Who Benefits from Remote Work?

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Abstract

We examine how remote work affects firm productivity and employment outcomes. Using an instrumental variables design based on pre-pandemic occupational remote work suitability, we find that remote work increases the productivity of startups while reducing productivity for established firms. This effect operates through two channels: labor market scaling and equity compensation. Remote startups experience substantially higher hiring rates, overcoming geographical hiring constraints for talent acquisition, and are able to align incentives for remote workers through equity compensation. Our findings suggest that remote work alleviates key disadvantage that young firms face in hiring, expanding, and competing for talent, while established firms experience challenges in adopting remote working practices.

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1 Introduction

The rise of remote work has fundamentally shifted how firms compete for talent and organize production. By lowering geographic boundaries in labor markets and changing the nature of workplace interaction, remote work practices have the potential to impact the sorting of workers across different firms as well as their performance. However, despite substantial research on how remote work impacts productivity and organizational structures at specific firms or occupations (Bloom et al., 2015; Gibbs et al., 2023; Choudhury et al., 2021; Duchin and Sosyura, 2025), little is known about *which* types of firms benefit or suffer from remote work adoption. This is especially pertinent given recent examples of employers pushing towards an in-person policy, especially among large firms.[1] It is thus critical to understand the economic consequences of remote work adoption and whether it levels the playing field between firms or reinforces existing competitive advantages.

This paper estimates the effect of remote work practices on firm performance across a broad cross-section of U.S. companies, focusing on a comparison of established and startup firms. To do so, we rely on a systematic measure of white-collar coding productivity, connected to firm-level remote work adoption rates, and labor market flows. The resulting sample directly connects measures of individual worker productivity along with worker-level attributes and firm-level remote work decisions, to enable a comprehensive assessment of remote work's implications. We focus on a comparison of established firms and younger startup firms (those younger than ten years) because the key mechanisms through which remote work affects productivity and scaling are likely to operate differently for firms at different stages of organizational development and with different abilities to hire (Hansen et al., 2023).

[1] Jamie Dimon, the CEO of JPMorgan, has said, "You can't learn working from your basement... I gave a very detailed answer about why [work from home] doesn't work for young people, why it doesn't work for management, why it doesn't work for innovation. I completely applaud your right to not want to go to the office every day. But you're not going to tell JPMorgan what to do." See: https://fortune.com/2025/05/16/jpmorgan-ceo-jamie-dimon-return-to-office-gen-z-workers-management/.

To establish the causal impacts of remote work, we rely on an instrumental variables design based on the pre-pandemic task suitability for remote work across occupations to instrument for remote work adoption. We find that firms whose occupations were easier to perform remotely (using the classification of Dingel and Neiman (2020)) were indeed more likely to adopt remote working practices in the subsequent post-pandemic period. We then use the rate of predicted remote work adoption from this instrument, further controlling for firm by worker fixed effects, to instrument for the role of remote work on worker productivity.

Our key finding is that remote work increases coder productivity for startups, while lowering productivity for established firms in the post-pandemic period. A programmer working at a remote startup experiences a statistically significant increase in productivity rank of twelve percentage points (out of one hundred) relative to remote established firms. By contrast, a programmer at an established firm which goes remote after 2020 sees a decline in productivity of nine percentage points in rank. These strongly varied outcomes contrast against an average effect which is slightly negative, but close to zero and insignificant. These results show that the implications of remote work are heterogeneous and depend crucially on firm age, which appears to be an important proxy for successful adoption. We observe comparable effects in OLS event studies comparing outcomes for remote startups against remote established firms. We also find that remote private firms raise more capital from venture capital, suggesting that such firms benefit overall from going remote.

To explore the mechanisms driving this result, we emphasize two key channels. One is equity compensation. Startups are more likely to offer equity as a component of compensation, which helps to align incentives (particularly in small teams, for which each worker's effort is more pivotal for company success). This financial alignment through equity compensation can substitute for in-person monitoring. The second mechanism is the role of labor market constraints faced by startups (Ouimet and Zarutskie, 2014; Beckman and

Burton, 2008; Gupta et al., 2024). Competition for talent acquisition disproportionately limits startups, with large firm hiring "crowding-out" startup access to talent (Bessen et al., 2023). These constraints are geographical in nature, as startups lack a diverse network of physical work locations compared to larger firms.

Consistent with remote work disproportionately alleviating hiring constraints for startups, we find that remote startups experience large increases in hiring rates, which are substantially larger than leaving rates and so result in substantial increases in net employee growth rates. Using our instrument, we estimate that a remote startup experiences an increase in employee growth of seven percent after 2020, while a remote established firm sees no difference over the same time period. Remote startups also increase their rate of job postings, as well as the rate of hiring per each job posting, indicating increased labor demand as well as matching rates. By contrast, we find that established firms actually experience slight decreases in headcount, as minor increases in hiring are completely matched by increases in departure rates. Startups are able to scale much faster when going remote, in other words, while established firms have greater difficulty in both hiring and retaining workers while remote.

We find that the two channels we emphasize, labor market scaling and equity compensation, combine to largely explain the productivity advantage enjoyed by remote startups. This result suggests that remote work is particularly beneficial for startups because it helps to both alleviate specific frictions they face in hiring, and plays to their strengths in using financial contracts to alleviate monitoring concerns. By contrast, larger firms appear to be less constrained in hiring (because they already tend to operate offices in many geographies, allowing them to hire workers in a range of geographies). They face therefore more of the negative effects of remote work in monitoring, and find it more difficult to substitute for equity compensation as an alternate monitoring tool.

We also show that the productivity increases follow employee hiring, particularly of engineers, pointing to spillover channels from newly hired remote workers. Consistent

with this explanation, existing workers who stay at remote startups also experience an increase in productivity. We further show that the effects on productivity are larger for startups which are fully remote, instead of hybrid, which are therefore able to access an even broader geographic market for talent. We find that remote startups are able to hire workers for lower salaries, suggesting greater labor access, and that the productivity effects are driven by geographic markets with weaker baseline access to talent. We also show that our results are not driven by the technology industry specifically, but instead appear to reflect broad-based productivity improvements across a range of industries. Finally, we also show that our results are robust to controls for contemporaneous shocks to industries and geographies. Overall, our results collectively point to important hiring constraints for young firms, which remote working practices appear to alleviate.

