Entrepreneurial Spawning from Remote Work

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

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Abstract

This paper shows that remote work increases wage workers' transition into entrepreneurship. Using big data on Internet activities, we create a novel firm-level measure of remote work. We show that firms with greater increases in remote work during the pandemic are more likely to see their employees subsequently becoming entrepreneurs. This holds both unconditionally and relative to other types of 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 forced turnover. Rather, remote work increases spawning by providing the time and downside protection needed for entrepreneurial experimentation. We calibrate that at least 13.4% of the post-pandemic increase in new firm entry can be attributed to spawning from remote work.

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*Kwan: University of Hong Kong, apkwan@hku.hk; Matthies: University of Notre Dame, bmatthie@nd.edu; Townsend: University of California San Diego, rrtownsend@ucsd.edu; Xu: University of Toronto, tingxu.xu@rotman.utoronto.ca (corresponding author). We thank Jose Maria Barrero, Toomas Laarits, Adair Morse, Kaushik Vasudevan and seminar participants at Notre Dame, KAIST/Korea University, and HKU (Applied Econ) 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.

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). There is also growing concern that remote work may inhibit innovation and idea generation (Brucks and Levav, 2022; Lin et al., 2023; Chen et al., 2022). As companies and policy makers continue to evaluate the merits of remote work, its spillover effect on entrepreneurship could be an important consideration in their cost-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. Yet most available measures are survey-based and only cover a limited sample of firms. Second, we need to observe worker-level transitions into entrepreneurship and worker-firm matches. Finally, variation in remote work policies across firms is not random. Firms that adopt more remote work friendly policies may also have employees who are more entrepreneurial, making it difficult to establish causality.

To overcome the measurement challenge, we exploit big data on Internet activities to create a firm-level measure of remote work. The data allows us to classify IP addresses and track anonymized individuals across their workplace, home, and mobile devices. It also links individuals to their employers. We aggregate this data across all employees of a firm to obtain a firm-month-level measure of remote work—the percentage of employee internet activities belonging to a remote IP address.[1] By measuring firm-wide remote work rather than an individual's take-up of remote work,

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

we mitigate individual-level endogeneity, exploiting the fact that individuals have limited influence over firm-wide policies. As such, we estimate an "intent-to-treat" effect.

We then link our firm-level remote work measure to employer-employee matched data from LinkedIn, available through Revelio Labs. The LinkedIn data contain 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 issues from stale CVs on LinkedIn, we track spawning activities up till December 2022, even though our LinkedIn data end in October 2023.[2] Although not all workers on are LinkedIn, our data capture the set of knowledge workers at risk of becoming an entrepreneur—the same set that is also well covered by our Internet activity data.

Our baseline cross-sectional analysis focuses on U.S. firms with at least 10 employees in February 2020 ("Feb2020 firms"), the month before COVID, and all their employees as of then ("Feb2020 employees").[3] 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 to December 2022. We conduct this analysis at both the individual and firm level. Our baseline OLS estimates show that a one standard deviation increase in remote work is associated with a 8% higher likelihood of entrepreneurial spawning at the individual level, and a 4% higher spawning share among employees 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 change. First, we control for a host of firm and employee characteristics in our baseline specification, including firms' pre-pandemic remote work and spawning share, as well as workers' past founder experience. These controls help absorb ex-ante entrepreneurial tendencies at both the firm and worker level.

Second, we use instrumental variables to isolate exogenous variation in remote work. Our primary instrument is the average commute distance of a firm's employees 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 will be more likely to adopt remote work during and post pandemic. We posit that, within a given county (captured by county fixed effects), variation in commute distance is idiosyncratic and largely predetermined before the pandemic, thus providing an exogenous source of variation in remote work tendencies. We also use local business closure orders issued during the pandemic as an alternative instrument. We verify that these instruments have no direct correlation with firms' pre-pandemic spawning in the cross-section, and that firms

[2]We are currently updating our data to late 2024.

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

sorted by these instruments trended similarly before the pandemic. Our preferred 2SLS estimate shows that a one standard deviation increase in a firm's remote work increases the spawning share among its employees by 45% relative to the mean.

Third, we use a dynamic difference-in-differences to compare changes in firms' spawning rates around the pandemic across firms with different tendencies to adopt remote work after 2020, as proxied by our instruments. We employ two samples for this analysis. The all-employees sample tracks the spawning activities of all employees of Feb2020 firms from 2016 to 2022. This sample allows for employee compositional changes, hence allowing for an effect through employee selection. Our second sample, the fixed-employees sample, tracks a fixed set of employees employed in February 2020 over time, regardless of whether they were with their Feb2020 employer in a year. This sample shuts down employee recomposition and hence the possibility that our results are driven by selection—e.g., remote work attracting more entrepreneurial employees or driving away less entrepreneurial ones. Both samples generate results consistent with our cross-sectional results, but the fixed-employees sample yields larger estimates, suggesting that the selection effect is likely negative. We also use the fixed employees sample to examine one's "entrepreneur status", i.e., whether spawned individuals remain in entrepreneurship in addition to the transition itself. This allows us to test if spawned individuals quickly returned to wage employment. We find a persistent and increasing effect on this outcome from 2020 to 2022, suggesting that the spawned entrepreneurship is not transient.

Our baseline 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 individual-level results being skewed by the largest firms. We show that 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 similar when restricting to spawnings that happened after an individual left her wage employer, suggesting remote work is not only spawning part-time, side entrepreneurship. Finally, using Census firm entry data, we show that our micro-level evidence also holds at the aggregate level: industries or locations with more remotable jobs saw greater new firm entry post the pandemic. This aggregate-level analysis mitigates any concerns with our LinkedIn- and Internet-based measures.

To what extent are our findings unique to entrepreneurship? It is possible that forces that drive remote workers into 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 who left their Feb2020 employer between March 2020 and December 2022. Examining entrepreneurial spawning among this sample thus tests whether remote work *disproportionately* directs workers to entrepreneurship relative to other labor destinations. We find similar results with this conditional analysis. A one standard deviation increase in remote work increases spawning into entrepreneurship relative to other job turnovers 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 uniquely shifts workers toward entrepreneurship. The conditional analysis also rules out any remaining concerns about truncation bias from stale CVs, 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 such a preference drives our results, we should expect the marginal entrepreneurs to concentrate in low-growth, flexibility-based entrepreneurship, such as self-employment. However, we find that the marginally spawned firm is more likely to be an employer, have a website, and receive subsequent VC funding than the average new firm. This suggests that the marginal entrepreneur tends to be high-quality. This result echoes our prior finding of a stronger spawning response among better educated workers. Alternatively, prolonged exposure to remote work can induce a yearning for in-person interactions, motivating workers to start their own business to fulfill their social needs. This should predict that the marginally spawned firm is more likely to operate in person. However, we find no such evidence: marginally spawned firms are just likely to have high levels of remote work as low levels.

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 (Agrawal et al., 2018). 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 fallback option when exploring entrepreneurship (Gottlieb et al., 2022). If this channel is at work, we should see stronger marginal entry into industries where experimentation is more valuable, such as those with a higher risk of failure. We indeed find such heterogeneity when splitting spawning by the average young firm failure rate in the destination industry. Further, if remote work enables experimentation by relaxing time constraint, we should expect our results to concentrate where such constraint is more binding: e.g., high-growth entrepreneurship that is more time-consuming; our prior result on entrepreneurship quality supports this prediction. Exploiting variation in slack time from child care, we further show that the spawning response is stronger when local K12 schools adopted more in-person learning during the pandemic, which gave remote work parents more time to experiment with entrepreneurship.

Finally, our results could reflect forced entrepreneurship, where remote work leads to layoffs or involuntary departures, and these workers subsequently start a business out of necessity. We rule this out by showing that our findings are similar when restricting to firms that experienced continued employment growth from 2020 to 2022, i.e., those unlikely to have had mass layoffs. Our analysis conditional on turnovers also rules out this channel, since layoffs should not trigger entrepreneurship more than other types of turnovers, 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 13.4% of the post-pandemic increase in new firm entry can be explained by spawning from remote work. Of course, there could by other channels through which remote work impacts entrepreneurship at the aggregate level, such as investment opportunities or local agglomeration. Nevertheless, our paper uncovers a novel link between two important macro phenomena post pandemic: the rise of remote work and increases in business entry.

Our paper adds to the fast-growing literature on remote work (see Barrero et al. (2023) for a review). 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 for hybrid arrangement, but mixed for fully remote (Bloom et al., 2015, 2024; Kwan et al., 2023; Emanuel and Harrington, 2023; Gibbs et al., 2023; Duchin and Sosyura, 2021; Flynn et al., 2024). At the same time, impact on innovation seems to be more negative (Brucks and Levav, 2022; Lin et al., 2023; Chen et al., 2022). 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 surplus from employees' spawning, firms may under-invest in remote work. Policy makers should take such spillover effects into account when designing future labor 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 incumbent firms. Gompers et al. (2005) show that the most prolific spawners are originally VC-backed firms. Hacamo and Kleiner (2022) and Bernstein et al. (2024) examine how economic cycles affect individuals' decision to become an entrepreneur or work for a startup firm. Hombert et al. (2020) 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".

Finally, our study has implication for the spatial distribution of entrepreneurship and innovation, which is known to be highly concentrated in a few hub cities (Feldman, 2013; Florida and Mellander, 2016). Because remote work removes geography barriers, it results in a more dispersed geographic distribution of workers (Brueckner et al., 2023; Ramani and Bloom, 2021) and, by our paper, distribution of startups spawned by them. As such, remote work could be an important policy tool to reduce spatial inequalities in entrepreneurship and economic activities (Glaeser and Hausman, 2020).

2 Data, Measures, and Samples

2.1 Data

Internet Activity Data. We create our firm-level remote work measure from novel Internet activity data. The data consist of individual-level Internet activities, including the user, the IP address, and timestamps of access. The data also include information about each user such as device type, approximate latitude and longitude when accessing the Internet, and the company they worked for. The data captures a substantial fraction of Internet activities, 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, focusing 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 Partner's 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. Overall, the platform aggregates around 1 billion content consumption events per day. From this dataset, the Data Partner performs two steps: (1) associating visitors with the companies they work for, when possible, and (2) quantifying the "topics" of the content visitors read about.[4] Our access to the data is designed to take special care with respect to confidentiality restrictions—while we observe browsing activity at the event level, we do not know the identity of any individual persons in the data. We only leverage the IP address and timestamp to form an association between a company and a rough location.

