supports the idea that remote work relaxes time and flexibility constraints for potential entrepreneurs.

Forced entrepreneurship. One last possibility is that remote work induces forced entrepreneurship by triggering layoffs. For example, firms that increased RW more have also have laid off more workers, who in turned started their own businesses out of necessity. This story is unlikely to be true given that, during COVID, firms that pivoted more to RW have adapted better, while those relying more on in-person work suffered more and had more layoffs (Forsythe et al., 2020; Mongey et al., 2021).

Another possibility is that high-RW firms experienced more quitting as they tried to bring

[16]Several studies (Gibbs et al., 2023; Yang et al., 2022; Parker, 2023; Emanuel et al., 2023) find that remote work leads to fewer contacts and less communication within the organization and reduces mentoring.

workers back to office in 2022 (Barrero et al., 2021a), and these quitted employees later started a business. To the extent that this quitting is driven by a preference for flexibility or "quite life", it amounts to the preference channel, which we ruled out above. Note that this story also implies the spawning should happen with a substantial delay, after high-RW firms bring workers back to office. The immediate response we see in 2020 speaks against this story.

To further test the forced entrepreneurship channel, we restrict our main analysis to firms that experienced continued employment growth in both 2020 and 2021, i.e., firms that are unlikely to have mass layoffs during COVID. Panel B of Table A.2 reports the results. We find larger rather than smaller effects than in our main sample. This suggests that our main results are unlikely to be drive by forced entrepreneurship. Finally, layoffs should trigger job turnovers in general, not disproportionately entrepreneurship. Our results conditional on job turnover should therefore rule out this channel, as we examine whether turnover goes disproportionately into entrepreneurship vis-a-vis other labor statuses that are equally triggered by layoff.

Take together, the evidence in this section suggests that remote work spawns entrepreneurship mainly by providing the time and downside protection needed for entrepreneurial experimentation.

7 Aggregate-Level Evidence

We validate our micro-level evidence with aggregate-level evidence based on US Census data. The advantage of this analysis is that we can address any potential concerns about LinkedIn not capturing all new businesses, or capturing them with time truncation. This also helps verify whether our micro-level evidence can aggregate to the macro level.[17] The downside, however, is that we have to make the assumption that spawning tends to happen in the same industry as the prior employer. In our data, 18% (23%) of spawned entrepreneurs are in the same NAICS 3-digit (2-digit) as their previous employer. This is high given that there are 102 NAICS 3-digits and 20 NAICS 2-digits, implying a same-industry probability of 0.98% and 5% only if spawning is random.

Industry-Level Firm Entry. We first examine how changes in industry-level new firm entry around COVID varies with an industry's remotability—the extent to which its jobs can be performed at home or remotely (Dingel and Neiman, 2020). We estimate a dynamic DID of the following

[17] For example, if most of the firm-level variation in RW is within industry (or country) rather than between them, then a shift to remote work wouldn't generate any sectoral or regional differences in entrepreneurship rate.

specification at the NAICS 3digit-year level:

$$Ln(new\ firms)_{i,t} = \alpha_i + \beta_t + \sum_{t \neq 2019}^{2015 \to 2021} \theta_t \times Remotability_i \times \mathbb{1}(Year = t) + \epsilon_{i,t},$$

where α_i indicates industry fixed effects and β_t indicates year fixed effects. The dependent variable is the log number of new business started in a NAICS-3digit-year based on US Business Dynamic Statistics (BDS). Unfortunately, the latest BDS stops at 2021. *Remotability* comes from Dingel and Neiman (2020) and is the average remotability of an industry's jobs. It is standardized before interacting with year indicators.

Figure 6 presents the results. We find that industries with higher remotability experienced higher new firm entry in 2020 and 2021, while they trended similarly before 2020. The interpreting assumption is that workers tend to stay in the same industry when spawning. Of course, one interpretation is that more remotable industries are more desirable during COVID, hence experiencing greater new firm entry. To the extent this desirability is preference-driven, we already ruled it out in Section 6. We also show in Table A.5 that spawning does not flow into industries with higher levels of remote work than the prior employer.

County-Industry Level Employment by New Firms. We also use employment at new firms (age 0-1) from the US Quarterly Workforce Indicators (QWI) to measure new firm creation.[18] The advantage of this measure is that we can focus on employer businesses and make sure our result does not pick up low-quality entry; additionally, we can conduct our analysis at the country-industry level, which allows for richer fixed effects to absorb potential confounders. The cost, however, is that we must make a stronger assumption that entrepreneurs tend to stay in the same county and industry as their prior employer.

We estimate a dynamic DID of the following specification at the county-industry(NAICS 2digit)-year level, using the second quarter of the QWI:

$$Employmentatnewfirms_{c,i,t} = \alpha_{c,i} + \beta_{c,t} + \sum_{t \neq 2019}^{2015 \to 2023} \theta_t \times \Delta RW_{c,i}\ (or\ Remotability_i) \times \mathbb{1}(Year = t) + \epsilon_{i,t},$$

where $\alpha_{c,i}$ indicates county-industry fixed effects and $\beta_{c,t}$ indicates county-year fixed effects. The dependent variable is employment count at new businesses started in a county-NAICS-2 digit-year based on QWI data. The sample is from 2015 to 2023, the last available year of QWI. We estimate the specification using a Poisson regression.

[18] We find similar results expanding to employment of firms of age 0-3.

We use two versions of treatment. First, we use our RW measure aggregated to county-industry level.[19] Second, we use the Dingel and Neiman (2020) measure at the NAICS-3digit level. We discretize both treatments based on the 90th percentile cutoff.

Figure 7 plots the dynamic DID results. We find that new firm employment increased more in county-industries (or industries) with greater increase in remote work. The results are similar whether we measure treatment based on ΔRW (Panels A and B) or Dingel and Neiman (2020) remotability (Panels C and D), or whether we include county-industry and year (Panels A and C) or additionally include county-year fixed effects (Panels B and D).

7.1 Calibrate to the Macro Time Series

How much of the post-Covid increase in startup rate could be explained by the shift to remote work? We conduct a back-of-the-envelope calculation to answer this. Based on Figure 1, across all firms, the average RW increased by about 0.15 from pre-COVID to post-COVID. This translates to a 4.2% increase in spawning rate based on our cross-sectional firm-level OLS estimate in Table 4, and a 5.6% increase based on our DID estimate. There were about 130 million full-time employed individuals in the US before COVID. The average quarterly number of high-propensity new business applications increased from 320K pre-COVID to 430K post Covid (Figure 9 of Decker and Haltiwanger (2023)).[20] Based on Current Population Survey (CPS) data, 63% entrepreneurs come from wage employment pre-Covid.[21] Thus, the implied annual spawning rate is 0.62% (=320*4*0.63/130000). A 5% RW-induced increase in this spawning rate would imply 40.3K (=0.62%*5%*130000) additional HP applications per year. Given that the actual increase in annual HP applications is 440K from pre-COVID to post-COVID, our RW-based estimate can account for 9.2% of this increase.

[19] To do so, we use the Aberdeen CiTDB to find establishments of firms for which we have remote work data. We then weight them in the county proportional to their total reading activity, times their employment share in the county. For example, if we observe 1 million observations, and 4% of their employment is in Los Angeles county, they contribute to LA county with a weight of 40,000. Then, we construct a weighted average remote work measure based on digital activity in the county.

[20] Decker and Haltiwanger (2023) define high-propensity new business applications as those that will likely become employers. They report that, historically, high-propensity (HP) applications have been strongly predictive of actual firm entry, with a national correlation of 0.93 and an elasticity roughly on one at the aggregate level, within states, and within industries.

[21] Based on CPS data from 2016 to 2019, the period before Covid, we compute the fraction of entrepreneurs each year who were in wage employment in the previous year. This number is consistent with the estimate from Kauffman Foundation, which finds it to be between 60% and 70%.