We also explore the heterogeneity of our results across workers depending on their preference for remote work schedules. Women have greater preference for flexibility (Goldin, 2014) and remote work (Banerjee et al., 2024). We find that female employees exhibit higher productivity in remote startups, while they see lower productivity in remote established firms. This result highlights the importance of organizational context in influencing how remote work affects productivity, even within workers who value remote work the most.

Our findings have important implications for understanding the future of business dynamism and productivity growth. By expanding the geographic scope of labor markets, remote work appears to reduce hiring frictions that young firms face given their limited geographic reach. This has implications for the distribution of innovation activity and productivity growth across the economy, and may help to account for the increased startup activity in the post-pandemic period (Decker and Haltiwanger, 2023), as well as the role of remote work driving entrepreneurial entry directly (Kwan et al., 2025). The productivity gains we document for remote work suggest these effects operate through talent reallocation and enhanced worker output, amplifying their potential impact on

aggregate productivity. Our work also helps to account for the heterogeneous spread of remote work practices. While remote work adoption was initially rapid and has since stabilized (Barrero et al., 2023), our finding that remote work effectiveness varies by firm age suggests that established firms face greater challenges with employee monitoring or coordination under remote work arrangements, helping to explain uneven adoption of these practices.

Our paper relates most closely to the literature on the impact of remote work on productivity (Bloom et al., 2015; Emanuel and Harrington, 2023; Emanuel et al., 2023; Atkin et al., 2023; Gibbs et al., 2023; Aksoy et al., 2025; Choudhury et al., 2021; Duchin and Sosyura, 2025). We differ in several respects. First, we introduce new data which provides estimates of productivity across a much broader sample of firms than previous literature. Many existing studies examine only a single firm, industry, or occupation. We instead examine heterogeneity in treatment exposure across firms, which is our main focus. Second, we also connect these productivity differences to key mechanisms and drivers, highlighting the role of labor market frictions.

We also connect to a literature on the impact of labor scaling. Only a few papers have explored the implications of remote work on the labor market dynamics (Brinatti et al., 2021; Cestone et al., 2024). Other papers have explored the role of scaling technologies more broadly (Brynjolfsson et al., 1994). Our focus is on the role of remote work as a technology which alleviates labor market frictions for startups.

Finally, we relate to a literature on frictions impacting startups more broadly (Ewens et al., 2018; Howell, 2017; Bernstein et al., 2024; Kerr and Nanda, 2010). We relate most closely to papers which have explored frictions relating to startup formation, such as Alon et al. (2018); Haltiwanger et al. (2014); Decker and Haltiwanger (2023); Karahan et al. (2019); Barrios et al. (2024). Our emphasis on equity compensation as a channel connects to work showing that equity pay is a more effective incentive in small firms, where the free-rider problem (**?**) is less severe. Consistent with this, **?** find that employee stock ownership

improves productivity only in small firms.

Other papers in macroeconomics explore the role for labor frictions in smaller firms, such as Moscarini and Postel-Vinay (2012), partially because they cannot rely as much on internal labor markets (Cestone et al., 2024). Schubert (2025) highlights organizational frictions for firms adopting remote work connected to adoption of generative AI. Other papers focus on whether remote work increases (Kwan et al., 2025) or decreases entrepreneurial entry (Barrios et al., 2024). We differ in that we primarily focus on operational aspects of startups—on productivity and labor market scaling—rather than the extensive margin decision of whether to start a new company.

2 Data and Empirical Strategy

We construct a novel data set by combining information from four primary sources: (i) Scoop, which provides firm-level remote work data, (ii) Revelio Labs, which includes employment records sourced from LinkedIn, (iii) a mapping between GitHub and LinkedIn user profiles provided by HumanPredictions,[2], (iv) GitHub contribution data accessed through the GitHub API, and (v) analysis of job postings data to measure labor market shifts and equity compensation. We describe each of these data sources, along with the procedures used for the final sample creation, in detail below.

2.1 Remote Work Data

To measure remote working conditions at the firm level, we use information from Scoop, which provides a Flex Index containing information on the full-time, hybrid, and fully remote working practices of over 3,000 U.S. firms with a combined 42 million employees. The Flex Index is the most comprehensive source on company office requirements, with

[2]HumanPredictions was a public data aggregator that developed a proprietary database of over 150 million technical talent worldwide. It was acquired in 2024 and no longer exists as a private firm.

information on office policies sourced from a combination of public corporate statements and crowd-sourced information by employees.

We map the Scoop classifications of remote work into a 0–1 scale of remoteness. The underlying data categorize firms as "Fully Flexible" (either fully remote or employee choice), "Structured Hybrid," or "Fully in Office." If the firm is structured hybrid, the data also report the number of days employees are required to be in the office. We generate a Remote variable which is 1 if the firm is fully flexible; 0 if the firm is fully in office; and varies between 0 and 1 to capture the fraction of the workweek employees allowed to be remote.

We also measure whether firms have an occupational mix conducive to remote work using the teleworkable measure of Dingel and Neiman (2020), applied to the occupational distribution of the firm measured through Revelio data, described next.

2.2 Employer-Employee Data

Revelio Labs continuously collects online resumes from various platforms (such as LinkedIn) to build a universal HR database. We use these data to build a matched employer-employee panel that records each employee's firm and job title on a semi-annual basis.

We begin by merging the Scoop firm set with the Revelio sample, using firm name and location as matching variables. Scoop contains a total of 4,883 firms. We successfully merge 4,371 firms, resulting in a match rate of 90%.

Next, we restrict the sample to U.S.-based employees who were employed by these firms at any point from 2016 onward. In cases where an employee lists multiple overlapping positions (e.g., a primary job along with part-time or volunteer work), we retain the job with the highest seniority level,[3] prioritizing the one listed higher on the user's LinkedIn profile, as it is more likely to represent the primary position.

[3]Revelio assigns each job a seniority score between 1 and 4 based on job title, description, and other attributes.

Finally, we classify firms into startups and established firms based on their age. Firms younger than ten years as of 2020 are defined as startups; all others are classified as established firms. We collapse the resulting data to the firm half-year level, creating a balanced firm-level panel.

2.3 GitHub Data

We obtain data on publicly available profiles of U.S.-based GitHub users from HumanPredictions, a public data aggregator that has compiled a proprietary database covering technical professionals worldwide. This dataset includes users' names, locations, programming languages, GitHub join dates, and LinkedIn profile links.