To associate users 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. If a profile consistently logs onto a publisher website from a work-associated IP address, this gives a strong indication that 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 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 activities 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 linking individuals to employers or classifying IP addresses can be mitigated. We also impose sample filters to reduce noises in using the data to measure remote work (see Section 2.2).

Employer-Employee Matched Data. We obtain employer-employee matched data from Revelio Labs, which is underlied by LinkedIn data. Our data consist of the universe of LinkedIn users, their CVs, and their employer profile pages up to October 2023. The CV data include each individual's job history, education, skills, and demographics, among others. Revelio/LinkedIn is used by many economics and finance 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

[4] From these two steps, the Data Partner produces analytics that 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 potential customers. Participating publishers receive some of the data analytics in return for providing the data.

(2024), Bernstein et al. (2024)). This data also give us 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 *at risk of* spawning. These tend to be knowledge workers or younger workers, who are well captured by LinkedIn. Additionally, workers on LinkedIn overlap 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 give us additional information on firms' NAICS, location, founding year, domain, etc.

US Census Aggregate-Level Data. Finally, we use industry- or county-level new firm entry data from Business Dynamic Statistics (BDS) and new firm job creation data from Quarterly Workforce Indicators (QWI) to verify our micro-level results at the aggregate level.

2.2 Key Measures

Remote Work Measure. Our firm-level remote work measure, RW, comes from Kwan et al. (2023). The measure 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 categories: 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) provide 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 (Mon-Fri 9am-5pm) that is originating from a remote IP. We define a remote IP as a residential, VPN, 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.[5] Our RW

[5]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, where an observation is a session-timestamp

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. Kwan et al. (2023) perform a variety of validation tests of this measure. 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 job remotability measure from Dingel and Neiman (2020). We refer readers to Kwan et al. (2023) for validation details.

Figure 1 shows the time-series of monthly RW averaged across firms in our sample. Remote work increased sharply at the start of the pandemic and stayed elevated, with a slight decline in 2021. Relative to the survey-based measure from Barrero et al. (2023), we see less of a reversal in 2021. This likely reflects the fact that, among the population of knowledge workers we capture, remote work is more persistent, whereas Barrero et al. (2023) survey the general population.[6]

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. For example, the value of RW is not close to zero pre-pandemic in Figure 1 (surveyed remote work share was 7% in 2019 (Bloom et al., 2024)), suggesting there is a baseline level of non-office IP activities even without remote work. For these reasons, we 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.[7] Specifically, we measure changes in a firm's RW from 2019 to 2020/2021. We define $\Delta RW_{f,2019\rightarrow2020/21} = 0.5(RW_{f,2020} + RW_{f,2021}) - RW_{f,2019}$, i.e., the increase from 2019 to 2020/2021 average. Table A.1 shows the top and bottom industries by $\Delta RW_{f,2019\rightarrow2020/21}$. 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 "take-up", and firm-level RW as "intent-to-treat". Given a firm's RW policy, an individual employee's decision to take up is obviously more endogenous, as it is driven by the person's expected costs and benefits of take-up which could correlate with her entrepreneurial tendencies.

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.

[6]Specifically, their sample is drawn from all working age workers in the US with at least $20,000 earnings in 2019 (later revised to $10,000).

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

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

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 (reported by LinkedIn)
4. The job title contains "founder" (including "co-founder"), "founding", "owner", or "entrepreneur". In the case no employees of the firm has any 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 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 spawnings from the Feb2020 firms from March 2020 to December 2022. For our firm-year-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.[8] This also makes sure we capture entrepreneurial spawning from relatively established, "incumbent" firms. Our individual-level cross-sectional sample consists of all US-based employees of these Feb2020 firms 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.

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

We then extend the cross-sectional samples into two firm-year-level panels covering the period of 2016 to 2022. The first panel tracks Feb2020 firms over time and include all their employees, not just those employed in February 2020. We call this sample "all-employees panel". The second panel tracks the Feb2020 employees over time regardless of whether they were with the Feb2020 firm in a given year. We call this sample "fixed-employees 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 from March 2020 to Dec 2022 was 0.34% across all employees. At the firm-level, the average spawning share over the same period was 0.43%. The difference reflects the fact that larger firms tend to have lower spawning rates (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-year-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 over 3 years, 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 their Feb2020 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 Feb2020 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 the Feb2020 firm's 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, log salary, as well as an indicator for past founder experience, all measured as of February 2020. $\boldsymbol{X_f}$ 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, controlling for individuals' past founder experience and firms' past spawning rate help absorb latent spawning factors at 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. Additionally, our subsequent firm-level analysis addresses this concern by weighting each firm equally. To mitigate potential measurement errors in remote work, we restrict to firms with more than 10 employees. We show robustness to relaxing these 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 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 firm employees as of February 2020, as well as their average past founder experience, in addition to the ones specified in Equation 1. Specifically, we estimate the following firm-level specification:

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

2SLS. We also estimated a 2SLS version of both the individual-level and firm-level regressions, instrumenting $\Delta RW_{f,2019\rightarrow 2020/21}$ with two instruments.

Our primary 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 instrument 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 mainly in Connecticut with a two hour daily commute and the other's live mainly 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, which should also persist longer into/after the pandemic. We posit that, within the same city, variation in commute distance across firms is idiosyncratic and largely predetermined before the COVID 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.[9] 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 then calculate the average commute distance within the middle 50 percentile of the distribution to obtain $Commute_i$, the average commute distance of employees at firm i.[10] 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 controls for pre-pandemic firm size and remote work, which capture the differential ability to remote work prior to the pandemic, as well as firms' pre-pandemic spawning rate and workers' prior founder experience, which capture employees' underlying entrepreneurial tendencies. These controls greatly reduces the residual correlation between commute distance and unobserved firm types that is not going through remote work.

Our alternative instrument is based on county-level business closure orders issued 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 when businesses were required to be closed in a county, taking into account both full and partial closures. Specifically, Spiegel and Tookes

[9] 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.

[10] This removes the two tails of the distribution, which tend to be ephemeral or outlying sessions that are not accurately measured.

(2021) categorize 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 each of these levels 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 at the county level and requires us to drop county fixed effects.

For our 2SLS to estimate a valid local average treatment effect (LATE), our instruments need to satisfy three assumptions: 1) relevance, 2) monotonicity, and 3) exclusion. We present evidence supporting each assumption in Section 4.1.

3.2 Firm Panel Analysis

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

$$SpawnShare_{f,t} = \alpha_f + \beta_t + \theta \times Commute_{f,2019} \times Post2020_t + \epsilon_{f,t} \tag{3}$$

, where f indicates the Feb2020 firm, $Post2020_t$ indicates years$\geq$ 2020, and $Commute_{f,2019}$ is our continuous commute distance instrument. We standardize $Commute_{f,2019}$ by removing mean and dividing by standard deviation, so that we can interpret a one-standard-deviation change effect. We also use our other instrument $BizClose$ as an alternative treatment variable. As such, this specification estimates the reduced form effects of our instruments, in a generalized difference-in-differences. 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. The sample period is 2016 to 2022, with 2019 as the omitted base year.

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

$$SpawnShare_{f,t} = \alpha_f + \beta_t + \sum_{t \neq 2019}^{2016 \to 2022} \theta_t \times Commute_{f,2019} \times \mathbb{1}(Year = t) + \epsilon_{f,t} \tag{4}$$

This specification will also tests whether the identifying assumption that firms with different levels of $Commute_{f,2019}$ or $BizClose$ trended similarly before 2020 is likely to hold.

Fixing Employers but not Employees. Our first approach focuses on a fixed set of firms with employees in February 2020. We then track all their employees from 2016 to 2022 and their spawning events from the Feb2020 firm. This approach allows for compositional changes in employees. Specifically, we obtain a sample of individual-years based on all employment spells with the Feb2020 firms from 2016 to 2022. We define spawning year as the minimum of the new business start year and job end year.[11] We then collapse this panel to the Feb2020-firm-year level. We estimate the specifications in Equations 3 and 4, with the dependent variable $SpawnShare_{f,t}$ being the share of employees spawned from the Feb2020 firm each year. 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 direction: firms with generous remote work policies retain employees who prefer flexibility or a quiet life, while firms quickly reverting back to in-person lose such type of employees, who then start a new business for flexibility reasons.

Fixing Employees but not Employers. Our second approach focuses on a fixed set of employees employed as of February 2020. We then track their entrepreneurial spawning from 2016 to 2022, regardless of whether they were still with the Feb2020 employer. Hence, different from the first approach, we track individuals' spawnings not just from the Feb2020 firm, but from any firm there were employed with in a year. We then link all these spawning events 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, this approach effectively removes individuals' selection into Feb2020 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 implement this, we construct a balanced panel of individual-years for the Feb2020 employees from 2016 to 2022. We then collapse this panel to the Feb2020-firm-year level. We estimate the same specifications as Equations 3 and 4, except that the dependent variable is the share of the Feb2020 employees that started a new business in a year regardless which employer they were with in that year. We refer to this sample as the *fixed employees sample*.

One concern with tracking a fixed set of individuals over time is that spawning events may be mean-reverting. If an individual just left wage employment to start her new firm, it will be hard to observe another spawning event immediately after, given that she needs to switch back

[11] 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. Due to our yearly panel nature, we do not track businesses started more a year away from the job end year.

to wage employment before she can spawn again. In other words, within a short period of time, most individuals can only spawn once. Another concern is that spawning event itself does not tell us how persistent the effect is, i.e., how long the individual stays in entrepreneurship after spawning. It is possible that spawned individuals quickly reverted back to wage employment, either because the started business quickly failed, or because it was a temporary arrangement when individuals were between jobs. In either case, this would suggest remote work only spawns low-quality entrepreneurship.