8 Conclusion

The majority of entrepreneurs were previous wage workers. How work is organized in wage employment could therefore impact workers' decision of whether to become an entrepreneur. This paper shows that recent paradigm shift to remote work induces wage workers to transition to entrepreneurship. Using big data on Internet activities, we create novel measure of firm-level remote work. We then link it to LinkedIn data to test how changes in firms' remote work policies affect workers' transition to entrepreneurship, a phenomena we call entrepreneurial spawning. We find that firms that increased remote work more post-COVID saw a higher fraction of their employees starting new firms. The response is stronger for younger and more educated employees. Marginally created new firms tend to be of higher quality than average new firm. The spawning effect of remote work also holds conditional on job turnover, suggesting remote work direct workers disproportional to entrepreneurs relative to other labor outcomes. These results are not driven by employee selection, preference change, or forced entrepreneurship from layoff. Rather, remote work provides the time and downside protection needed for entrepreneurial experimentation, allowing workers to better use their wage job as a fallback option when exploring entrepreneurship. We estimate that at least 10% of the post-pandemic increase in new firm entry can be explained by the rise of remote work. Firms and policy makers need to take such spillover effect into account when designing future work policies.

References

Agrawal, A., C. Catalini, A. Goldfarb, and H. Luo (2018). Slack time and innovation. *Organization Science 29*(6), 1056–1073.

Agrawal, A., I. Hacamo, and Z. Hu (2021). Information dispersion across employees and stock returns. *The Review of Financial Studies 34*(10), 4785–4831.

Aksoy, C. G., J. M. Barrero, N. Bloom, S. J. Davis, M. Dolls, and P. Zarate (2022). Working from home around the world. *Brookings Papers on Economic Activity 2022*(2), 281–360.

Babina, T. (2020). Destructive creation at work: How financial distress spurs entrepreneurship. *Review of Financial Studies 33*(9), 4061–4101.

Babina, T. and S. T. Howell (2024). Entrepreneurial spillovers from corporate r&d. *Journal of Labor Economics 42*(2), 469–509.

Babina, T., P. Ouimet, and R. Zarutskie (2023). Ipos, human capital, and labor reallocation. *Journal of Financial and Quantitative Analysis*.

Barrero, J. M., N. Bloom, and S. J. Davis (2021a). Let me work from home, or i will find another job. *University of Chicago, Becker Friedman Institute for Economics Working Paper* (2021-87).

Barrero, J. M., N. Bloom, and S. J. Davis (2021b). Why working from home will stick. Technical report, National Bureau of Economic Research.

Barrero, J. M., N. Bloom, and S. J. Davis (2023). The evolution of work from home. *Journal of Economic Perspectives 37*(4), 23–49.

Bernstein, S., E. Colonnelli, D. Malacrino, and T. McQuade (2022). Who creates new firms when local opportunities arise? *Journal of Financial Economics 143*(1), 107–130.

Bernstein, S., R. R. Townsend, and T. Xu (2024). Flight to safety: How economic downturns affect talent flows to startups. *The Review of Financial Studies 37*(3), 837–881.

Bloom, N., R. Han, and J. Liang (2024). Hybrid working from home improves retention without damaging performance. *Nature*, 1–6.

Bloom, N., J. Liang, J. Roberts, and Z. J. Ying (2015). Does working from home work? evidence from a chinese experiment. *The Quarterly journal of economics 130*(1), 165–218.

Chen, A., M. B. Zhang, and Z. Zhang (2023). Talent market competition and firm growth. *Available at SSRN*.

Decker, R. and J. Haltiwanger (2023). Surging business formation in the pandemic: Causes and consequences. *Brookings Papers on Economic Activity*, 3–24.

Decker, R., J. Haltiwanger, R. Jarmin, and J. Miranda (2014). The role of entrepreneurship in us job creation and economic dynamism. *Journal of Economic Perspectives 28*(3), 3–24.

Dingel, J. I. and B. Neiman (2020). How many jobs can be done at home? *Journal of Public Economics 189*, 104235.

Eisfeldt, A. L., G. Schubert, M. B. Zhang, and B. Taska (2023). The labor impact of generative ai on firm values. *Available at SSRN 4436627*.

Emanuel, N. and E. Harrington (2023). Working remotely? selection, treatment, and the market for remote work. *Selection, Treatment, and the Market for Remote Work (May 1, 2023). FRB of New York Staff Report* (1061).

Emanuel, N., E. Harrington, and A. Pallais (2023). The power of proximity to coworkers: training for tomorrow or productivity today? Technical report, National Bureau of Economic Research.

Engelberg, J., J. Guzman, R. Lu, and W. Mullins (2023). Partisan entrepreneurship. *Journal of Finance, forthcoming*.

Forsythe, E., L. B. Kahn, F. Lange, and D. Wiczer (2020). Labor demand in the time of covid-19: Evidence from vacancy postings and ui claims. *Journal of public economics 189*, 104238.

Gibbs, M., F. Mengel, and C. Siemroth (2023). Work from home and productivity: Evidence from personnel and analytics data on information technology professionals. *Journal of Political Economy Microeconomics 1*(1), 7–41.

Gompers, P., J. Lerner, and D. Scharfstein (2005). Entrepreneurial spawning: Public corporations and the genesis of new ventures, 1986 to 1999. *The journal of Finance 60*(2), 577–614.

Gottlieb, J. D., R. R. Townsend, and T. Xu (2022). Does career risk deter potential entrepreneurs? *The Review of Financial Studies 35*(9), 3973–4015.

Hacamo, I. and K. Kleiner (2022). Forced entrepreneurs. *The Journal of Finance 77*(1), 49–83.

Han, P., C. Liu, X. Tian, and K. Wang (2021). The death of distance? covid-19 lockdown and venture capital investment.

Hansen, S., P. J. Lambert, N. Bloom, S. J. Davis, R. Sadun, and B. Taska (2023). Remote work across jobs, companies, and space. Technical report, National Bureau of Economic Research.

Hombert, J., A. Schoar, D. Sraer, and D. Thesmar (2014). Can unemployment insurance spur entrepreneurial activity? Technical report, National Bureau of Economic Research.

Jeffers, J. S. (2024). The impact of restricting labor mobility on corporate investment and entrepreneurship. *The Review of Financial Studies 37*(1), 1–44.

Jiang, W. (2017). Have instrumental variables brought us closer to the truth. *Review of Corporate Finance Studies 6*(2), 127–140.

Kerr, W. R., R. Nanda, and M. Rhodes-Kropf (2014). Entrepreneurship as experimentation. *Journal of Economic Perspectives 28*(3), 25–48.

King, R. G. and R. Levine (1993). Finance and growth: Schumpeter might be right. *The quarterly journal of economics 108*(3), 717–737.

Kwan, A., B. Matthies, and A. Yuskavage (2023). Measuring the impact of remote work using big data. *working paper*.

Mongey, S., L. Pilossoph, and A. Weinberg (2021). Which workers bear the burden of social distancing? *The Journal of Economic Inequality 19*(3), 509–526.

Parker, K. (2023). About a third of us workers who can work from home now do so all the time.

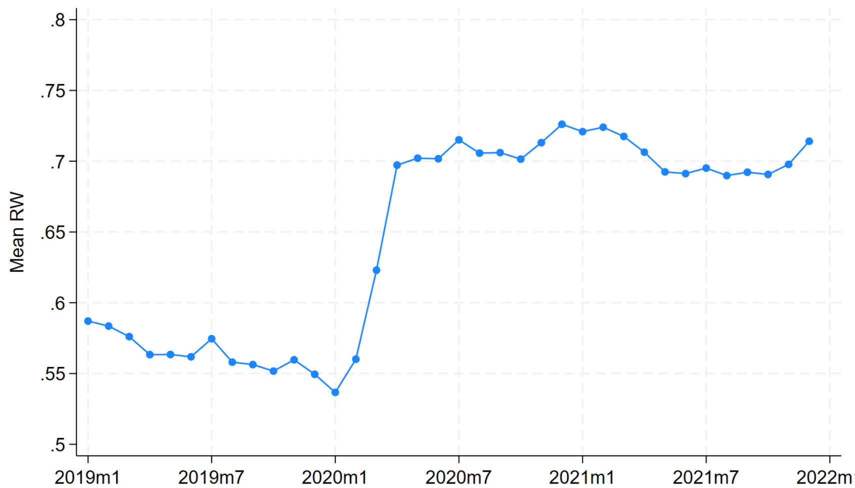
Pugsley, B. W. and A. Èahin (2019). Grown-up business cycles. *The Review of Financial Studies 32*(3), 1102–1147.