We merge this GitHub data to the employer-employee panel constructed in the previous step, which links employment data from Revelio Labs to remote work indicators from Scoop. The merge is performed directly using the LinkedIn URLs provided by HumanPredictions for each GitHub user. HumanPredictions created this crosswalk using publicly available information, identifying cases where users list both GitHub and LinkedIn profiles together.

Next, we append this dataset with GitHub contribution data retrieved from GitHub's official API (Application Programming Interface). The API allows programmatic access to all publicly available data on GitHub.[4] We collect data on both public and private contributions from 2016 to 2022. Public contributions are those for which baseline source code is available. Private (or restricted) contributions are those where the base code is only visible to users who can edit the repository, and only the timing and quantity of contributions is available. We use both public and private contributions as our measure of user productivity.

GitHub commits provide valuable information on employee contributions. They are

[4]The GitHub GraphQL API (accessible at: https://api.github.com/graphql) provides the most comprehensive and up-to-date information, including private contributions, making it essential for capturing the full scope of user activity.

necessarily restricted to employees working in some software engineering or coding function, but span multiple industries—including technical firms as well as non-technical firms. The frequency of commits are commonly used within industry to judge the productivity or outputs of workers. Other papers using the GitHub data to measure productivity and work patterns include (Holub and Thies, 2023; McDermott and Hansen, 2021; Gupta et al., 2024). Appendix B validates that GitHub contributions serve as a high-frequency, granular measure of individual productivity, strongly correlated with other indicators of worker quality, including career advancement and publications, as well as firm-level outcomes such as organizational capital and Tobin's Q. It further shows that these relationships remain stable from the pre- to post-pandemic period. This mitigates against possible concerns that the economic significance of our productivity measure of GitHub commits may have changed in meaning over time.

The main filter we apply to the Revelio data is to require employees to have made at least one GitHub commit to a restricted repository in the pre-pandemic period. This requirement is there to ensure that our sample covers workers who are making active progress on firm-specific projects.

2.4 Job Posting Data

We access job postings from the Revelio Labs COSMOS database. This dataset aggregates postings from approximately 440,000 company websites, major job boards, and staffing firm platforms, comprising over 4.1 billion current and historical postings across 6.6 million companies. Revelio links these postings to its employee-level database through internal firm identifiers. We leverage this linkage to merge job postings with firm-level remoteness measures from Scoop and hiring outcomes from Revelio's employee data. This integration enables the construction of two key measures. First, it allows for the development of firm-level measures of job posting activity and the calculation of the ratio of hires to job postings. Second, job posting text is used to construct a firm-level measure of equity compensation:

we first identify postings with equity-related language, then apply LLM-based coding to classify whether those postings offer equity compensation, and finally aggregate these posting-level classifications to the firm level.

2.5 Fundraising Data

We draw on information on funding rounds from Crunchbase. Crunchbase provides comprehensive coverage of venture capital and private equity transactions. We collect data on all transactions through Crunchbase through 2023, including the round date, amount raised, and funding stage (i.e., seed, Series A, B, C, etc.). We successfully match 3,071 firms in our sample. Many firms may remain unmatched if, for example, they are public firms for the duration of the sample and never seek venture or other private funding. We additionally exclude public firms in our Crunchbase analysis to restrict attention to private firms which are potentially interested in further private capital rounds.

2.6 Summary Statistics

In Figure 1 we highlight the key descriptive fact motivating our analysis: the higher prevalence of remote work among younger firms. We measure remote work using Scoop data, as a measure of how many days a week workers can typically work remotely as of 2023, and plot this relationship against firm age as measured at the start of 2020, prior to the growth in remote work. We observe a strong negative association, namely that remote work is particularly prevalent among younger firms, with an R^2 of 14%. In our subsequent analysis, we focus particularly on younger firms, those less than ten years in age, which we classify as startups (as opposed to firms older than this, which we label established firms).[5]

[5]The ten-year cutoff for young firms is standard in the literature. For example, Ayyagari et al. (2011) and Haltiwanger et al. (2013) both adopt ten years as the threshold separating young and established firms. Appendix Figure A1 shows effects for productivity and growth across different cutoff definitions. We observe monotonically declining effects with higher age cutoffs, but focus on a cutoff of ten to be consistent with the

Table 1 provides summary statistics for our sample firms, including startups and established firms separately. We summarize variables at both the firm-level (Panel A), as well as at the employee level for individuals currently working at those firms (Panel B). Our comprehensive data enables the measurement of a range of characteristics about firms both public and private, including labor market characteristics and flows, remote work characteristics, compensation characteristics, firm characteristics, as well as the productivity levels of current employees. Our sample includes 3,508 firms for which we have matched data on both remote work status as well as other characteristics, as well as 37,186 employees with a GitHub user profile.

We observe a number of striking baseline differences between startups and established firms in our sample. Startups feature more labor market churn, with both higher rates of employees joining and leaving, resulting, on net, in higher growth rates over our sample period. Startups are both more conducive to remote work based on occupational mix, as well as more likely to pick remote work ex post. They are also slightly more likely to offer equity compensation. They tend to be located in higher-rent markets, and they tend to have higher employee productivity as well. The remainder of the paper focuses on disentangling these descriptive associations.

2.7 Empirical Strategy

To understand the implications of remote work on firm productivity, we consider the following triple-differences OLS specification at the individual level:

$$y_{ijt} = \beta_1 \text{Remote}_j \times \mathbb{1}(\text{Post}_t) + \beta_2 \text{Remote}_j \times \mathbb{1}(\text{Post}_t) \times \text{Startup}_j + \gamma_i \times \phi_j + \theta_t + \varepsilon_{ijt}. \quad (1)$$

Where y_{ijt} refers to employees' productivity i working in firm j at time t, measured through GitHub commits. Contributions, our main dependent variable at the individual

literature.

level, are measured as a count variable and contain many zeros as well as a highly skewed distribution. Following Chen and Roth (2023), we use the percentile rank of contributions as the dependent variable to avoid the limitations of a $log(1+x)$ transformation. We also show results using the raw number of contributions to help interpret the economic magnitude in number of contributions. Our coefficient of interest is β_2, and so our focus is on the differential impact of remote work on startup employee productivity, relative to remote established firms. All other interactions ($\text{Startup}_j \times \mathbb{1}(\text{Post}_t)$) are estimated but not reported for brevity. We include firm $\times$ individual fixed effects to account for employee-firm-match-specific characteristics. $\mathbb{1}(\text{Post}_t)$ is an indicator that takes the value of 1 between January 2020 and June 2022, and 0 otherwise. We consider time periods as half-years. We focus on a comparison of pre- and post-pandemic periods because of the dramatic rise in prevalence of remote work after the coronavirus pandemic (Barrero et al., 2023). This period, notably, saw heterogeneous remote work practices across firms, which we capture using a continuous measure for the remote practices among firms.