To mitigate these concerns, we examine founder status as an alternative outcome in our dynamic analysis on the fixed employees sample. We define an individual as a founder as long as she is in entrepreneurship in a given year, regardless of whether this is the first or second business she founded after transitioning to entrepreneurship, or whether this is the second time she spawns after returning to wage employment. We then similarly collapse the individual-year level panel to the Feb2020-firm-year level to define $FounderShare$. This alternative outcome is not subject to mean reversion. Additionally, it captures how long one stays in entrepreneurship, either continuously or in different episodes, after being exposed to remote work in their initial wage job. We estimate the same specifications as Equations 3 and 4, but replacing the $SpawnShare_{f,t}$ with $FounderShare_{f,t}$:

$$FounderShare_{f,t} = \alpha_f + \beta_t + \theta \times Commute_{f,2019} \times Post2020_t + \epsilon_{f,t} \tag{5}$$

$$FounderShare_{f,t} = \alpha_f + \beta_t + \sum_{t \neq 2019}^{2016 \to 2022} \theta_t \times Commute_{f,2019} \times \mathbb{1}(Year = t) + \epsilon_{f,t} \tag{6}$$

4 Main Results

4.1 Cross-Sectional Results

Table 3 presents the cross-sectional result. Column 1 reports the individual-level OLS result based on Equation 1. We multiply the dependent variable by 100 to display meaningful decimal points. 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 standard deviation increase in ΔRW increases worker-level spawning likelihood 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)).

Next we turn to instrumental variable results. Our primary instrument is firm-level average commute distance before COVID. Column 1 of Table A.2 shows the first-stage results at the individual level. The instrument is strong, with a F-stat of 35. Specifically, a one standard deviation increase in $Commute$ increases ΔRW by 4% relative to the mean. Larger, younger firms have higher ΔRW during COVID, so are firms with higher RW or higher spawning rate pre-pandemic. Firms whose employees have on average shorter tenure, higher seniority, lower salary, or more past founder experience are also more likely to experience higher ΔRW during COVID. Column 2 of Table 3 shows the individual-level 2SLS result. Based on the instrumented coefficient, a one standard deviation increase in ΔRW increases worker-level spawning likelihood by 120%, a large effect. Other control variables retain similar coefficients as OLS results.

Columns 3 and 4 show 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 share of employees (in percentage points) 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. A one standard deviation increase in ΔRW increases firm-level spawning share by 4% under OLS, and by 45% under 2SLS. Importantly, the F-stat of $Commute$ is much higher at the firm-level (299) than at the individual-level, as the individual-level sample skews towards larger firms. In the first stage, a one standard deviation increase in $Commute$ increases ΔRW by 6% of mean.

Our estimated 2SLS effects are much larger than their OLS counterparts. However, their ratios are within the range surveyed by Jiang (2017) from the literature. The large 2SLS effects cannot be driven by weak instrument, as our first-stage F-stats are high. Instead, it could be explained by the presence of confounders that bias OLS estimate downward relative to 2SLS. For example, companies with greater RW increase during COVID may also offer better non-wage amenities or job flexibility, hence better retaining their employees and reducing spawning. Another explanation is that our 2SLS estimates a local average treatment effect (LATE), which can be higher than the average treatment effect (ATE). This means that compilers have a stronger response to RW than the average firm. In our setting, compilers are firms that listen to their employees and tailor RW policies to their commuting needs. Such firms may also monitor their employees less closely once remote, and their employees likely have strong bargaining power (due to their skills or human capital intensive nature of business), both of which could lead to higher spawning sensitivity to RW.

Table 4 shows that we find qualitatively similar results with our alternative instrument, county-level business closure measures during COVID. Note that this instrument requires us to drop county fixed effects, which could explain the larger magnitude of the estimates. The *BizClose* instrument is strong at the firm-level (F-stat=84.8), while almost reaching the conventional F-stat threshold of 10 at the individual level (F-stat=9.4). Columns 2 and 4 of Table A.2 present the full first stage results.

Although we cannot rule out that confounders could still exist that violate the exclusion condition of our instruments, the consistent results across our two instruments after conditioning on a variety of controls greatly reduces such remaining concerns. We present more evidence on the validity of our instruments next.

Instrument Validity For our 2SLS to estimate a valid local average treatment effect (LATE), our instrument needs to satisfy three conditions: 1) relevance, 2) monotonicity, and 3) exclusion.[12] We have shown that our instruments have a strong first-stage effect, with F-stats well above the conventional rule of thumb of 10.

The monotonicity condition implies that there are no "defiers" in our sample, i.e., firms that adopt *lower* levels of remote work during pandemic in response to higher employee commute distance. We test this following the methodology in Dobbie et al. (2018) (also used in González-Uribe and Reyes (2021) and Bias and Ljungqvist (2023)). The intuition is that monotonicity implies that the first-stage coefficient on the instrument should be non-negative in all subsamples formed based on observables. We test this implication in a variety of subsamples split by different firm characteristics. Table A.3 presents the result. We find that, across all subsamples, our instruments exhibit significant and strong first-stage coefficients, lending support to the monotonicity assumption.

The exclusion condition is ultimately untestable. Nevertheless, we provide some evidence consistent with this condition is likely to hold in our sample. Table A.4 shows that our two instruments have no significant correlation with firms' pre-pandemic spawning share or the share of employees with past founder experience. This suggests that, conditional on our controls, the instruments have no detectable correlations with unobservables that affect a firm's ex-ante spawning tendencies. This helps rule out the possibility that our instruments directly impact post-pandemic spawnings through their correlation with ex-ante unobservables, such as corporate culture or employee types, etc.[13] Note that even if there is a correlation that we failed to detect in Table A.4, such effects will still be controlled for as we control for pre-pandemic firm spawning rate and employee past founder

[12] Note that if the monotonicity assumption is violated, our 2SLS estimates would still be a weighted average of marginal treatment effects, but the weights would not sum to one (Angrist et al., 1996; Heckman and Vytlacil, 2005).

[13] Our subsequent panel analysis also rules out endogeneity from unobserved employee types by holding employee composition constant.

experience in our analysis. Furthermore, our subsequent panel DID analysis shows that firms with differently levels of *Commute* or *BizClose* trended similarly before the pandemic. Finally, to the extent that the exclusion restriction is violated, our reduced form estimates can still be interpreted as the causal impact of commute distance (or local business closure orders) on post-pandemic spawning. These reduced form results are available in Table A.5.

4.2 Firm Panel Results

We then turn to dynamic evidence estimated from firm-level panels. Table 5 shows the firm-level DID results estimated following Equations 3 and 5. Columns 1 and 2 show the results on the all employees sample, where we allow for employee recomposition. Specifically, we track the spawning activities by *all* employees from the Feb2020 firms, regardless of whether these employees joined these firms before or after February 2020. The dependent variable is the spawning share multiplied by 100. Different from the fixed employees sample (columns 3 to 6), this sample allows for the possibility that employees' selection on firm's remote work policies drives some of our results.

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 proxy for increases in remote work through *Commute* or *BizClose*.[14] In particular, firms with a one standard deviation higher *Commute* experienced a 6% increase in spawning rate post 2020 relative to pre 2020. This effect is 7.5% when we proxy for RW increase with *BizClose*.

Columns 3 to 6 present the results on the fixed employees sample. We focus on a fixed set of individuals employed as of February 2020, and link their Feb2020 employers' RW policies to these individuals' spawning activities over time, regardless of which employer they were with. We estimate this on a balanced panel of Feb2020 firms from 2016 to 2022. Columns 3 and 4 show similar results as columns 1 and 2. Individuals who experienced greater increase in RW with their Feb2020 employers are more likely to spawn into entrepreneurship post Covid. In particular, those whose Feb2020 employer had a one standard deviation higher *Commute* are 10% more likely to spawn post-2020 than pre-2020. This effect is 11% when we use *BizClose* to proxy for remote work increase. 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. The larger effects relative to columns 1 and 2 suggest that employee selection induces a negative bias in our results: increases in remote work tend to attract employees that were less

[14]We also find similar result using industry-level remotability (Dingel and Neiman, 2020) as a treatment—the extent to which jobs in an industry can be done remotely—as an alternative treatment.

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.

Columns 5 and 6 of Table 5 also focus on the fixed employees sample but examine employees' founder status, i.e., whether they stay in entrepreneurship after spawning. The specification follows Equation 5. The dependent variable is 100 times the fraction of individuals that were in entrepreneurship in a given year. Unlike spawning events, founder status is not mean reverting, and captures how long one stays in entrepreneurship in addition to the switch. We continue to find a positive effect. Those whose Feb2020 employer had a one standard deviation higher *Commute* (*BizClose*) were 7.2% (10.4%) more likely to be an entrepreneur post-2020, relative to the mean.

We visualize the dynamics of the above DID results in Figures 4 and 5. Figure 4 focuses on the all employees sample and estimates Equation 4. It shows a significant increase in spawning rate after the start of pandemic for firms with higher *Commute* in 2019. The effect was strongest in 2020, and declined a bit in 2021 and 2022. Importantly, firms with different levels of pre-pandemic commute distance trended similarly before the pandemic, lending support to the parallel trends assumption. We observe similar effects in Figure A.2 Panel A when using *BizClose* as an alternative treatment variable, though the effect is more diminished for 2022. Figure 5 shows the dynamics for the fixed employees sample, removed of employee selection effects. In Panel A, we find a stronger and more persistent effect on spawning rate relative to Figure 4, suggesting the declining effects in 2021/22 in Figure 4 were driven by stronger selection effects in those two years, when employee turnovers were higher. Panel B examines employees' founder status and estimates Equation6. We find a steadily increasing effect from 2020 to 2022. This reflects the cumulation of the yearly spawning effects in Panel A. It also suggests that the newly spawned individuals did not quickly return to wage employment; rather, they stayed in entrepreneurship, leading to persistent effect of remote work on entrepreneurship. We find similar results in Panels B and C of Figure A.2 with *BizClose*.

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.7 shows the results for all firms above 10 employees.

Additional controls. Table A.6 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 Times higher education ranking. We dummy out individuals without any education information. Table A.6 shows that the results remain similar.