Schoar, A. (2010). The divide between subsistence and transformational entrepreneurship. *Innovation policy and the economy 10*(1), 57–81.

Spiegel, M. and H. Tookes (2021). Business restrictions and covid-19 fatalities. *The Review of Financial Studies 34*(11), 5266–5308.

Yang, L., D. Holtz, S. Jaffe, S. Suri, S. Sinha, J. Weston, C. Joyce, N. Shah, K. Sherman, B. Hecht, et al. (2022). The effects of remote work on collaboration among information workers. *Nature human behaviour 6*(1), 43–54.

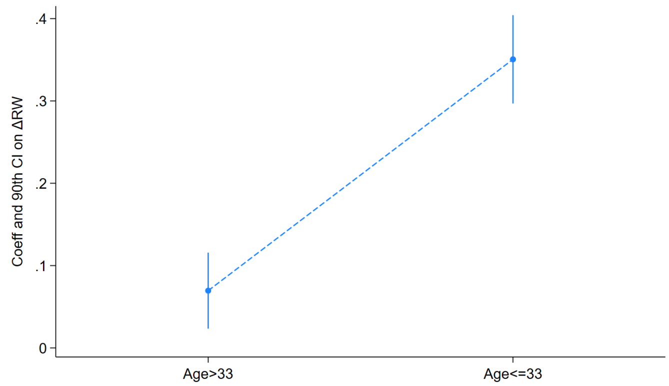
Figure 1: Monthly Remote Work Measure from 2019 to 2021



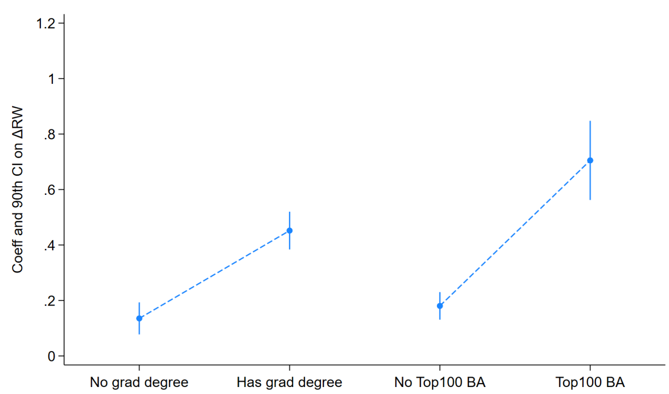
This graph plots the monthly average of our firm-level RW measure for the set of firms active in February 2020, for the period of January 2019 to December 2021.

Figure 2: Heterogeneity by Employee Characteristics

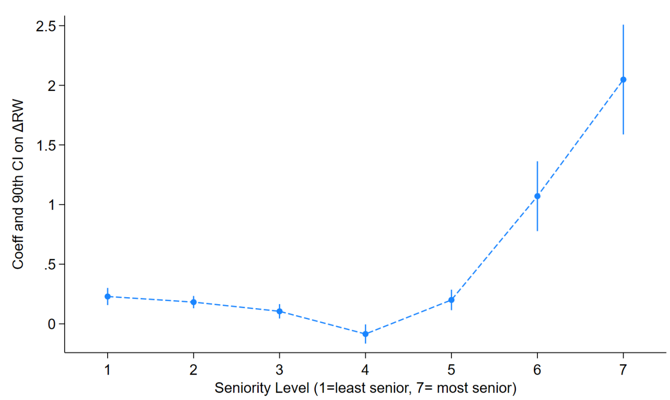
(a) By Employee Age



(b) By Employee Education



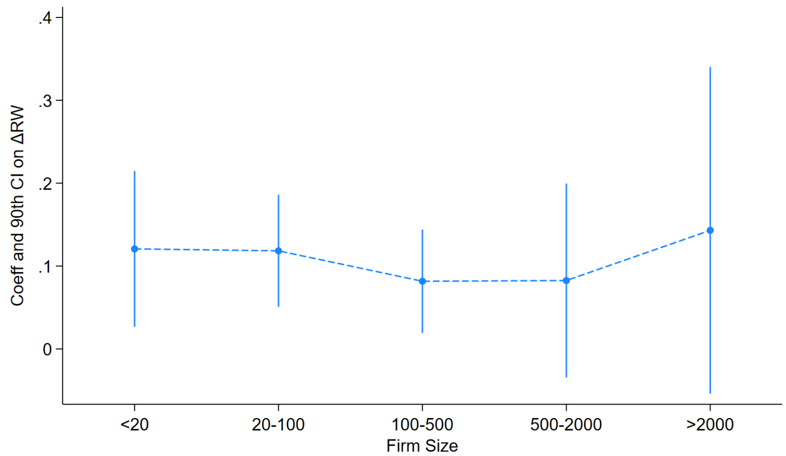
(c) By Employee Seniority



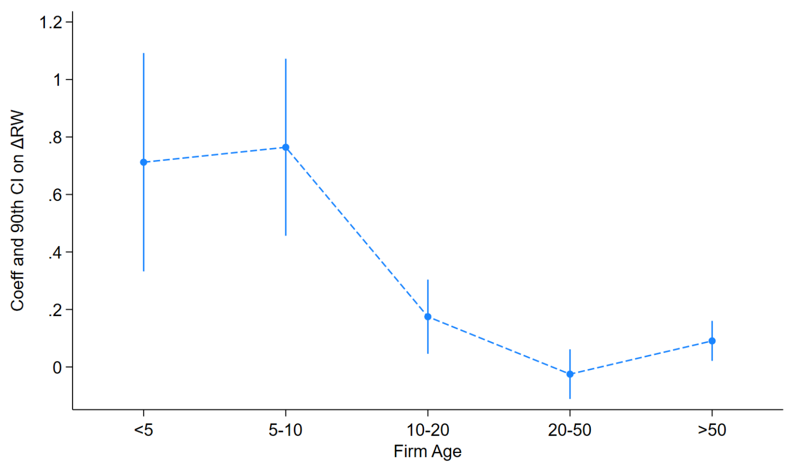
This figure shows the heterogeneity of our cross-sectional results along employee characteristics. We interact our individual-level OLS specification in column 1 of Table 3 indicators for employees' age≤33 (Panel A), education levels (Panel B), and seniority (Panel C). We then plot the coefficient and 90th confidence interval of the interaction terms.

Figure 3: Heterogeneity by Firm Characteristics

(a) By Firm Employment Size

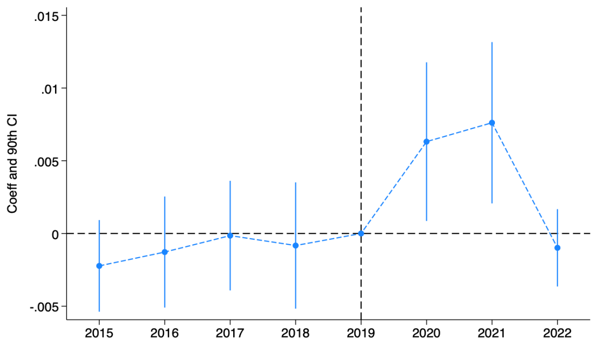


(b) By Firm Age

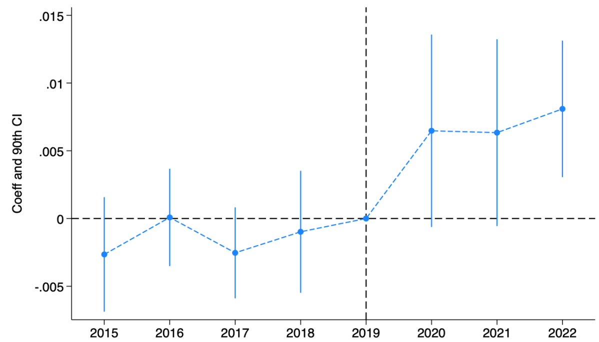


This figure shows the heterogeneity of our cross-sectional results along firm characteristics. We interact our firm-level OLS specification (column 1 of Table 4) with indicators for firms employment size (Panel A) and age bins (Panel B) (all measured as of February 2020). We then plot the coefficient and 90th confidence interval of the interaction terms.