We consider an analogous specification when analyzing labor market outcomes, which are measured at the firm level:

$$y_{jt} = \beta_1 \text{Remote}_j \times \mathbb{1}(\text{Post}_t) + \beta_2 \text{Remote}_j \times \mathbb{1}(\text{Post}_t) \times \text{Startup}_j + \gamma_j + \theta_t + \varepsilon_{jt} \quad (2)$$

Where y refers to firm j scaling variables such as employment growth rates. Additional controls include firm and time fixed effects. Our sample period is 2016–2022 and our sample frequency is half-years. We focus on the triple interaction $\text{Remote}_j \times \mathbb{1}(\text{Post}_t) \times \text{Startup}_j$. As such, β_2 captures the impact of firm remote policies on firm scaling, in the aftermath of the pandemic period, differentially for startups.

To provide support for the parallel trends assumption of the difference-in-differences methodology, we present an event study analysis which compares outcomes over time. This specification allows us to observe characteristics of firms and employees both before

and after the pandemic:

$$y_{it} = \sum_{\tau=-T}^{T} \beta_1^\tau \, \text{Remote}_j \times \mathbb{1}(t=\tau) + \sum_{\tau=-T}^{T} \beta_2^\tau \, \text{Remote}_j \times \mathbb{1}(t=\tau) \times \text{Startup}_j + \gamma_i \times \phi_j + \theta_t + \varepsilon_{it}$$

and

$$y_{jt} = \sum_{\tau=-T}^{T} \beta_1^\tau \, \text{Remote}_j \times \mathbb{1}(t=\tau) + \sum_{\tau=-T}^{T} \beta_2^\tau \, \text{Remote}_j \times \mathbb{1}(t=\tau) \times \text{Startup}_j + \gamma_j + \theta_t + \varepsilon_{jt}.$$

While the event study allows for diagnostics over time, it does not address a natural concern in this setting: the endogenous selection of remote work policies by firms. Potentially, firms may engage in positive selection: those firms that anticipate higher productivity or are the most interested in scaling up may differentially pick higher remote work policies. Alternatively, we may observe negative selection: for instance, firms with lower productivity may have lower wages, and so offer remote work as a non-pecuniary perk to employees. To address this issue, we further implement an instrumental variables analysis.

The key motivation of our IV strategy is that occupations have a latent ability to be done remotely, even in the pre-2020 world when white-collar work was typically done on-site. We take advantage of existing measures of the latent ability of occupations to be done remotely, based on assessments of the tasks required of such occupations conducted before the pandemic, to predict ex-post whether such occupations were in fact done remotely (Dingel and Neiman, 2020). Such measures have been subsequently connected to realized remote work decisions at the occupational level (Mongey et al., 2021). Our approach takes the next step in assessing the latent remote capability of an entire firm, as a function of whether the occupational mix of employees in the firm, measured before the pandemic,

were more or less conducive to remote work.

Specifically, we consider a first stage regression of the general form:

$$\text{Remote}_j = \beta \cdot \text{Telework}_j + \varepsilon_j$$

In which we instrument for a firm's remote work policy using the aggregated telework score of the occupations done by the firm's employees, measured by estimating the Dingel and Neiman (2020) score at the occupation level and aggregating up at the firm level. For a given firm j with E_N employees, E_o employees in each occupation o, and a remote work score DN_o, we have:

$$\text{Telework}_j = \sum_{o=1}^{O} \frac{E_o \times \text{DN}_o}{E_N}.$$

We refer to the ex ante index as Telework (Telework_j), and the realized Scoop remoteness score as Remote (Remote_j). Figure 2 shows the relationship between our Telework and Remote variables. We observe a very strong relationship between these variables, with a β coefficient of 0.85, and an $R^2 = 0.25$, indicating that we can account for a fourth of the variation in whether firms go remote based on the variation in the remotability of their occupational mix. Moreover, the relationship appears fairly linear and monotone.

In the second stage, we then use the predicted level of firm remoteness to regress against other firm outcomes. Because our main specification focuses on a triple interaction, we include two instruments (Teleworkable $\times$ Post, and Teleworkable $\times$ Post $\times$ Startup) to proxy for two endogenous variables (Remote $\times$ Post and Remote $\times$ Post $\times$ Startup). Appendix Table A1 shows this full first stage specification. We observe substantial F-tests of around 123 in our baseline specification at the individual level, and 18 at the firm-level, which are above conventional cutoffs (Staiger and Stock, 2005).

3 Productivity and Remote Work

We begin by examining how remote work affects individual worker productivity, and whether these effects differ between startups and established firms. Our primary measure of productivity is the percentile rank of GitHub contributions at the user-half-year level, which captures the output of GitHub users in our sample. As discussed in Section 2 and validated in Appendix B, GitHub contributions provide a high-frequency, granular measure of individual productivity, with consistent validity across both the pre- and post-pandemic periods.

3.1 OLS Results for Individual Productivity

Table 2 presents our main OLS results on productivity effects, estimating the specification from Equation (1). Column (1) of this table shows OLS estimates on remote firms, without differentiating between startups and established firms. On average, our results indicate a modest negative and statistically insignificant relationship between remote work and productivity. Column (2) introduces the triple interaction term, which additionally explores heterogeneity between startups and non-startups. Our estimates reveal substantial heterogeneity: the coefficient on Remote $\times$ Post $\times$ Startup is positive and statistically significant, indicating that remote work increases productivity for startup employees by five percentage point ranks (out of one hundred) relative to employees at established firms. By contrast, the baseline coefficient on Remote $\times$ Post is negative and statistically significant. In economic terms, the productivity of established firms' remote workers declines by one percentage point in rank, indicating that the typical established firm experiences a statistically significant decline in worker productivity. Accounting for the baseline negative effects of remote work on productivity, startups have still a net positive impact of remote work on productivity. These trends persist in our baseline specification in column (3), which further controls for firm $\times$ individual fixed effects. To provide another measure

of magnitudes, column (4) shows the impact on the raw number of contributions, which increases for remote startups at around 25% of the pre-pandemic average.