Spawning before vs after departing wage job. About one-third of the spawning events in our sample happened before the worker formally left her wage employment job. This can occur either because side entrepreneurship was 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., part-time entrepreneurship while one holds a full-time job, it may call into question the quality of the spawned businesses, as well as whether there is any career risk associated with transitioning to entrepreneurship from wage employment. To check this, we split our dependent variable by whether a spawning event happens before or after one formally departs the wage job, and rerun our main analysis in Table A.8. We find that the response is stronger for spawnings that happened after quitting than those happened while employed. Although experimentation with entrepreneurship could start well 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 a top school BA (Panel B). These results are consistent with the finding in Bernstein et al. (2022), who show that young and skilled individuals are most responsive to local entrepreneurial opportunities in launching new firms. Interestingly, there is a non-linear heterogeneity with seniority (Panel C). Medium-ranked employees are least responsive to remote work, while lower-ranked employees have a stronger responses. However, the most senior employees, those at the executive level, have the strongest response, being 3 to 4 times more responsive than all other groups.[15]

Figure 3 explores heterogeneity across firm types. We find that, conditional on a firm's remote

[15] 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.

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 increases than employees of older firms. This finding is consistent with the "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 show here that, not only are young firms more likely to have a higher baseline spawning rate, their spawning rate is also more sensitive to remote work arrangements.

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 drive 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 destinations. 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 effect reflects mechanisms unique to entrepreneurship.

This conditional analysis has two additional benefits. First, by conditioning on observing a turnover, i.e., the individual updating her CV on LinkedIn, it addresses any concerns about truncation issues or stale CVs in LinkedIn data. Second, this analysis helps to rule out the interpretation that our results are driven by forced entrepreneurship, where remote work increases entrepreneurship by increasing layoff or involuntary departures, and these workers subsequently found a business out of necessity. This is because these forced turnovers should not disproportionately flow into entrepreneurship more than it flows into unemployment or other wage employment, particularly during an economic downturn (Pugsley and Èahin, 2019).[16]

Table 6 presents the result of this conditional analysis. We find that, conditional on leaving their Feb2020 wage jobs, worker who were more exposed to remote work were more likely to pursue entrepreneurship relative to being unemployed or wage employed with another firm. This effect holds both in OLS and in 2SLS. For example, based on Columns 1 and 2, a one standard deviation increase in ΔRW increases departures to 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

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

explore this more in the next section.

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.[17]

We further directly examine the quality of the marginally spawned new businesses. To this end

[17]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.

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 VC backing.[18] 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 standard deviation 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 for VC-backed firms than non-VC backed firms, with the former being 3 to 4 times larger (Panel C). 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. These results are also consistent with our finding in Figure 5 Panel B that the spawned individuals stayed in entrepreneurship rather than quickly failed.

An alternative explanation is that workers, missing in-person social interactions after prolonged exposure to remote work, start their own businesses to fulfill their social needs. This should predict that the marginally spawned business is more likely to have a low as opposed to high level of RW. To test this, we split the spawning outcome by whether the spawned firm had an above- or below-median RW within 2 years of spawning. Due to their nascency, we do not observe RW for many spawned firms. Hence, we do this split only within the set of spawned firms for which can reliably measure RW.[19] Table A.9 shows the results. We find that, across both OLS and 2SLS, the marginal spawning effect is similar for high- vs low-RW firms, with no statistically significant difference. As such, a preference for in-person interactions is unlikely to explain our results.

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.

[18]We define initial employment as the maximum employment in the initial two years of a business' life. We observe a firm's website from its LinkedIn page and restrict to independent business sites 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.

[19]Presumably, the ones with misssing RW are likely firms with few or no employees. These firms should go against the story that entrepreneurs found them to socialize with people.

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.[20] Remote 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.[21] 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.

To provide further direct evidence on the slack time channel, we exploit variation in local schools' learning model during the pandemic to generate variation in slack time for workers who are parents. Parents whose children's school was remote/hybrid for longer during the pandemic would

[20]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).

[21]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.

have less time for entrepreneurial experimentation if working from home. We obtain data on K12 schools' learning model during pandemic from `www.covidschooldatahub.com`, which provides the fraction of time each school was in-person, hybrid, or virtual during 2020-2021, as well as school's location. We collapse this data to the county level to obtain the fraction of time local schools were in person and use it as an interaction variable in our baseline individual-level analysis. Table A.10 shows the results. We find that workers of age 25-50, i.e, those most likely to have school-age children, had a stronger spawning response if their local schools where in-person more during the pandemic. This is consistent with in-person schooling frees up more time for remote work parents. In contrast, we do not find such an interaction effect for workers outside of the 25-50 age range, who were unlikely to have school-age children at home.[22]

Forced entrepreneurship. One last possibility is that remote work induces forced entrepreneurship by triggering layoffs or involuntary turnovers. For example, firms that increased RW more may 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 workers back to office in 2022 (Barrero et al., 2021a), and these quitted workers later started a business. To the extent that this quitting is driven by a preference for flexibility or "quiet life", it amounts to the preference channel, which we ruled out above. Note that this story also implies that spawning should happen with a substantial delay after high-RW firms started to revert to in-person. The immediate response we see in 2020 goes 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 were unlikely to have had mass layoffs during COVID. Panel B of Table A.7 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 turnovers into 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 destinations such as unemployment or other wage jobs.

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. Preference change, employee selection, and forced turnovers cannot explain our results.

[22]We do not present the IV results as our instruments are not strong enough for the interaction term.

7 Aggregate Effects

7.1 Industry/County-Level Analysis

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.[23] 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 specification at the NAICS 3digit-year level:

$$Ln(new\ firms)_{i,t} = \alpha_i + \beta_t + \sum_{t \neq 2019}^{2016 \to 2022} \theta_t \times Remotability_i \times \mathbb{1}(Year = t) + \epsilon_{i,t},$$

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). The latest BDS stops at 2022. *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 from 2020 to 2022, 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.9 that spawning does not flow into industries with higher levels of remote work than the prior employer.

[23] 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.

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.[24] 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:

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

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.

We use two versions of treatment. First, we use our RW measure aggregated to county-industry level.[25] 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 A.3 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).

7.2 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 7.3%

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

[25] 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.

increase in spawning rate based on our cross-sectional estimate in column 1 of Table 3. There were about 130 million full-time employed individuals in the US before COVID. The average quarterly number of high-propensity (HP) new business applications increased from 320K pre-COVID to 430K post Covid (Figure 9 of Decker and Haltiwanger (2023)).[26] Based on Current Population Survey (CPS) data, 63% entrepreneurs come from wage employment pre-Covid.[27] Thus, the implied annual spawning rate is 0.62% (=320*4*0.63/130000). A 7.3% RW-induced increase in this spawning rate would imply 58.8K (=0.62%*7.3%*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 13.4% of this increase.

7.3 Allocative Implications

We cannot direct comment on the distributional effects of remote work-induced spawning. On one hand, spawnings from remote work could simply reflect a reallocation of innovative activities that would have happened in-house, in which case there will be no changes in aggregate output. On the other hand, it is possible that the spawned entrepreneurs pursue innovations that would not have happened inside the prior employer, due to frictions in innovation incentives. Indeed, we show in Table **??** that there is a significant spawning effect even outside the industry of the spawning firm. It is also possible that remote work relaxes constraints on employees ability to explore their outside option in entrepreneurship, leading to better matches. In these cases, spawnings could reflect more efficient reallocations. Regardless, the impact on incumbent firms is likely to be negative if spawning destroys firm-specific knowledge, or/and if firms cannot adequately share-in the surplus released by spawning (Ma et al., 2023). We leave such allocative effects to future research.

8 Conclusion

The majority of entrepreneurs are prior wage workers. How work is organized in wage employment therefore impacts workers' decision to become an entrepreneur. This paper shows that the 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

[26]Decker and Haltiwanger (2023) define high-propensity new business applications as those that will likely become employers. They report that, historically, high-propensity 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.

[27]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 is between 60% and 70%.

entrepreneurship, i.e., entrepreneurial spawning. We find that firms that increased remote work more during COVID saw a higher share of their employees starting new firms. The response is stronger among 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 turnovers, suggesting remote work directs workers disproportionally to entrepreneurship relative to other labor outcomes. These effects are not driven by employee selection, preference change, or forced turnover. 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 13.4% 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 effects into account when designing future work policies.

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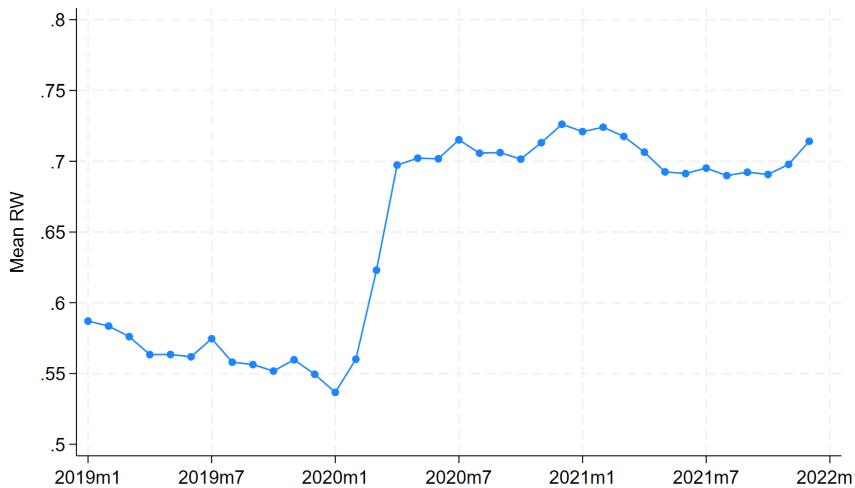
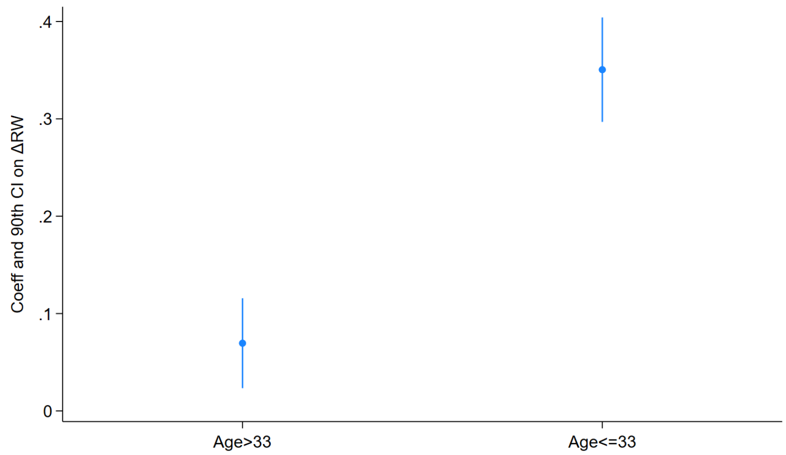
Figure 1: Monthly Remote Work Measure from 2019 to 2021