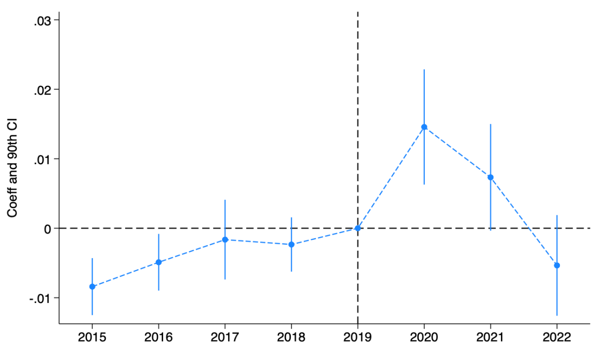
Figure 4: Firm-Level Dynamics: Fix Employees but not Their Employers



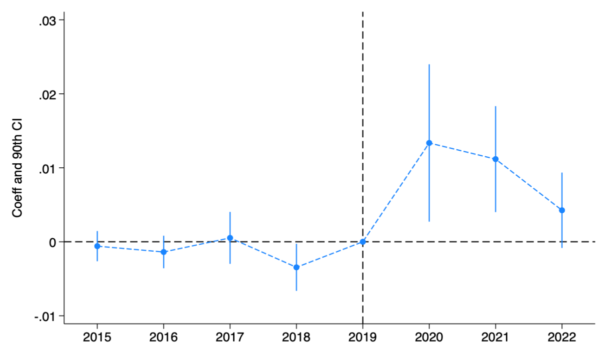
(a) Treat $= \Delta RW$



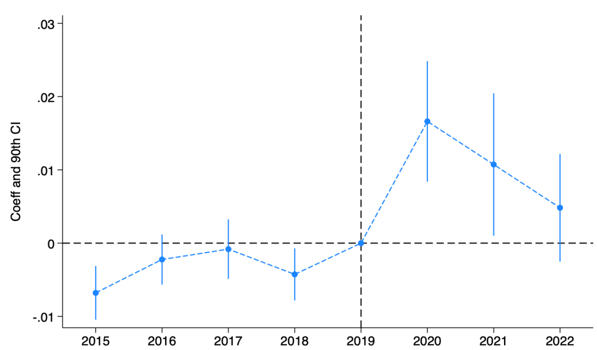
(b) Treat $= Commute$



(c) Treat $= DemShare$



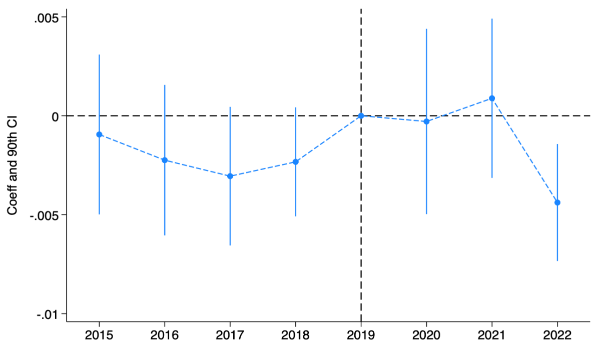
(d) Treat $= BizClose$



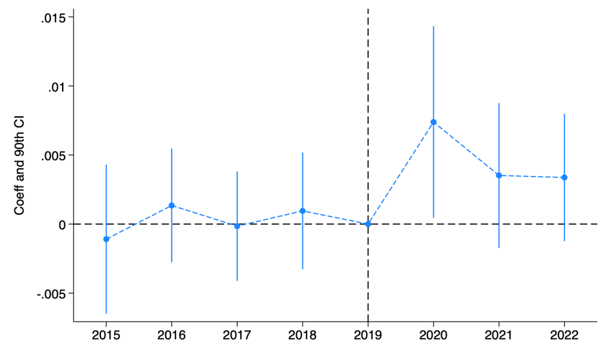
(e) Treat $= Remotability$

These figures show the dynamic DID effects estimated from firm-level panel regression in Equation 4. The sample tracks a fixed set of employees employed in February 2020 from 2015 to 2022 regardless of which employer they were with. We then link these spawning events to these individuals' Feb2020 employers and their RW policy change. The sample is collapsed to firm-year level and the dependent variable is 100 times the share of spawning employees. 2019 is the omitted base year. Each dot (bar) represents the point estimate (90th confidence interval) of the coefficient on $Treat_f \times \mathbb{1}(Year = t)$ in Equation 4, where $Treat_f$ is a continuous treatment variable that is either ΔRW (Panel A), $Commute$ (Panel B), $DemShare$ (Panel C), $BizClosure$ (Panel D), or $Remotability$ (Panel E)..

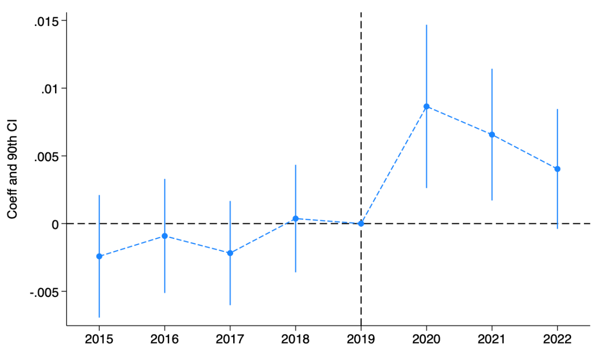
Figure 5: Firm-Level Dynamics: Fix Employers but not Their Employees