Figure 3 Panel A shows an event study difference-in-differences design on worker productivity, highlighting the coefficient on Remote $\times$ Startup for each time period separately, relative to an omitted period prior to the pandemic. We observe no differential effects prior to the pandemic, while productivity increases by the second half of 2020, and persists through the end of our sample in the second half of 2022.

3.2 Instrumental Variables Results for Individual Productivity

While our OLS estimates provide evidence consistent with a role for remote work practices in adversely affecting productivity for large firms, while being positive for startups, they are subject to possible selection bias as a result of differential adoption of remote work. To address this concern, we present IV results in Table 3, which use the Dingel and Neiman (2020) measure of teleworkability, scaled at the firm level, to instrument for the firm's remote adoption rates. We observe a strong and linear first stage in this specification, as discussed in subsection 2.7. We report the F-statistics from that first stage at the bottom of each column and summarize them in Table A1.

Our IV results suggest a causal interpretation of our baseline estimates. Our estimates show that large firms adopting remote work exhibit lower productivity, while startups do not suffer the same penalty and experience a positive productivity effect overall. In our baseline specification in column (3), which includes firm $\times$ individual fixed effects, we find that established firms experience a decline of nine percentage points in firm rank when they go remote, while remote startups experience a relative statistically significant increase of twelve percentage points. The larger magnitudes of the IV estimates compared to the OLS are consistent with negative selection into remote work (i.e., firms experiencing productivity challenges being more likely to select into remote work), or measurement error in our remote work variable which attenuates the OLS estimates closer to zero (Jiang,

2017).

Taken together, our findings suggest that remote work has heterogeneous effects on firm productivity which depend critically on firm age. The negative effects of remote work on established firms is consistent with more recent efforts by large firms to bring workers back to the office.[6] Our results point to challenges for established companies in operating remotely, while more positive effects for startups suggest important competitive advantages for these firms.

3.3 Fundraising

While these results show benefits for remote startups in terms of individual productivity, an important question is whether these benefits result in broader benefits to the firm. This is a challenging question to answer for many of the startups and smaller firms in our sample, which are typically not public and so lack traditional accounting information.

To address this data limitation and examine some broader firm implications, we turn an analysis of fundraising which links our sample to capital raises from Crunchbase. Because this data source predominately covers venture capital and other capital raises for private firms, we restrict our focus in these specifications to private firms only. Under this restriction, we have a sample which has many fewer older incumbent firms, and is disproportionately comprised of startups. As a result, we interpret the results as being largely applicable to the startup population, and in the empirical specifications omit the interaction of remote and post period with age.

Table 4 shows the results of a specification which regresses remote status of a firm on different fundraising outcomes. Panel A shows results with the OLS specification, while Panel B shows results from our IV specification. The four columns highlight four different fundraising outcomes drawn from Crunchbase: (1) whether any capital was raised; (2)

[6] https://www.axios.com/2025/01/01/back-to-work-office-companies? lists several examples of established firms pushing for in-person policies.

whether the company raises a series A or later round; (3) the rank of fundraising within the same time period; and (4) the total amount raised in millions. We control for time and firm fixed effects across all of these specifications.

We find that firms which go remote experience increases in fundraising after the pandemic, consistent with the notion that remote work improves the productivity of these firms such that they are able to successfully attract more capital from venture sources. We find economically meaningful and statistically significant outcomes across specifications, suggesting that remote private firms are 2.2 percentage points more likely to raise capital in a given time period, and raise around $10 million in each period. They are also more likely to raise series A+ rounds, suggesting that fundraising success continues into the financially meaningful later stages, and they are also raise their relative rank in fundraising.

To be sure, improvements in fundraising may be the product of several attributes of remote work. Remote startups may potentially experience lower overhead from savings in office leases, which may present a more attractive financial profile for investors. A distributed workforce may signal certain attributes to investors such as flexibility or the potential for future scalability. These harder to measure benefits stand in addition to the benefits for individual worker productivity. However, they provide an additional piece of corroborative evidence that remote status appears to benefit startups, as evidenced by their favorable treatment in venture capital markets.

4 Mechanisms

4.1 Labor Market Constraints

The relative benefit of remote work for startups is consistent with startups facing significant scaling or hiring constraints (Ouimet and Zarutskie, 2014; Beckman and Burton, 2008; Gupta et al., 2024). Previous work has documented that startups and innovative activity are tightly clustered geographically (Kalyani et al., 2021; Moretti, 2021), and that large

firm hiring in a particular geography crowds-out startup employment (Bessen et al., 2023). Moreover, startups operate fewer locations relative to larger firms, which tend to have offices across several geographies across the U.S. or globally, thereby limiting the available pool of employees. These frictions are especially pronounced in major tech hubs where established firms have strong brands and existing recruiting pipelines. Remote work may expand the geographic scope of labor markets, reducing labor market frictions which bind more tightly for young firms, allowing startups to hire from a national or global talent pool rather than competing solely in expensive, competitive local markets.

To explore this mechanism, in Table 5 we highlight the impacts of remote work on labor market outcomes. Panel A highlights OLS results, while Panel B shows IV estimates. Columns (1)–(2) show the impact of remote work on overall employment growth at the firm level. We observe a small positive estimate in both the OLS and IV specifications that disguises considerable heterogeneity: startups grow substantially after adopting remote work, while larger firms do not. We confirm these results in an event study analysis in Panel B of Figure 3, which further shows persistence in this trend through the end of our sample in 2022. Importantly, this result is drawn from a large and comprehensive sample of firms, and so the inference is not subject to any possible limitations of our GitHub sample.

Columns (3)–(4) decompose this result into the two margins of joining and leaving. Remote startups see considerably higher joining rates than do remote established firms.[7] Large firms also see moderate but statistically significant leaving rates which offset the joining rates, contributing to net zero impacts on their aggregate employment. This finding points to important retention challenges of remote work, particularly for large firms. This may potentially be a product of attenuated corporate culture or other firm amenities, ease of interviewing for other positions while remote, or improved outside options for remote

[7]Appendix Figure A2 shows an event study for joining and leaving rates, while Appendix Figure A3 shows an event study for job postings and fill rates.

workers at large companies (such as entrepreneurship, as in Kwan et al. (2025)). Our estimates on departure rates for startups are comparable for large firms in magnitude, but the impact of joining is substantially larger. This suggests that, for startups, the dominant labor margin is increased labor market scaling opportunities.