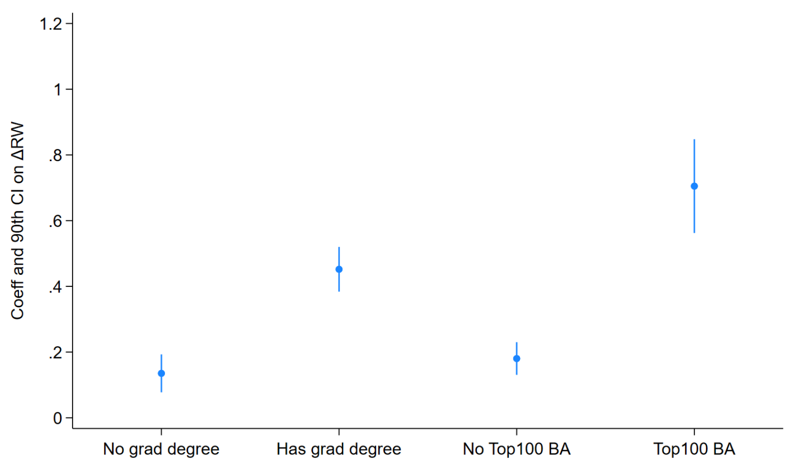
This graph plots the monthly average of our firm-level RW measure for the set of firms active in February 2020, for the period of January 2019 to December 2021.

Figure 2: Cross-Sectional Heterogeneity by Employee Characteristics

(a) By Employee Age



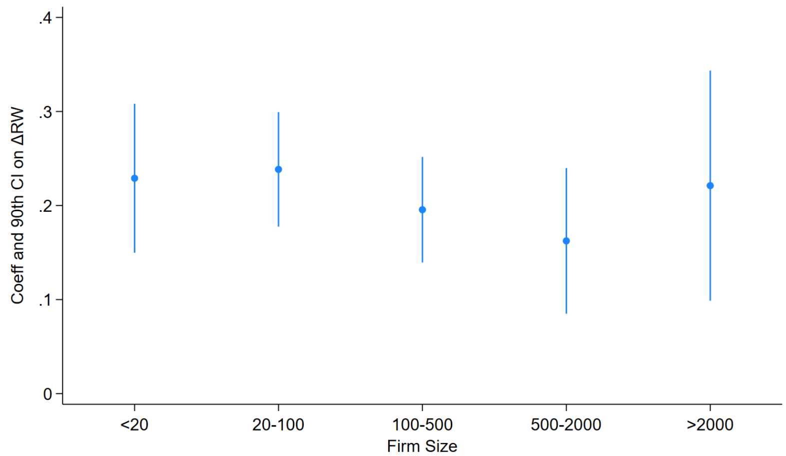
(b) By Employee Education



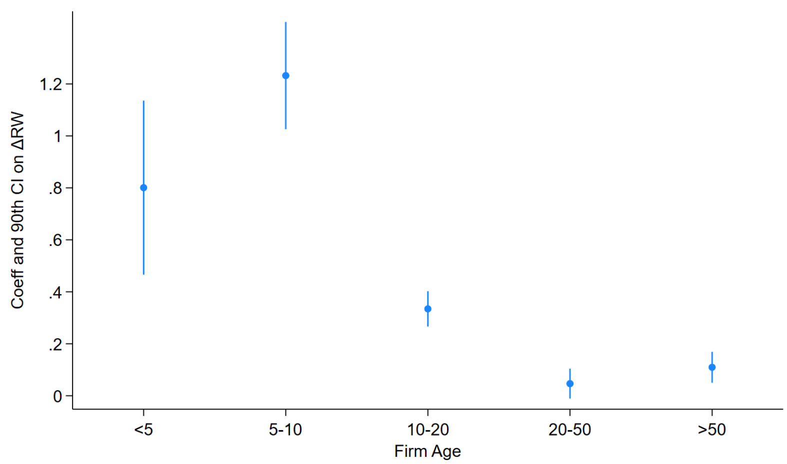
This figure shows the heterogeneity of our cross-sectional results along employee characteristics. We interact our individual-level OLS specification in column 1 of Table 3 with indicators for employees' age≤33 (Panel A) or education levels (Panel B). We then plot the coefficient and 90th confidence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

Figure 3: Cross-Sectional Heterogeneity by Firm Characteristics

(a) By Firm Employment Size

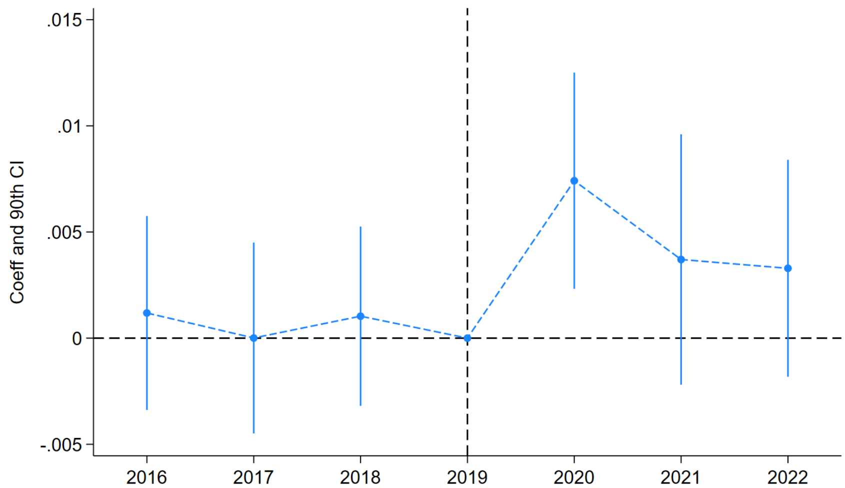


(b) By Firm Age



This figure shows the heterogeneity of our cross-sectional results along firm characteristics. We interact our firm-level OLS specification (column 1 of Table 3) 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.

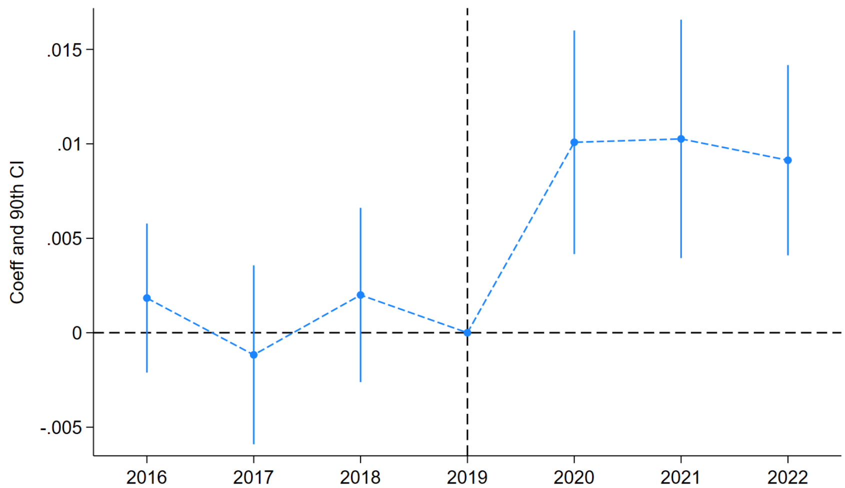
Figure 4: Firm-Level Dynamics by *Commute*: All Employees Sample



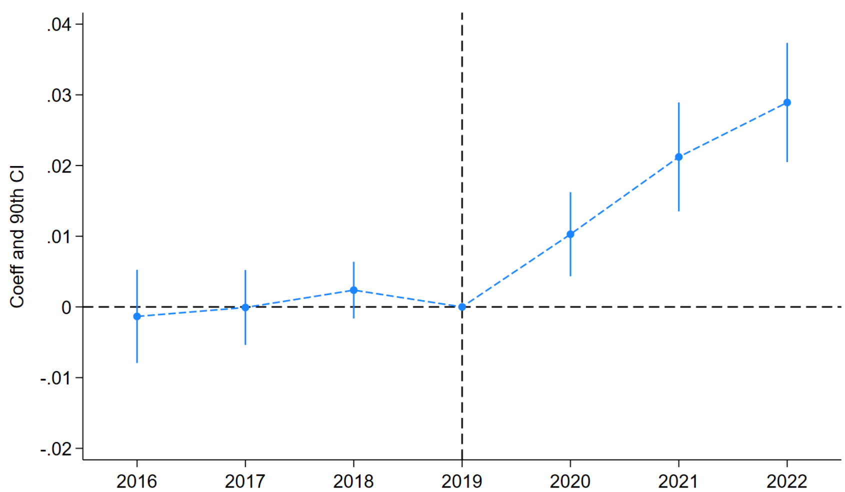
(a) Outcome = SpawnShare

This figure shows the dynamic DID effects estimated from firm-level panel regression in Equation 4 for the all employees sample. Each dot (bar) represents the point estimate (90th confidence interval) of the coefficient on $Commute_{f,2019} \times \mathbb{1}(Year = t)$. The sample tracks all employees of Feb2020 firms (i.e., firms active in February 2020) from 2016 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. The dependent variable is 100 times the fraction of employees who spawned from the Feb2020 firm in a given year. 2019 is the omitted base year. Standard errors are clustered at the NAICS 4-digit level.