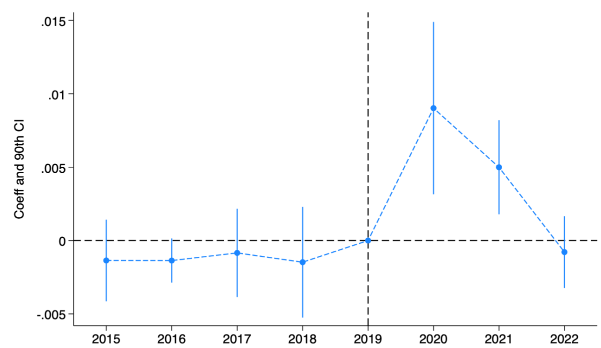
(a) Treat $= \Delta RW$



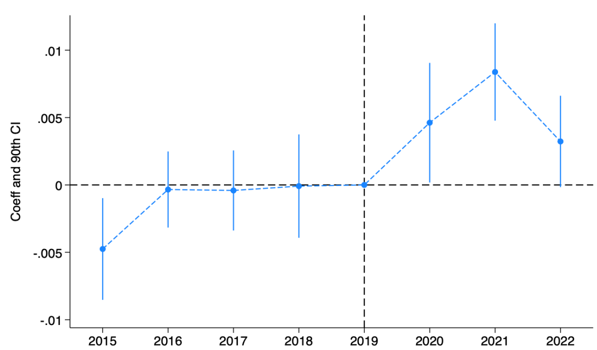
(b) Treat $= Commute$



(c) Treat $= DemShare$



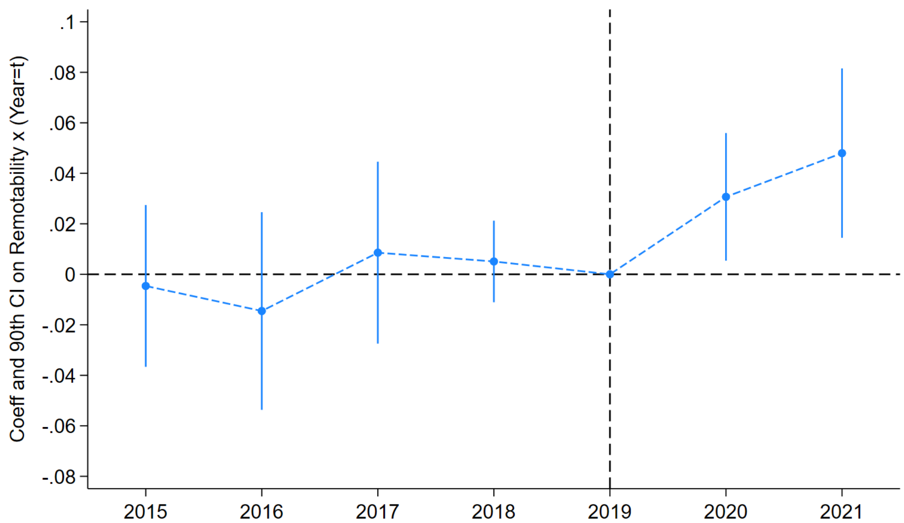
(d) Treat $= BizClose$



(e) Treat $= Remotability$

These figures show the dynamic DID effects estimated from firm-level panel regression in Equation 4. The sample tracks all employees of Feb2020 firms (i.e., firms active in February 2020) from 2015 to 2022. As such, we fix the set of firms but allow for employee compositional change. For each employee, we only track their spawning events from the Feb2020 firms. We then link these events to Feb2020 firms' RW policy change. The sample is collapsed to firm-year level and the dependent variable is 100 times the fraction of employees spawning from the Feb2020 firm. 2019 is the omitted base year. Each dot (bar) represents the point estimate (90th confidence interval) of the coefficient on $Treat_f \times \mathbb{1}(Year = t)$ in Equation 4, where $Treat_f$ is a continuous treatment variable that is either ΔRW (Panel A), $Commute$ (Panel B), $DemShare$ (Panel C), $BizClosure$ (Panel D), or $Remotability$ (Panel E).

Figure 6: Aggregate Evidence: BDS New Firm Entry

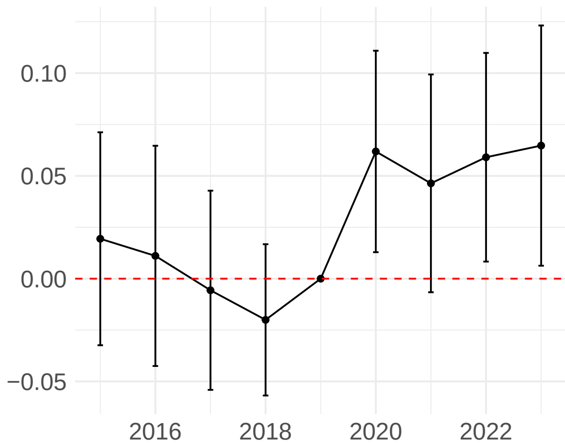


This figure shows how industry-level new firm entry around COVID varies with an industry's remotability—the extent to which its jobs can be performed at home or remotely (Dingel and Neiman, 2020). We estimate a dynamic DID of the following specification at the industry(NAICS 3digit)-year level:

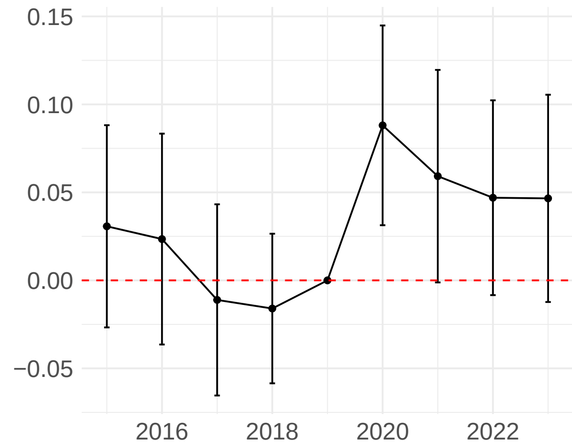
$$Ln(new\ firms)_{i,t} = \alpha_i + \beta_t + \sum_{n \neq 2019}^{2015 \to 2021} \theta_t \times Remotability_i \times \mathbb{1}(Year = t) + \epsilon_{i,t}$$

α_i indicates industry fixed effects. β_t indicates year fixed effects. The dependent variable is the log number of new business started in a NAICS-3digit year based on US Business Dynamic Statistics (BDS) data. *Remotability* comes from Dingel and Neiman (2020) and is the average remotability of an industry's jobs; it is standardized before interacting with year indicators. The sample is from 2015 to 2021, the last year of BDS. The figure plot the coefficient and 90th confidence interval of the interaction terms θ_t.

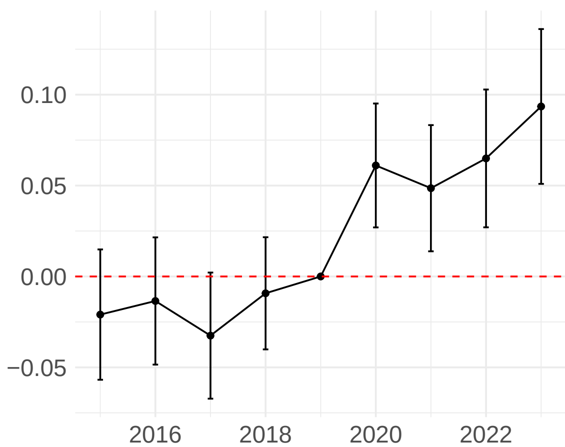
Figure 7: Aggregate Evidence: QWI Young Firm Employment



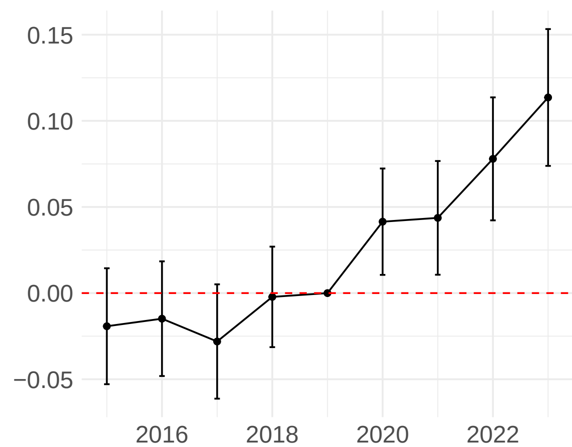
(a) Treat $= \Delta RW > 90^{th}\ pct$



(b) Treat $= \Delta RW > 90^{th}\ pct$



(c) Treat $=$ *Remotability*



(d) Treat $=$ *Remotability*

This figure shows how industry-level new firm employment around COVID varies with an industry's remotability—the extent to which its jobs can be performed at home or remotely (Dingel and Neiman, 2020). We estimate a dynamic DID of the following specification at the county-industry(NAICS 2digit)-year level, using the second quarter of the QWI:

$$Employment\ at\ new\ firms_{i,t} = \alpha_i + \beta_t + \sum_{n \neq 2019}^{2015 \to 2023} \theta_t \times Remotability_i \times \mathbb{1}(Year = t) + \epsilon_{i,t}$$

α_i indicates county-by-industry fixed effects. β_t indicates year fixed effects. The regression is estimated using a Poisson regression. The dependent variable is the employment at new business started in a NAICS-2 digit-county-year based on US Quarterly Workforce Indicators (QWI) data. *Remotability* comes from Dingel and Neiman (2020) and is the average remotability of an industry's jobs; it is standardized before interacting with year indicators. The sample is from 2015 to 2023, Q2, the last available date of the QWI. The figure plot the coefficient and 90th confidence interval of the interaction terms θ_t.