Table 6 explores firms' labor demand, as measured by job postings and the number of hires per job posting. Because the joining rate is endogenous to both employee and employer decisions, looking at postings helps to isolate the specific role for employers. We find that established firms substantially decrease their job postings, and have a moderate rise in the number of hires per job posting. By contrast, startups experience a relative increase in the number of job postings, as well as a much larger increase in the number of hires per job posting. This suggests that startups are proactively responding to remote working opportunities by expanding their labor market searches.

4.2 Labor Market Channel as a Driver for Productivity

To directly estimate the role for labor market scaling in driving the productivity outcomes we observe, column (2) in Table 7 adds the remote-margin firm-growth channel to the baseline specification in column (1), Remote $\times$ Post $\times$ $\mathbb{1}$(Firm growth above median), together with its lower-order post term. The inclusion of this channel materially reduces the coefficient on Remote $\times$ Post $\times$ Startup by about half, indicating that a substantial component of the impact of remote work on startup productivity appears to come through benefits of labor market scaling. It is important to note that changes in productivity and the scaling of startups occur simultaneously, making it difficult to attribute causality to any single factor. However, the overall results suggest that the productivity gains are greatest for startups that experience the most significant scaling following the transition to remote work.

4.3 Equity Compensation Channel

Another channel through which remote startups might benefit is equity compensation. Startups rely on equity-heavy compensation to recruit employees (for instance, we observe startups are 11% more likely to offer equity compensation in Summary Table 1). Additionally, the incentive effects of equity compensation are likely to be largest in small firms in which each employee's effort is more likely to be pivotal to firm success (**??**). Remote startups may therefore be more able to hire high quality workers, who are attracted by these contracts, and the alignment of incentives may help remote startups cope with the loss in monitoring through in-person presence.

Column (3) of Table 7 adds the equity compensation channel to our baseline specification, including controls which interact remote status with an indicator for whether the firm offers equity compensation. We continue to instrument for all remote covariates in this specification with their telework equivalent. The addition of the equity compensation channel reduces the coefficient on the triple interaction of remote, post, and startup by over half.

Column (5) includes both the labor market scaling and equity compensation channels simultaneously. The coefficient on the triple interaction falls to essentially zero, indicating that together these two channels fully account for the differential productivity effects of remote work across firm age. Neither channel alone is sufficient, but their joint inclusion virtually entirely explains the remote startup productivity advantage.

4.4 Alternative Explanations

In addition to labor market scaling, explored in the previous section, there are other plausible mechanisms which may also explain the differential productivity success of remote startups.

To assess the relative importance of these mechanisms, we conduct a competing chan-

nels analysis which includes proxies for different channels which may impact remote work. Table 7 presents results from specifications that let these competing channels enter on the remote margin, adding Remote $\times \mathbb{1}(\text{Post}) \times Z$ together with the corresponding lower-order $\mathbb{1}(\text{Post}) \times Z$ term for the organizational centrality score.

In Table 7, the centrality-score channel does not produce comparable attenuation. This suggests that labor market scaling and equity compensation, appear more relevant for the differential success of remote startups than this alternative organizational-composition channel we measure.

4.5 Connecting Labor Market Scaling and Productivity

So far, our findings suggest that labor market scaling and productivity are related outcomes for remote startups. Next, we also directly connect these outcomes at the firm-level with three separate tests.

First, we examine the impact of transitioning to remote work on the wages that firms offer to their employees. The outcome variable is the logarithm of salary for each employee observed on GitHub. Wages are imputed by Revelio based on employees' position, firms, and locations.[8]

Table 8 presents the results. In our baseline specification, we find a decrease in salaries paid to employees at startups that transition to remote work in the post-period. Column (2) shows that this decrease ranges from 2 to 5 percent relative to the pre-remote salary level. This result is particularly notable given that we previously found productivity to rise among remote startup employees and that these startups expanded rapidly following the transition to remote work. A declining wage, in the context of significantly increased employment, points to improved labor market access.

[8]Revelio predicts the salary for each position based on role, seniority, company, and country using a regression-based model. The model is trained on over 200 million salaries from job postings and publicly available labor certification applications, and inflation rates at the country level are used to adjust salaries over time.

To better understand the source of this effect, column (3) further tightens the fixed effects by adding job-title fixed effects to the match specification, indicating that startups pay lower wages within the same role after going remote, so the decline is not driven by composition or promotion shifts. This pattern is consistent with startups expanding recruitment into new, typically lower-wage locations while keeping job descriptions comparable, thereby depressing average salaries despite higher productivity.

Second, Table 9 compares the outcomes for fully remote firms with those of all firms and with hybrid firms only. We find that established firms that transition to a fully remote work model experience a decline of 1 to 3 percentage points in employee productivity relative to hybrid or in-person firms. In contrast, startups that become fully remote exhibit an increase of 3 to 4 percentage points in employee productivity. These results are consistent with the notion that fully remote startups benefit from access to broader geographic labor markets, enabling higher productivity gains compared to startups that adopt only a hybrid model.[9]

Finally, in Appendix Figure A8 we highlight the results from a projection exercise which looks at the impact of hiring additional engineers for fully remote firms on employee productivity. We observe that productivity steadily rises in the period after the hiring for engineer hires, but not for non-engineer hires. This result is consistent with our notion that remote firms are able to access talent which has positive spillover effects on other workers' productivity in the firm.

4.6 Remote Work and Geography

To better measure the impact of geographical location on startup productivity, in Table 10 we estimate the effects of remote startup productivity across labor markets. We follow the top software engineer labor markets defined in Moretti (2021), who found substantial

[9]Appendix Figures A4 and A5 show event studies around our key outcomes using the binary definition of fully remote work.

clustering of workers in top metros, and alternately keep and exclude workers in the top five and ten CSAs.

Remote startups see the most positive productivity effects when dropping the top five and ten top CSAs, where Moretti (2021) emphasizes that such workers tend to be clustered. In other words, our estimates are driven by non-core markets where firms are able to tap into far larger geographical markets for hiring than previously. By contrast, the effects for remote startups are negative, though insignificant, in the top five markets, where firms previously enjoyed substantial access to employees. These effects suggest that geographical broadening of labor market pools can help explain our results.