Figure 5: Firm-Level Dynamics by *Commute*: Fixed Employees Sample



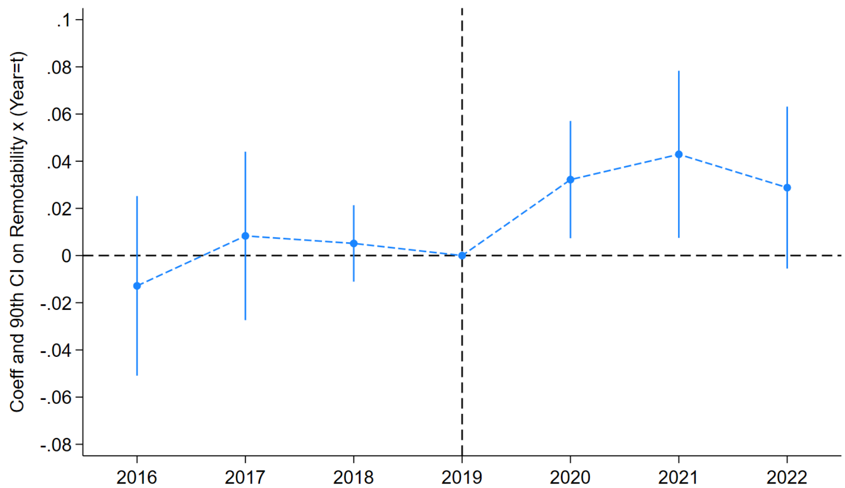
(a) Outcome = SpawnShare



(b) Outcome = FounderShare

Panel A (B) shows the dynamic DID effects estimated from firm-level panel regression in Equation 4 (Equation 6) for the fixed employees sample. Each dot (bar) represents the point estimate (90th confidence interval) of the coefficient on $Commute_{f,2019} \times \mathbb{1}(Year = t)$. The sample tracks a fixed set of employees employed in February 2020 from 2016 to 2022 regardless of which employer they were with in a year. We then link these individuals' spawning events (Panel A) and founder status (Panel B) to their Feb2020 employer and the firm's RW policy change. The sample is collapsed to firm-year level. The dependent variable in Panel A is 100 times the fraction of employees who spawned in a given year. The dependent variable in Panel B is 100 times the fraction of employees who were a founder in a given year. 2019 is the omitted base year. Standard errors are clustered at the NAICS 4-digit level.

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:

$$Ln(new\ firms)_{i,t} = \alpha_i + \beta_t + \sum_{n \neq 2019}^{2016 \rightarrow 2022} \theta_t \times Remotability_i \times \mathbb{1}(Year = t) + \epsilon_{i,t}$$

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 2016 to 2022, the last year of BDS. 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
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
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

Variable	N	p5	p50	p95	Mean	SD
	All Employees Sample					
SpawnShare	958259	0.000	0.000	0.000	0.067	0.577
Commute	946669	-1.308	0.075	1.294	0.041	0.810
BizClose	913556	-1.392	0.147	1.714	0.014	0.999
Post2020	958259	0.000	0.000	1.000	0.434	0.496
	Fixed Employees Sample					
SpawnShare	1220904	0.000	0.000	0.000	0.089	0.672
FounderShare	1220904	0.000	0.000	1.639	0.279	1.254
Commute	1205912	-1.377	0.039	1.262	-0.001	0.818
BizClose	1164160	-1.410	0.137	1.712	0.003	1.005
Post2020	1220904	0.000	0.000	1.000	0.375	0.484

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, and covers the sample period of 2016 to 2022. In top top panel, the sample is based on all employees of Feb2020 firms over 2016 to 2022 and is collapsed to Feb2020-firm-year level allowing compositional changes in employees. In the bottom panel, 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 2016 to 2022 and collapse them to Feb2020-firm-year level. All variables are at the firm-level, except that *BizClose* is at the county-level.

Table 3: Cross-Sectional Analysis

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample:	Individual-level		Firm-level	
Dep var:	$Spawn_{2020\text{-}2022}$		$SpawnShare_{2020\text{-}2022}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.208***	3.218***	0.116***	1.308*
	(0.028)	(0.878)	(0.038)	(0.677)
RW_{2019}	0.224***	1.925***	0.137***	0.857**
	(0.036)	(0.521)	(0.039)	(0.404)
Ln(emp)	-0.030***	-0.041***	-0.034***	-0.039***
	(0.003)	(0.004)	(0.004)	(0.005)
Firm age	-0.001***	-0.000***	-0.001***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning ratio	0.130***	0.118***	0.066***	0.065***
	(0.010)	(0.010)	(0.011)	(0.011)
Tenure	-0.014***	-0.014***	-0.041***	-0.040***
	(0.001)	(0.001)	(0.003)	(0.003)
Seniority	0.094***	0.091***	0.197***	0.181***
	(0.007)	(0.006)	(0.018)	(0.022)
Ln(salary)	0.026***	0.031***	0.137***	0.172***
	(0.006)	(0.007)	(0.034)	(0.041)
Prior founder	4.538***	4.523***	8.293***	8.161***
	(0.155)	(0.154)	(0.797)	(0.779)
First-stage IV coeff:				
Commute		0.009***		0.010***
		(0.002)		(0.001)
Kleibergen-Paap F-stat		35.043		299.455
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13542997	13542997	136121	136121
R-squared	0.003	0.000	0.052	0.014

This table examines the impact of remote work on employees' entrepreneurial spawning at the individual level. In columns 1 and 2, the sample is at the individual level and and includes on all employees employed with firms of employment size between 10 and 5000 as of February 2020. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022. In columns 3 and 4, the sample is at the firm level and includes on all firm with employment size between 10 and 5000 as of February 2020. 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 and 3 estimate the OLS results while columns 2 and 4 estimate the 2SLS result, where the instrument *Commute* is the average commute distance of a firm's employees in 2019. 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: Alternative Instrument

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample:	Individual-level		Firm-level	
Dep var:	$Spawn_{\text{2020-2022}}$		$SpawnShare_{\text{2020-2022}}$	
$\Delta RW_{2019\rightarrow2020/21}$	0.258***	8.209***	0.156***	4.317***
	(0.035)	(2.676)	(0.039)	(1.049)
RW_{2019}	0.248***	4.727***	0.166***	2.667***
	(0.044)	(1.493)	(0.040)	(0.648)
Ln(emp)	-0.026***	-0.059***	-0.031***	-0.054***
	(0.003)	(0.012)	(0.005)	(0.008)
Firm age	-0.001***	0.000	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning ratio	0.140***	0.102***	0.063***	0.060***
	(0.012)	(0.016)	(0.012)	(0.012)
Tenure	-0.015***	-0.013***	-0.044***	-0.038***
	(0.001)	(0.001)	(0.003)	(0.003)
Seniority	0.094***	0.086***	0.189***	0.141***
	(0.007)	(0.006)	(0.018)	(0.020)
Ln(salary)	0.036***	0.039***	0.184***	0.243***
	(0.006)	(0.009)	(0.032)	(0.039)
Prior founder	4.504***	4.454***	8.520***	7.974***
	(0.153)	(0.147)	(0.771)	(0.752)
First-stage IV coeff:				
BizClose		0.034***		0.060***
		(0.011)		(0.006)
Kleibergen-Paap F-stat		9.367		84.753
NAICS 4-dig FE	Yes	Yes	Yes	Yes
Observations	13204037	13204037	131453	131453
R-squared	0.003	-0.012	0.040	-0.078

This table examines the impact of remote work on employees' entrepreneurial spawning at the individual level, using an alternative instrument $BizClose$, which measures the fraction of time in 2020 and 2021 that a county's local businesses were mandated to close. In columns 1 and 2, the sample is at the individual level and and includes on all employees employed with firms of employment size between 10 and 5000 as of February 2020. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022. In columns 3 and 4, the sample is at the firm level and includes on all firm with employment size between 10 and 5000 as of February 2020. The dependent variable is 100 times the fraction of employee starting a new business between March 2020 and December 2022. The key independent variable $\Delta RW_{2019\rightarrow2020/21}$ is the change in the Feb2020 firm's RW from 2019 to 2020/2021 average. Columns 1 and 3 estimate the OLS results while columns 2 and 4 estimate the 2SLS result. 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

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	All Employees Sample		Fixed Employees Sample			
Dep var:	*SpawnShare*		*SpawnShare*		*FounderShare*	
Commute × Post2020	0.004**		0.009***		0.020***	
	(0.002)		(0.002)		(0.005)	
BizClose × Post2020		0.005***		0.010***		0.029***
		(0.001)		(0.001)		(0.004)
Feb2020-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	937599	904795	1055173	1018640	1055173	1018640
R-squared	0.061	0.062	0.04	0.041	0.57	0.569

This table shows the DID results estimated on a firm-year panel following Equations 5 and 5. The sample period is 2016 to 2022. The dependent variable in columns 1 to 4 is 100 times the fraction of employees who spawned in a given year. The dependent variable in columns 5 and 6 is 100 times the fraction of employees who were a founder (i.e., stayed din entrepreneurship) in a given year. *Post2020* is a dummy indicating years 2020-2022. We interact *Post2020* with two continuous treatment variables, *Commute* and *BizClose*, both standardized to reflect the effect of a one standard deviation change. The sample in columns 1 and 2 tracks all employees of Feb2020 firms from 2016 to 2022 (i.e., allowing for compositional changes) and examine their spawning events from the Feb2020 firms. The sample in columns 3 to 6 tracks a fixed set of employees employed in February 2020 over 2016-2022 regardless of who their employers were in a year. We then link these individuals' spawning events (columns 3 and 4) and founder status (columns 5 and 6) to their Feb2020 employers and collapse to Feb2020-firm-year level. 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)
	OLS	2SLS
Dep var:	$Spawn_{2020\text{-}2022}$	
$\Delta RW_{2019\rightarrow 2020/21}$	0.305***	2.854**
	(0.053)	(1.234)
RW_{2019}	0.387***	1.869**
	(0.064)	(0.730)
Ln(emp)	-0.072***	-0.081***
	(0.006)	(0.007)
Firm age	-0.001***	-0.001***
	(0.000)	(0.000)
Prior spawning ratio	0.198***	0.187***
	(0.016)	(0.017)
Tenure	0.015***	0.016***
	(0.003)	(0.003)
Seniority	0.171***	0.168***
	(0.010)	(0.011)
Ln(salary)	0.244***	0.247***
	(0.025)	(0.025)
Prior founder	7.998***	7.987***
	(0.273)	(0.273)
First-stage IV coeff:		
Commute		0.009***
		(0.001)
Kleibergen-Paap F-stat		35.937
NAICS 4-dig FE	Yes	Yes
County FE	Yes	Yes
Observations	6447182	6447182
R-squared	0.006	0.004

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: Quality of Marginally Spawned Firms

Panel A. Employer

	(1)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn as:			
	Employer	Non-employer	Employer	Non-employer
$\Delta RW_{2019\rightarrow2020/21}$	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)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn as:			
	Has website	No website	Has website	No website
$\Delta RW_{2019\rightarrow2020/21}$	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)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn as:			
	VC-backed	Non-VC-backed	VC-backed	Non-VC-backed
$\Delta RW_{2019\rightarrow2020/21}$	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