Table 1: Summary Statistics: Cross Sectional Sample

Panel A. Cross-Sectional Sample: Individual Level

Variable	N	p5	p50	p95	Mean	SD
$Spawn_{2020\text{-}2022}$	13542997	0.000	0.000	0.000	0.337	5.799
$\Delta RW_{2019\rightarrow 2020/21}$	13542997	-0.053	0.133	0.358	0.140	0.125
RW_{2019}	13542997	0.253	0.586	0.778	0.555	0.154
Ln(emp)	13542997	2.996	6.084	8.295	5.903	1.673
Firm age	13542997	10.000	47.000	159.000	62.272	49.501
Prior spawning rate	13542997	0.000	0.000	0.265	0.064	0.357
Tenure	13542997	1.000	3.000	19.000	5.206	6.250
Seniority	13542997	1.000	2.000	5.000	2.461	1.536
Ln(salary)	13542997	10.147	11.176	12.009	11.127	0.618
Prior founder	13542997	0.000	0.000	0.000	0.002	0.048
Commute	13542997	-0.621	0.289	1.361	0.317	0.621
DemShare	9310490	0.000	0.833	1.000	0.722	0.304
BizClose	12855359	0.070	0.200	0.307	0.180	0.076

Panel B. Cross-Sectional Sample: Firm Level

Variable	N	p5	p50	p95	Mean	SD
$SpawnShare_{2020\text{-}2022}$	136121	0.000	0.000	2.941	0.425	1.475
$\Delta RW_{2019\rightarrow 2020/21}$	136121	-0.119	0.124	0.391	0.128	0.154
RW_{2019}	136121	0.227	0.582	0.816	0.557	0.173
Ln(emp)	136121	2.303	3.296	5.894	3.599	1.125
Firm age	136121	9.000	35.000	128.000	47.283	39.088
Prior spawning rate	136121	0.000	0.000	0.000	0.081	0.727
Avg. Tenure	136121	2.312	5.212	10.556	5.656	2.597
Avg. Seniority	136121	1.619	2.520	3.701	2.575	0.632
Avg. Ln(salary)	136121	10.737	11.179	11.624	11.179	0.273
Avg. Prior founder	136121	0.000	0.000	0.019	0.003	0.013
Commute	136121	-1.152	0.160	1.282	0.121	0.754
DemShare	71364	0.000	0.833	1.000	0.639	0.401
BizClose	129830	0.069	0.187	0.307	0.177	0.077

This table presents the summary statistics for our cross-sectional samples. The individual-level sample focuses on all employees employed with a firm of employment size 10 to 5000 as of February 2020. The firm-level sample includes all firms with an employment size of 10 and 5000 as of February 2020.

Table 2: Summary Statistics: Firm Panel

Panel A. Firm Panel: Fixing Employees but not Their Employers

Variable	N	p5	p50	p95	Mean	SD
$SpawnShare_{\text{2020-2022}}$	1154056	0.000	0.000	0.045	0.088	0.646
$\Delta RW_{2019\rightarrow 2020/21}$	1154056	-1.492	0.009	1.663	0.034	0.951
Commute	1139784	-1.345	0.043	1.253	0.006	0.806
DemShare	607416	-1.659	0.458	0.862	-0.026	0.997
BizClose	1099936	-1.410	0.137	1.712	0.006	1.004
Remotability	1111456	-1.361	-0.010	1.270	-0.013	0.986
Post2020	1154056	0.000	0.000	1.000	0.375	0.484

Panel B. Firm Panel: Fixing Employers but not Their Emplyees

Variable	N	p5	p50	p95	Mean	SD
$SpawnShare_{\text{2020-2022}}$	1016489	0.000	0.000	0.000	0.064	0.543
$\Delta RW_{2019\rightarrow 2020/21}$	1016489	-1.421	0.013	1.603	0.039	0.912
Commute	1004174	-1.264	0.078	1.280	0.049	0.794
DemShare	554493	-1.610	0.484	0.880	0.017	0.973
BizClose	969149	-1.392	0.147	1.714	0.016	0.999
Remotability	977347	-1.368	-0.008	1.284	-0.010	0.989
Post2020	1016489	0.000	0.000	1.000	0.384	0.486

This table presents the summary statistics for our firm-level panels. The samples includes all firms with an employment size of 10 and 5000 as of February 2020. In Panel A, the sample is based on a fixed set of employees employed in February 2020 firms. We track these individuals over time across all their employers from 2015 to 2022 and collapse them to Feb2020-firm-year level. In Panel B, the sample is based on all employees of Feb2020 firms over 2015 to 2022 and is collapsed to Feb2020-firm-year level allowing compositional changes in employees. All variables are at the firm-level, except that *Remotability* is at the NAICS 3-digit level and *BizClose* is at the county-level.

Table 3: Cross-Sectional Analysis: Individual-Level

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Dep var:			$Spawn_{2020\text{-}2022}$			
$\Delta RW_{2019\rightarrow 2020/21}$	0.208***	3.218***	0.263***	4.697***	0.258***	8.209***
	(0.028)	(0.878)	(0.039)	(1.060)	(0.035)	(2.676)
RW_{2019}	0.224***	1.925***	0.283***	2.742***	0.248***	4.727***
	(0.036)	(0.521)	(0.046)	(0.575)	(0.044)	(1.493)
Ln(emp)	-0.030***	-0.041***	-0.032***	-0.050***	-0.026***	-0.059***
	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.012)
Firm age	-0.001***	-0.000***	-0.001***	-0.000**	-0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning rate	0.130***	0.118***	0.166***	0.144***	0.140***	0.102***
	(0.010)	(0.010)	(0.017)	(0.019)	(0.012)	(0.016)
Tenure	-0.014***	-0.014***	-0.014***	-0.014***	-0.015***	-0.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Seniority	0.094***	0.091***	0.093***	0.089***	0.094***	0.086***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)
Ln(salary)	0.026***	0.031***	0.028***	0.031***	0.036***	0.039***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.009)
Prior founder	4.538***	4.523***	4.626***	4.605***	4.504***	4.454***
	(0.155)	(0.154)	(0.181)	(0.180)	(0.153)	(0.147)
First-stage IV coeff:						
Commute		0.009***				
		(0.002)				
DemShare				0.015***		
				(0.003)		
BizClose						0.034***
						(0.011)
Kleibergen-Paap F-stat		35.043		25.178		9.367
NAICS 4-dig FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	No	No
Observations	13542997	13542997	9608356	9608356	13204037	13204037
R-squared	0.003	0.000	0.004	-0.001	0.003	-0.012

This table examines the impact of remote work on employees' entrepreneurial spawning at the individual level. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022. The key independent variable $\Delta RW_{2019\rightarrow 2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. Columns 1, 3, 5 estimate the OLS results on samples with each of the instrument being non-missing. Columns 2, 4, 6 estimate the corresponding 2SLS results. The sample focuses on all employees employed with firms of employment size between 10 and 5000 as of February 2020. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 4: Cross-Sectional Analysis: Firm-Level

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Dep var:			$SpawnShare_{\text{2020-2022}}$			
$\Delta RW_{2019\rightarrow 2020/21}$	0.116***	1.308*	0.152**	3.913***	0.156***	4.317***
	(0.038)	(0.677)	(0.060)	(1.220)	(0.039)	(1.049)
RW_{2019}	0.137***	0.857**	0.187***	2.452***	0.166***	2.667***
	(0.039)	(0.404)	(0.053)	(0.733)	(0.040)	(0.648)
Ln(emp)	-0.034***	-0.039***	-0.044***	-0.061***	-0.031***	-0.054***
	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.008)
Firm age	-0.001***	-0.000***	-0.001***	-0.000	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning rate	0.066***	0.065***	0.065***	0.064***	0.063***	0.060***
	(0.011)	(0.011)	(0.023)	(0.023)	(0.012)	(0.012)
Avg. Tenure	-0.041***	-0.040***	-0.044***	-0.039***	-0.044***	-0.038***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Avg. Seniority	0.197***	0.181***	0.210***	0.165***	0.189***	0.141***
	(0.018)	(0.022)	(0.018)	(0.025)	(0.018)	(0.020)
Avg. Ln(salary)	0.137***	0.172***	0.146***	0.233***	0.184***	0.243***
	(0.034)	(0.041)	(0.044)	(0.055)	(0.032)	(0.039)
Avg. Prior founder	8.293***	8.161***	9.051***	8.607***	8.520***	7.974***
	(0.797)	(0.779)	(1.115)	(1.151)	(0.771)	(0.752)
First-stage IV coeff:						
Commute		0.010***				
		(0.001)				
DemShare				0.014***		
				(0.001)		
BizClose						0.060***
						(0.006)
Kleibergen-Paap F-stat		299.455		93.618		84.753
NAICS 4-dig FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	No	No
Observations	136121	136121	72117	72117	131453	131453
R-squared	0.052	0.014	0.064	-0.052	0.040	-0.078