5 Robustness

We further explore the robustness of our main findings by examining heterogeneity across workers more likely to have a preference for remote work and testing whether our results are driven by specific industries or occupations.

5.1 Gender

Table 11 explores how the productivity effects of remote work vary by gender. We focus on female employees, for whom prior literature has emphasized the importance of workplace flexibility (Mas and Pallais, 2017; Goldin, 2014) and thereby a preference for remote work (Banerjee et al., 2024). We find that women experience larger relative increases in productivity at remote startups, relative to men, consistent with the fact that female employees have a larger preference for this type of work arrangements and therefore benefit from them the most.

5.2 Occupation and Geography

We also explore the robustness of our estimates across occupational and geographical definitions. Table 12 addresses concerns that our results might be concentrated in technology firms or among workers in technical occupations, where remote work may be particularly suitable. We re-estimate our baseline specifications after excluding firms in the technology sector (column (1)), core technical occupations (column (2)), or firms headquartered in California and New York state which have a high concentration of technology firms in the U.S. (column (3)). Our effects are economically and statistically strong when excluding technology firms based on sector. We have weaker statistical precision when dropping technical firms based on occupation codes, though the magnitude remains similar to our baseline specification. Finally, our results are substantially stronger in economic and statistical terms when excluding California and New York, suggesting the effects are mainly driven by firms outside of these core markets. These results suggest that our findings are not limited to just firms in the technology sector, technical roles, or in large technology hubs, but relate to productivity more broadly across a range of firm types.

5.3 Employee Selection

To explore the role of employee selection in driving our results, we conduct two sets of analysis focused separately on employees who stay at or join firms over this period. In Appendix Table A3 we conduct a robustness test in which we subset on employees who stay at firms over the post-pandemic period.[10] The coefficient on the triple interaction of post-period remote startups is statistically significant and slightly larger in economic terms. This suggests that our results are not driven by new entrants to the firm, but instead come from existing employees. These workers appear to be more productive after the adoption of remote working practices by startups, perhaps due to positive spillovers from newly

[10]This specification uses firm and individual fixed effects separately, since firm $\times$ individual controls would be absorbed in this specification.

hired workers in these companies (as suggested by Appendix Figure A8).

We separately estimate the impacts on employees who join new firms over this period through an event study design, which compares the worker's relative productivity at their old company directly against their productivity when joining a remote startup or remote established firms in Appendix Figure A9. We see limited evidence that selection of ex ante more productive employees accounts for the higher productivity of remote startups: the baseline productivity differences for workers joining remote startups and remote established firms are comparable, and if anything the employees who join remote established firms are slightly more productive, although this effect is not statistically significant.

Individual productivity jumps for employees upon joining remote firms (around six percentile ranks for remote established firms, and nine percentile ranks for remote startups), before reverting in subsequent time periods. However, the productivity rate reverts back to the baseline at the prior firm by one year after hire for workers at remote established firms, while remaining five percentile points above baseline for remote startups. These results suggest that the productivity advantages for remote startups are not purely the result of selection of ex ante productive employees; but persist among stayers at the firm and appear to relate to positive treatment effects for new hires.

5.4 Contemporaneous Industry and Geographical Shocks

We also account for other contemporaneous shocks which may impact firms over this sample period. Column (1) of Table A4 includes industry *times* half-year fixed effects to the specification in column (3), Table 3, which help account for sector-specific shocks. These shocks may relate to differential technology adoption, regulatory burdens, labor market demand effects, or industry specific labor market factors, which may coincide with remote work transition. We observe quite similar effects in economic magnitude and significance, which mitigates the concern that concurrent industry-level shocks are

responsible for our baseline findings. Column (2) adds instead state $\times$ half-year fixed effects, which account for geographic variation in this time period. Such local-specific shocks capture geographical differences in pandemic restrictions, recovery rates from the Covid pandemic, housing market dynamics, and labor market flows. Our results are also quite similar in this specification. Finally, column (3) adds both sets of industry and state by time fixed effects, and we again observe very similar results in that specification. We conclude therefore that our results are robust to the consideration of other industry or local contemporaneous shocks.

6 Conclusion

This paper examines how remote work adoption affects firm productivity, focusing on heterogeneous impacts between startups and established firms. We use comprehensive data on individual worker productivity using GitHub, connected to firm-level remote work policies and labor market flows and instrument for remote work based on pre-pandemic occupational suitability. We find remote work substantially benefits startups while imposing costs on established firms. Employees at remote startups experience an increase in productivity, while those at established firms see declines in productivity.

These results are amplified for workers with a larger preference for remote work, persist among non-tech firms, are stronger in non-core business markets, and are robust to considering other contemporaneous shocks. They also persist in fundraising advantages for private firms. These findings highlight the importance of organizational context when evaluating the impacts of remote work.

We identify access to a greater labor market pool as one mechanism to explain these differential effects. Remote startups experience substantially higher labor market scaling, with higher hiring rates and employee growth compared to established firms. This suggests that geographic expansion of labor markets particularly benefits young firms previously

constrained in their ability to compete for talent in concentrated tech hubs. By contrast, established firms struggle with employee retention under remote work, with increased departure rates offsetting modest gains in hiring. These labor market scaling effects appear to account for around half of the productivity gains for remote startups, pointing to the role of knowledge spillovers and complementarities among remote workers which enhance firm-level output.

We can account for the other half of productivity advantages for remote startups through equity compensation. Because startups offer more equity compensation and each worker's stake represents a meaningful claim on firm value, equity pay serves as a stronger incentive in small organizations where the free-rider problem is less severe. Together, labor market scaling and equity compensation fully account for the startup productivity premium, pointing to complementary channels through which remote work alleviates frictions faced by young firms, and caters to their organizational and compensation advantaes.

Our findings have important implications for the future of business dynamism and innovation. By reducing geographical frictions in labor markets, remote work appears to level the playing field between young and established firms, which may potentially reverse long-standing trends towards declining business dynamism, as documented by Decker et al. (2016). The changing composition of workers across firms as well as productivity benefits also has implications for the geographic distribution of innovation activity, by helping to connect workers across labor markets into a connected national pool.