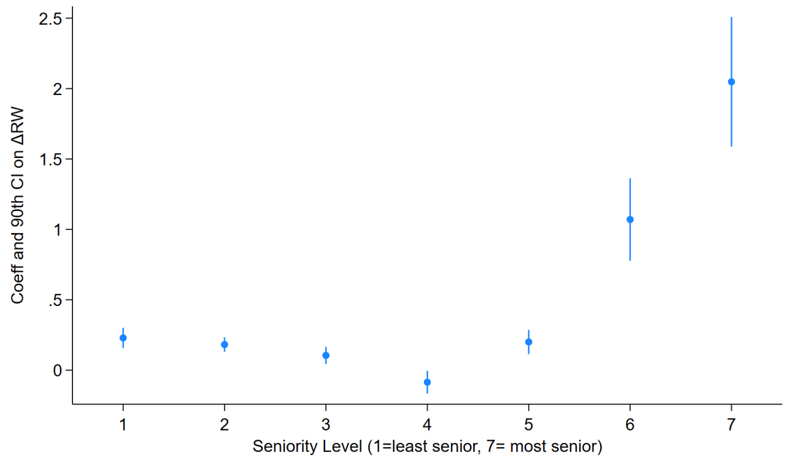
	(1)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn into industry with			
	High risk	Low risk	High risk	Low risk
$\Delta RW_{2019 \rightarrow 2020/21}$	0.144***	0.042***	1.973***	0.875**
	(0.021)	(0.016)	(0.484)	(0.425)
P-val of diff	0.000		0.089	
Kleibergen-Paap F-stat			35.043	35.043
NAICS 4-dig FE	Yes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

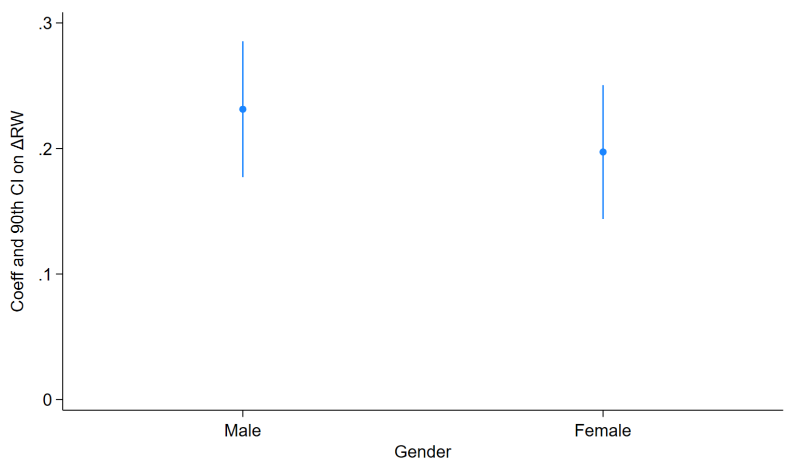
Appendix Figures and Tables

Figure A.1: Additional Cross-Sectional Heterogeneity

(a) By Employee Seniority

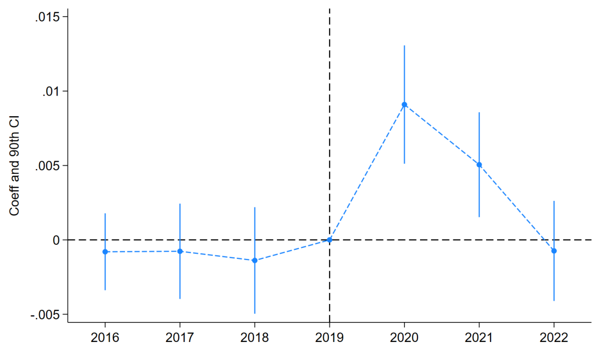


(b) By Employee Gender

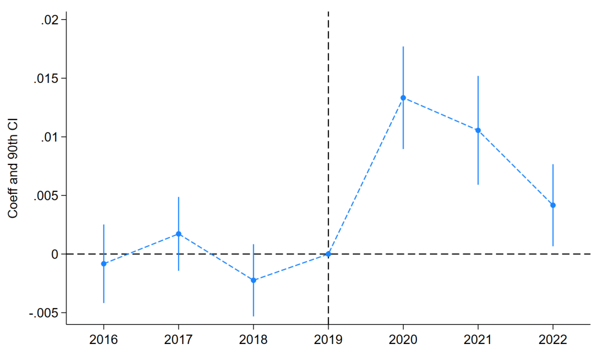


This figure shows additional heterogeneity of our cross-sectional results along employees' seniority and gender. We interact our individual-level OLS specification in column 1 of Table 3 with indicators for employees' seniority (Panel A) and gender (Panel B). We then plot the coefficient and 90th confidence interval of the interaction terms.

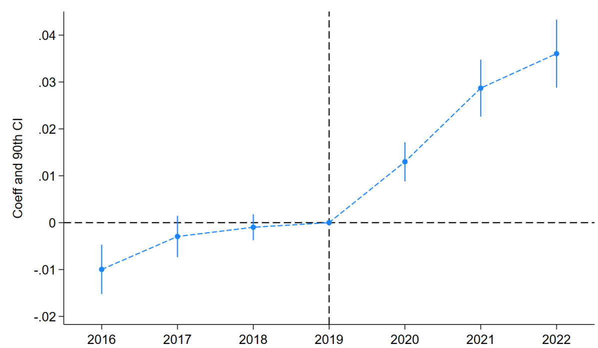
Figure A.2: Firm-Level Dynamics by *BizClose*



(a) All Employees Sample, Y=SpawnShare



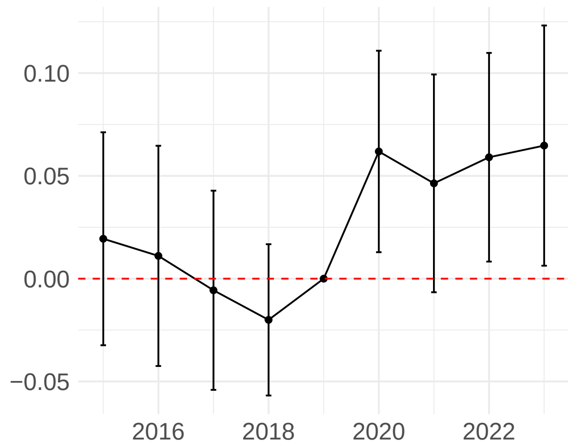
(b) Fixed Employees Sample, Y=SpawnShare



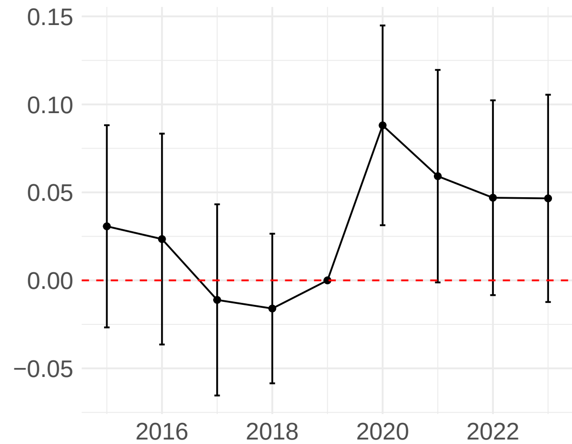
(c) Fixed Employees Sample, Y=FounderShare

These figures show the dynamic DID effects estimated from firm-level panel regression in Equation 4, where we replace the treatment variable $Commute_{f,2019}$ with county-level $BizClose_c$, the fraction of time over 2020 and 2021 that local businesses were mandated to close. Each dot (bar) represents the point estimate (90th confidence interval) of the coefficient on $BizClose_c \times \mathbb{1}(Year = t)$. In Panel A, the sample tracks all employees of Feb2020 firms (i.e., firms active in February 2020) from 2016 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. In Panels B and C, the sample tracks a fixed set of employees employed in February 2020 from 2016 to 2022 regardless of which employer they were with. We then link these individuals' spawning events (Panel B) and founder status (Panel C) to their Feb2020 employer and its RW policy change. The sample is collapsed to firm-year level. The dependent variable is 100 times the fraction of employees who spawned in a given year in Panel B, while it is 100 times the fraction of employees who were a founder in a given year in Panel C. In both graphs, 2019 is the omitted base year. Standard errors are clustered at the NAICS 4-digit level.

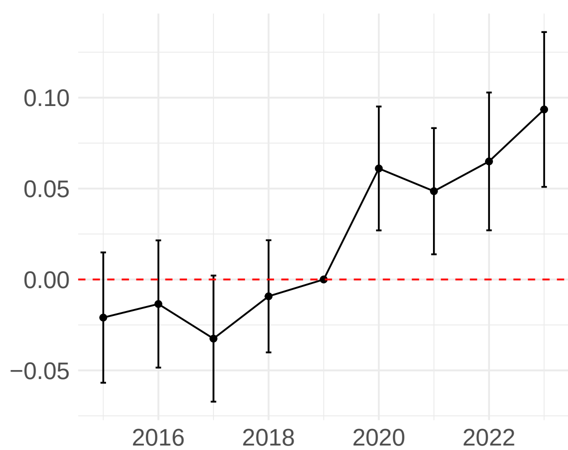
Figure A.3: Aggregate Evidence: QWI Young Firm Employment



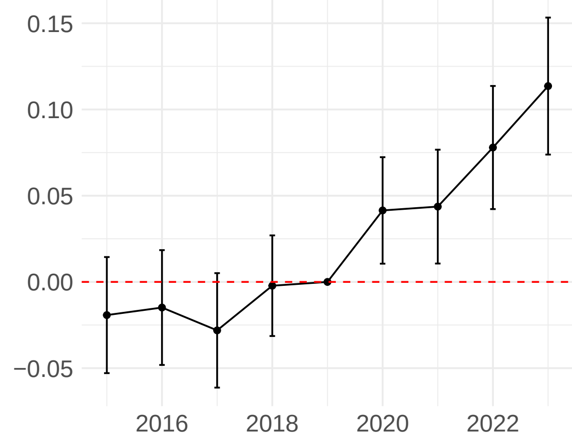
(a) Treat $= \Delta RW > 90^{th}\ pct$



(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:

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

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 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: First Stage Results

	(1)	(2)	(3)	(4)
Sample:	Individual-level		Firm-level	
Dep var:	$\Delta RW_{2019\rightarrow 2020/21}$			
Commute	0.009***		0.010***	
	(0.002)		(0.001)	
BizClose		0.034***		0.060***
		(0.011)		(0.006)
RW_{2019}	-0.568***	-0.563***	-0.609***	-0.601***
	(0.014)	(0.015)	(0.008)	(0.009)
Ln(emp)	0.003***	0.004***	0.004***	0.005***
	(0.000)	(0.001)	(0.000)	(0.000)
Firm age	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning ratio	0.386***	0.462***	0.075	0.077
	(0.070)	(0.069)	(0.053)	(0.054)
Tenure	-0.000***	-0.000***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Seniority	0.001***	0.001***	0.012***	0.012***
	(0.000)	(0.000)	(0.001)	(0.001)
Ln(salary)	-0.002***	-0.001	-0.029***	-0.018***
	(0.001)	(0.001)	(0.003)	(0.003)
Prior founder	0.005***	0.006***	0.108***	0.127***
	(0.001)	(0.001)	(0.027)	(0.028)
Kleibergen-Paap F-stat	35.043	9.367	299.455	84.753
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	No	Yes	No
Observations	13542997	13204037	136121	131453
R-squared	0.523	0.490	0.477	0.457

The table shows the first-stage results for our 2SLS specifications in Tables 3 and 4, using firm-level *Commute* (columns 1 and 3) and county-level *BizClose* (columns 2 and 4) as instrument. 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: Testing the Monotonicity Condition

Subsample by:	RW_{2019}	Ln(emp)	Firm age	Prior spawning ratio	Avg. tenure	Avg. seniority	Avg. ln(salary)	Avg. prior founder	Avg. grad degree	Avg. top100 BA	Avg. age
					First-stage coeff on *Commute*						
High	0.004***	0.013***	0.012***	0.011***	0.010***	0.010***	0.010***	0.013***	0.010***	0.013***	0.010***
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Low	0.014***	0.009***	0.009***	0.010***	0.010***	0.011***	0.011***	0.010***	0.011***	0.011***	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
					First-stage coeff on *BizClose*						
High	0.045***	0.051***	0.054***	0.053**	0.050***	0.071***	0.069***	0.049***	0.057***	0.065**	0.042***
	(0.006)	(0.007)	(0.008)	(0.022)	(0.009)	(0.008)	(0.009)	(0.009)	(0.014)	(0.025)	(0.009)
Low	0.076***	0.067***	0.064***	0.060***	0.065***	0.047***	0.047***	0.061***	0.061***	0.058***	0.074***
	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)

The table tests the monotonicity condition for our instruments following the method in Dobbie, Goldin, and Yang (2018). Monotonicity implies that the first-stage coefficient on the instrument should be non-negative in all subsamples formed based on observables. We test this implication in subsamples split by various firm characteristics (indicated in top row) at the median. Given our instruments are at the firm-level, we test this condition in our firm-level sample. The table reports the coefficient and standard errors on our instruments in the first stage, controlling for the same variables and fixed effects as firm-level specifications in Tables 3 and 4. 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: Correlation of IVs with Pre-Pandemic Spawning and Employee Founder Experience

	(1)	(2)	(3)	(4)
	$SpawnShare_{2019}$		$PriorFounderShare_{2019}$	
Commute	0.001		0.007	
	(0.003)		(0.005)	
BizClose		0.029		0.030
		(0.027)		(0.055)
RW_{2019}	0.013	0.014	0.131***	0.134***
	(0.010)	(0.010)	(0.028)	(0.028)
Ln(emp)	-0.007***	-0.007***	-0.037***	-0.037***
	(0.002)	(0.002)	(0.004)	(0.004)
Firm age	-0.000***	-0.000***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.009***	-0.009***	-0.005**	-0.008***
	(0.001)	(0.001)	(0.002)	(0.002)
Seniority	0.036***	0.037***	0.132***	0.129***
	(0.007)	(0.007)	(0.017)	(0.017)
Ln(salary)	0.010	0.013	0.131***	0.155***
	(0.013)	(0.013)	(0.031)	(0.033)
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	No	Yes	No
Observations	136121	131344	136139	131362
R-squared	0.018	0.008	0.034	0.022

The table provides evidence consistent with our instruments satisfying the exclusion condition. We do this by showing an insignificant relationship between each of two instruments and firms' pre-pandemic spawning share and the share of employees with past founder experience. The dependent variable in columns 1 and 2 is 100 times the fraction of employees that spawned from the firm to become an entrepreneur in 2019. The dependent variable in columns 3 and 4 is 100 times the fraction of employees in 2019 with past founder experience (measured over 2015 to 2019). All other control variables and fixed effects follow that of columns 3-4 of Tables 3 and 4. 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: Cross-Sectional Analysis: Reduced Form Effects

	(1)	(2)	(3)	(4)
Sample:	Individual-level		Firm-level	
Dep var:	$Spawn_{2020\text{-}2022}$		$SpawnShare_{2020\text{-}2022}$	
Commute	0.029***		0.014**	
	(0.006)		(0.007)	
BizClose		0.281***		0.258***
		(0.054)		(0.062)
RW_{2019}	0.096***	0.103***	0.060*	0.072**
	(0.025)	(0.029)	(0.034)	(0.031)
Ln(emp)	-0.032***	-0.025***	-0.034***	-0.030***
	(0.003)	(0.003)	(0.004)	(0.005)
Firm age	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior spawning ratio	13.086***	14.002***	6.613***	6.320***
	(1.009)	(1.169)	(1.117)	(1.180)
Tenure	-0.014***	-0.015***	-0.041***	-0.044***
	(0.001)	(0.001)	(0.003)	(0.003)
Seniority	0.094***	0.094***	0.197***	0.194***
	(0.007)	(0.007)	(0.018)	(0.018)
Ln(salary)	0.026***	0.034***	0.133***	0.166***
	(0.006)	(0.006)	(0.033)	(0.032)
Prior founder	4.539***	4.505***	8.303***	8.524***
	(0.156)	(0.153)	(0.797)	(0.772)
NAICS 4-dig FE	Yes	Yes	Yes	Yes
County FE	Yes	No	Yes	No
Observations	13542997	13204037	136121	131453
R-squared	0.003	0.003	0.052	0.040

The table presents the reduced form effects of our two instruments in our cross-sectional analysis. The sample and specification otherwise follow Tables 3 and 4. 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.6: Cross-Sectional Analysis: Additional Controls

	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Sample:	Individual-Level		Firm-Level	
Dep var:	$Spawn_{2020\text{-}2022}$		$SpawnShare_{2020\text{-}2022}$	
$\Delta RW_{2019 \rightarrow 2020/21}$	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)
Tenure	-0.008***	-0.008***	-0.018***	-0.017***
	0.000	0.000	(0.003)	(0.003)
Seniority	0.077***	0.075***	0.244***	0.227***
	(0.005)	(0.005)	(0.020)	(0.024)
Ln(salary)	-0.014**	-0.011*	0.139***	0.175***
	(0.006)	(0.006)	(0.033)	(0.042)
Prior founder	4.386***	4.379***	8.043***	7.924***
	(0.147)	(0.146)	(0.786)	(0.772)
Age	-0.007***	-0.006***	-0.024***	-0.024***
	0.000	0.000	(0.002)	(0.002)
Has grad degree	0.078***	0.077***	0.058	0.050
	(0.009)	(0.009)	(0.064)	(0.062)
Top100 BA	0.079***	0.074***	0.691***	0.633***
	(0.015)	(0.015)	(0.135)	(0.140)
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 specification follows Table 3, 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. The instrument *Commute* is the average commute distance of a firm's employees in 2019. 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.7: 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 Table 3. The instrument *Commute* is the average commute distance of a firm's employees in 2019. 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.8: 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. The specification follows Table 3. The instrument *Commute* is the average commute distance of a firm's employees in 2019. 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.9: Heterogeneity by RW of Spawned Firm

	(1)	(2)	(3)	(4)
	OLS	OLS	2SLS	2SLS
Dep var:	Spawn into industry with			
	High RW	Low RW	High RW	Low RW
$\Delta RW_{2019 \rightarrow 2020/21}$	0.017***	0.016***	0.231**	0.297***
	(0.005)	(0.005)	(0.110)	(0.111)
P-val of diff	0.887		0.805	
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	0.000	0.000	0.000	0.000

This table shows the heterogeneity of our individual-level cross-sectional results by whether the spawned business is in an industry (NAICS 4-digit) with an above-median (high) or below-median (low) level of RW measured post 2020. The specification follows Table 3. The instrument *Commute* is the average commute distance of a firm's employees in 2019. The dependent variable is 100 times a dummy indicating that the employee started a new business between March 2020 and December 2022 in a NAICS 4-digit industry with an above- or below-median level of RW post 2020. Because RW is missing for most spawned business due to small employment size and infrequent tracking, we use the the average RW of the spawned firm's industry (NAICS 4-digit) as a proxy for the spawned firm's level of remote work. 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.10: Heterogeneity by Local K12 Schools' Learning Model

	(1)	(2)
Dep var:	$Spawn_{2020\text{-}2022}$	
Sample:	Age 25-50	Other ages
$\Delta RW_{2019 \to 2020/21}$	0.191***	0.180***
	(0.039)	(0.050)
$\Delta RW_{2019 \to 2020/21} \times \%SchoolInPerson$	0.107*	-0.123
	(0.059)	(0.083)
Controls	Yes	Yes
NAICS 4-dig FE	Yes	Yes
County FE	Yes	Yes
Observations	10181985	3099449
R-squared	0.004	0.003

This table shows the heterogeneity of our individual-level cross-sectional results by local K12 school learning model. The interaction variable, $\%SchoolInPerson$, is the fraction of time K12 schools in a county was in-person as opposed to remote or hybrid during 2020 and 2021. The data come from `www.covidschooldatahub.com`. The specification follows Table 3 Column 1. Column 1 restricts to individuals of age 25 to 50, while column 2 restricts of individuals of other ages (i.e, <25 or >50). The dependent variable is 100 times a dummy indicating that the employee started a new business 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.