This table examines the impact of remote work on firm's spawning share at the firm level. The dependent variable is 100 times the fraction of employee starting a new business between March 2020 and December 2022. The key independent variable $\Delta RW_{2019\rightarrow 2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. Columns 1, 3, 5 estimate the OLS results on samples with each of the instrument being non-missing. Columns 2, 4, 6 estimate the corresponding 2SLS results. The sample focuses on all firm with employment size between 10 and 5000 as of February 2020. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 5: Firm-Level DID

Panel A. Fixing Employees but not Their Employers

	(1)	(2)	(3)	(4)	(5)
Dep var:			*SpawnShare*		
$\Delta RW_{2019\rightarrow 2020/21}\times$ Post2020	0.005***				
	(0.001)				
Commute × Post2020		0.008***			
		(0.002)			
DemShare × Post2020			0.009***		
			(0.002)		
BizClose × Post2020				0.011***	
				(0.002)	
Remotability × Post2020					0.014***
					(0.005)
Feb2020-Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1154056	1139784	607416	1099936	1111456
R-squared	0.04	0.04	0.043	0.04	0.04

Panel B. Fixing Employers but not Their Employees

	(1)	(2)	(3)	(4)	(5)
Dep var:			*SpawnShare*		
$\Delta RW_{2019\rightarrow 2020/21}\times$Post2020	0.000				
	(0.001)				
Commute × Post2020		0.005***			
		(0.002)			
DemShare × Post2020			0.007***		
			(0.002)		
BizClose × Post2020				0.005***	
				(0.001)	
Remotability × Post2020					0.006***
					(0.002)
Feb2020-Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1016489	1004174	554493	969149	977347
R-squared	0.064	0.064	0.059	0.064	0.063

This table shows the DID results estimated on a firm-year panel following Equation 5. The sample period is 2015 to 2022. The dependent variable is 100 times the share of spawning employees for a firm-year. *Post2020* is a dummy indicating years 2020-2022. We interact *Post2020* with four continuous firm-level treatment variables (all standardized): $\Delta RW_{2019\rightarrow 2020/21}$, the three instruments used in Table 3, and industry-level *Remotability*. Panel A tracks a fixed set of employees employed in February 2020 over 2015-2022 regardless of their employer. We then link these individuals' spawning events to their Feb2020 employers and collapse to firm-year level. Panel B is analogous but tracks all employees of Feb2020 firms from 2015 to 2022 (i.e., allowing for employee compositional change), focusing only on spawning events from the Feb2020 firms. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 6: Cross-Sectional Analysis: Individual-Level Conditional On Turnover

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Dep var:			$Spawn_{2020\text{-}2022}$			
$\Delta RW_{2019\rightarrow 2020/21}$	0.305***	2.854**	0.395***	4.899***	0.397***	7.876***
	(0.053)	(1.234)	(0.075)	(1.723)	(0.060)	(2.997)
RW_{2019}	0.387***	1.869**	0.488***	3.061***	0.444***	4.776***
	(0.064)	(0.730)	(0.080)	(0.977)	(0.076)	(1.714)
Ln(emp)	-0.072***	-0.081***	-0.073***	-0.090***	-0.066***	-0.096***
	(0.006)	(0.007)	(0.007)	(0.009)	(0.006)	(0.013)
Firm age	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning rate	0.198***	0.187***	0.246***	0.222***	0.212***	0.174***
	(0.016)	(0.017)	(0.028)	(0.030)	(0.019)	(0.021)
Tenure	0.015***	0.016***	0.016***	0.017***	0.014***	0.017***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
Seniority	0.171***	0.168***	0.168***	0.163***	0.171***	0.161***
	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Ln(salary)	0.244***	0.247***	0.236***	0.237***	0.255***	0.251***
	(0.025)	(0.025)	(0.028)	(0.029)	(0.027)	(0.028)
Prior founder	7.998***	7.987***	8.069***	8.051***	7.948***	7.905***
	(0.273)	(0.273)	(0.323)	(0.323)	(0.274)	(0.272)
First-stage IV coeff:						
Commute		0.009***				
		(0.001)				
DemShare				0.016***		
				(0.003)		
BizClose						0.039***
						(0.011)
Kleibergen-Paap F-stat		35.937		27.945		11.364
NAICS 4-dig FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	No	No
Observations	6447182	6447182	4611393	4611393	6283367	6283367
R-squared	0.006	0.004	0.006	0.003	0.006	-0.001

This table examines the impact of remote work on employees' entrepreneurial spawning at the individual level, conditional on individuals that experienced turnovers from their Feb2020 employers post February 2020. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022. The key independent variable $\Delta RW_{2019\rightarrow 2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. Columns 1, 3, 5 estimate the OLS results on samples with each of the instrument being non-missing. Columns 2, 4, 6 estimate the corresponding 2SLS results. The sample focuses on all employees employed with a firm between size 10 and 5000 as of February 2020, and who have left the firm between March 2020 and December 2022. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 7: Spawning by Quality of Started Business

Panel A. Employer

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Dep var:	Spawn as:			
	Employer	Non-employer	Employer	Non-employer
$\Delta RW_{2019 \rightarrow 2020/21}$	0.125***	0.084***	1.808***	1.411***
	(0.017)	(0.017)	(0.530)	(0.444)
Kleibergen-Paap F-stat			35.043	35.043
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	13542997	13542997
R-squared	0.002	0.002	0.000	0.000
% effect	9.6%	6.7%	138.6%	112.6%

Panel B. Has website

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Dep var:	Spawn as:			
	Has website	No website	Has website	No website
$\Delta RW_{2019 \rightarrow 2020/21}$	0.180***	0.028***	2.718***	0.489***
	(0.025)	(0.008)	(0.756)	(0.177)
Kleibergen-Paap F-stat			35.043	35.043
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	13542997	13542997
R-squared	0.003	0.001	0.001	0.000
% effect	8.4%	6.9%	127.0%	117.8%

Panel C. VC-backed

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Dep var:	Spawn as:			
	VC-backed	Non-VC-backed	VC-backed	Non-VC-backed
$\Delta RW_{2019 \rightarrow 2020/21}$	0.039***	0.170***	0.709***	2.498***
	(0.008)	(0.024)	(0.215)	(0.700)
Kleibergen-Paap F-stat			35.043	35.043
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	13542997	13542997
R-squared	0.001	0.003	-0.001	0.001
% effect	24.8%	7.1%	456.3%	104.0%

This table examines the quality of the spawned businesses measured by employment. The specifications follow columns 1 and 2 of Table 3, using the instrument *Commute*. All columns include control variables but are omitted from reporting. The sample consists of all employees employed as of February 2020 with firms of employment size 10 to 5000. The dependent variable in columns 1 and 3 (columns 2 and 4) is 100 times a dummy indicating that the employee started a new employer (non-employer) business between March 2020 and December 2022. We identify employer (non-employer) businesses as those whose maximum employment from entry to December 2022 is positive (zero). % effect in the bottom row indicates the percentage effect of a one-std-dev increase in ΔRW relative to outcome mean. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 8: Heterogeneity by Experimentation Value

	(1)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn into industry with			
	High risk	Low risk	High risk	Low risk
$\Delta RW_{2019\rightarrow 2020/21}$	0.144***	0.042***	1.973***	0.875**
	(0.021)	(0.016)	(0.484)	(0.425)
P-val of diff	0.000		0.089	
Kleibergen-Paap F-stat			35.043	35.043
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	13542997	13542997
R-squared	0.002	0.002	0.000	0.001

This table shows the heterogeneity of our individual-level cross-sectional results with respect to experimentation value of the entry industry. The specifications follow columns 1 and 2 of Table 3. The instrument is *Commute*. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022 in a high-risk vs low-risk industry. *High risk* (*Low risk*) indicates NAICS 3-digit industries with above (below) median exit rates of young (age≤5) firms from 2015 to 2019. The measures is created from U.S. Business Dynamic Statistics (BDS). The sample consists of all employees employed in February 2020 with firms of employment size 10 to 5000. P-value indicates the significance of the coefficient difference between high-risk and low-risk columns. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Appendix Figures and Tables

Table A.1: Top and Bottom Industries by $\Delta RW_{2019\rightarrow 2020/21}$

NAICS 2-digit	Description	Average $\Delta RW_{2019\rightarrow 2020/21}$
Top 5 industries		
51	Information	0.149
81	Other Services (except Public Administration)	0.143
54	Professional, Scientific, and Technical Services	0.141
21	Mining, Quarrying, and Oil and Gas Extraction	0.135
56	Administrative & Support, Waste Management and Remediation Services	0.134
Bottom 5 industries		
55	Management of Companies and Enterprises	0.113
62	Health Care and Social Assistance	0.112
11	Agriculture, Forestry, Fishing and Hunting	0.110
23	Construction	0.109
44	Retail trade	0.106

This table shows the top and bottom 5 NAICS 2-digit industries by the average increase in RW from 2019 to 2020/21.

Table A.2: Cross-Sectional Analysis: Alternative Samples

Panel A. All firms above

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample	Individual-Level		Firm-Level	
Dep var:	$Spawn_{\text{2020-2022}}$		$SpawnShare_{\text{2020-2022}}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.210***	2.441***	0.116***	1.303*
	(0.030)	(0.873)	(0.038)	(0.674)
Kleibergen-Paap F-stat		19.112		298.816
Controls	Yes	Yes	Yes	Yes
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	17491541	17491541	136491	136491
R-squared	0.003	0.540	0.052	0.014

Panel B. Restrict to Firms with Growing Employment

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample	Individual-Level		Firm-Level	
Dep var:	$Spawn_{\text{2020-2022}}$		$SpawnShare_{\text{2020-2022}}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.328***	2.567***	0.175**	2.611**
	(0.053)	(0.870)	(0.081)	(1.074)
Kleibergen-Paap F-stat		25.578		79.864
Controls	Yes	Yes	Yes	Yes
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	4313397	4313397	26984	26984
R-squared	0.004	0.002	0.096	-0.013

This table examines the robustness of our cross-sectional results to alternative samples. The specification follows those in columns 1 and 2 of Tables 3 and 4. The 2SLS specification is based on the instrument *Commute*. Panel A includes all firms with at least 10 employees in February 2020 (including those with more than 5000 employees). Panel B restricts to firms that experienced continued employment growth during Covid. In both panels, columns 1 and 2 present individual-level results based on all Feb2020 employees of these firms, and columns 3 and 4 present collapsed firm-level results. The dependent variable is 100 times a dummy indicating (the fraction of) employee starting a new business between March 2020 and December 2022 in (columns 1 and 2) columns 3 and 4. The key independent variable $\Delta RW_{2019\rightarrow 2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. For brevity, we do not report the coefficients of the control variables. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.3: Cross-Sectional Analysis: Additional Controls

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample	Individual-Level		Firm-Level	
Dep var:	$Spawn_{\text{2020-2022}}$		$SpawnShare_{\text{2020-2022}}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.145***	2.425***	0.102***	1.255*
	(0.025)	(0.743)	(0.038)	(0.685)
RW_{2019}	0.183***	1.474***	0.125***	0.822**
	(0.030)	(0.438)	(0.037)	(0.406)
Ln(emp)	-0.027***	-0.035***	-0.033***	-0.039***
	(0.002)	(0.003)	(0.004)	(0.005)
Firm age	-0.001***	-0.000***	-0.001***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning rate	0.114***	0.106***	0.065***	0.064***
	(0.009)	(0.010)	(0.011)	(0.011)
(Avg.) Tenure	-0.008***	-0.008***	-0.018***	-0.017***
	0.000	0.000	(0.003)	(0.003)
(Avg.) Seniority	0.077***	0.075***	0.244***	0.227***
	(0.005)	(0.005)	(0.020)	(0.024)
(Avg.) Ln(salary)	-0.014**	-0.011*	0.139***	0.175***
	(0.006)	(0.006)	(0.033)	(0.042)
(Avg.) Prior founder	4.386***	4.379***	8.043***	7.924***
	(0.147)	(0.146)	(0.786)	(0.772)
(Avg.) Age	-0.007***	-0.006***	-0.024***	-0.024***
	0.000	0.000	(0.002)	(0.002)
(Avg.) Has grad degree	0.078***	0.077***	0.058	0.050
	(0.009)	(0.009)	(0.064)	(0.062)
(Avg.) Top100 BA	0.079***	0.074***	0.691***	0.633***
	(0.015)	(0.015)	(0.135)	(0.140)
(Avg.) Educ missing	(0.003)	(0.003)	-0.134***	-0.140***
	(0.005)	(0.005)	(0.039)	(0.040)
First-stage IV coeff:				
Commute		0.009***		0.010***
		(0.002)		(0.001)
Kleibergen-Paap F-stat		34.873		295.334
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Job Role FE	Yes	Yes	No	No
Observations	13526705	13526705	136121	136121
R-squared	0.005	0.001	0.055	0.018

This table examines the robustness of our cross-sectional results to including more controls. The sample and specification follow those in columns 1 and 2 of Tables 3 and 4, except that we additionally control for individual's age as of 2020, education (dummy for having a grad degree, dummy for top100 undergrad school, and dummy for missing education info), and role fixed effects. The key independent variable $\Delta RW_{2019\rightarrow 2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.4: Cross-Sectional Analysis: Spawning Before vs After Departure

Panel A. Spawning Before Departing Wage Job

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample	Individual-Level		Firm-Level	
Dep var:	$Spawn\_before_{\text{2020-2022}}$		$SpawnShare\_before_{\text{2020-2022}}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.068***	0.211	0.062***	0.140
	(0.014)	(0.269)	(0.022)	(0.391)
Kleibergen-Paap F-stat		35.043		299.455
Controls	Yes	Yes	Yes	Yes
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	136121	136121
R-squared	0.002	0.002	0.029	0.010

Panel B. Spawning After Departing Wage Job

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample	Individual-Level		Firm-Level	
Dep var:	$Spawn\_after_{\text{2020-2022}}$		$SpawnShare\_after_{\text{2020-2022}}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.140***	3.007***	0.054*	1.168**
	(0.021)	(0.782)	(0.031)	(0.497)
Kleibergen-Paap F-stat		35.043		299.455
Controls	Yes	Yes	Yes	Yes
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	136121	136121
R-squared	0.002	-0.001	0.042	0.004

This table splits the dependent variable in our main analysis by whether spawning happens before (Panel A) or after (Panel B) a worker formally leaves her wage employment job. Both panels follow the sample and specification used in columns 1 and 2 of Tables 3 and 4. The 2SLS specification is based on the instrument *Commute*. The key independent variable $\Delta RW_{2019\rightarrow 2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. For brevity, we do not report the coefficients of the control variables. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.5: Heterogeneity by Change in RW

	(1)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn into industry with			
	Higher RW	Lower RW	Higher RW	Lower RW
	than origin firm			
$\Delta RW_{2019 \rightarrow 2020/21}$	-0.832***	1.005***	0.100	2.657***
	(0.048)	(0.057)	(0.359)	(0.609)
Kleibergen-Paap F-stat			35.043	35.043
Controls	Yes	Yes	Yes	Yes
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	13542997	13542997
R-squared	0.002	0.003	0.001	0.001

This table shows the heterogeneity of our individual-level cross-sectional results with respect to experimentation value of the entry industry. The specifications follow columns 1 and 2 of Table 3. The instrument is *Commute*. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022 in a high-risk vs low-risk industry. *High risk* (*Low risk*) indicates NAICS 3-digit industries with above (below) median exit rates of young (age$\leq$5) firms from 2015 to 2019. The measures is created from U.S. Business Dynamic Statistics (BDS). The sample consists of all employees employed in February 2020 with firms of employment size 10 to 5000. P-value indicates the significance of the coefficient difference between high-risk and low-risk columns. Standard errors are reported in parentheses and are clustered at the NAICS 4-digit level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.