At the same time, our estimates of negative productivity and labor market effects of remote work for established firms points to important monitoring and governance challenges faced by larger and more established companies which may act as a headwind towards greater adoption of remote working practices.

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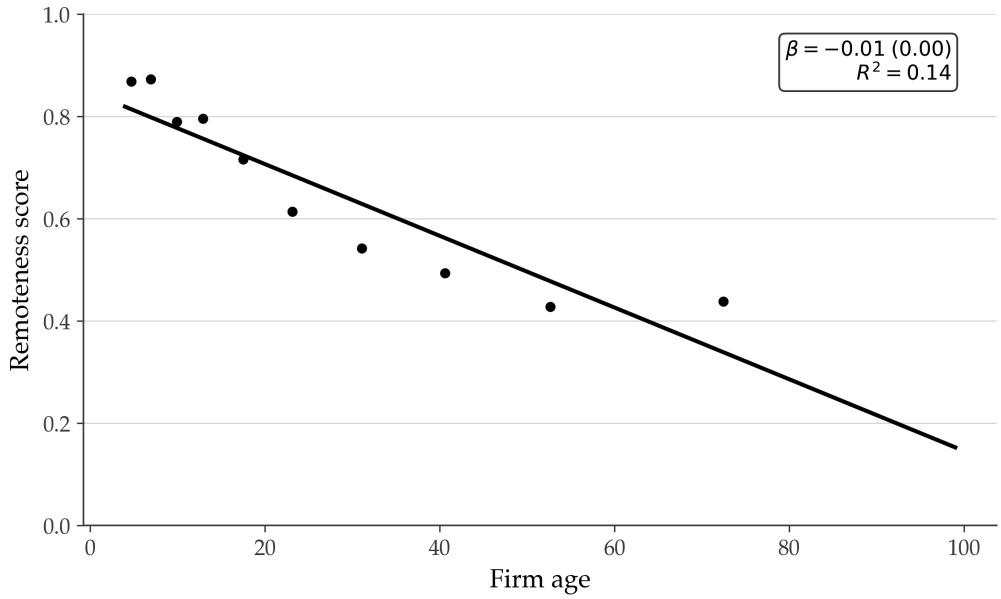
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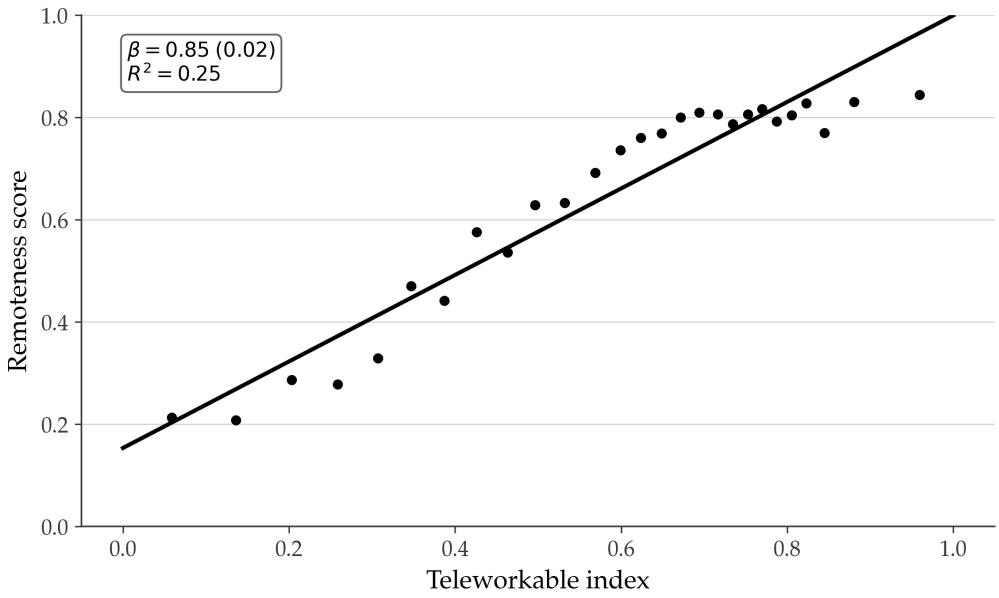
Figures

Figure 1: Firm Age and Remote Work



Notes: Built from the pre-2020 Scoop–Revelio firm panel, the figure bins firm age (capped at 100 years) into equal-frequency groups and reports the corresponding mean remoteness score observed during the remote-work period, together with a binscatter fit following Cattaneo et al. (2024).

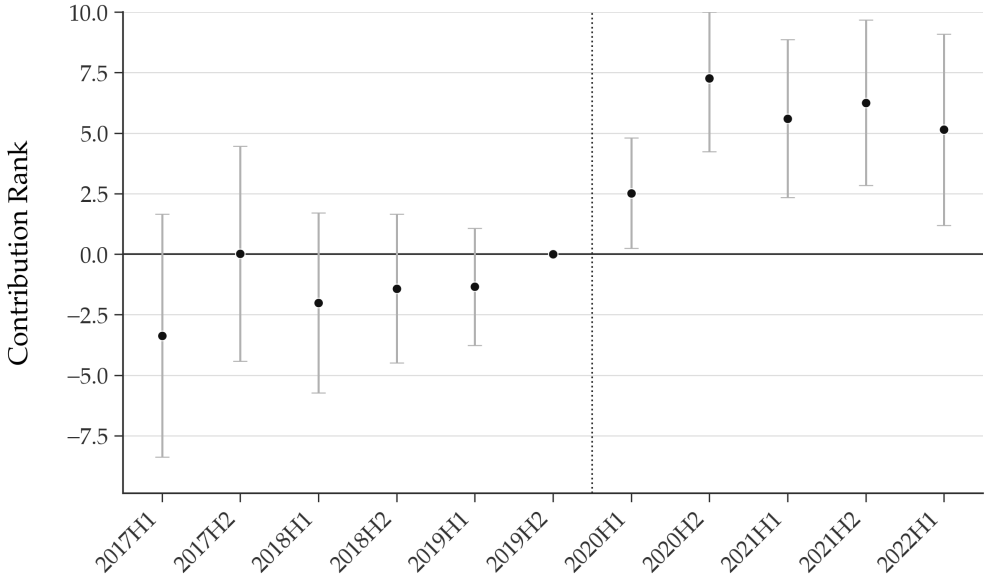
Figure 2: Telework Index and Remote Work



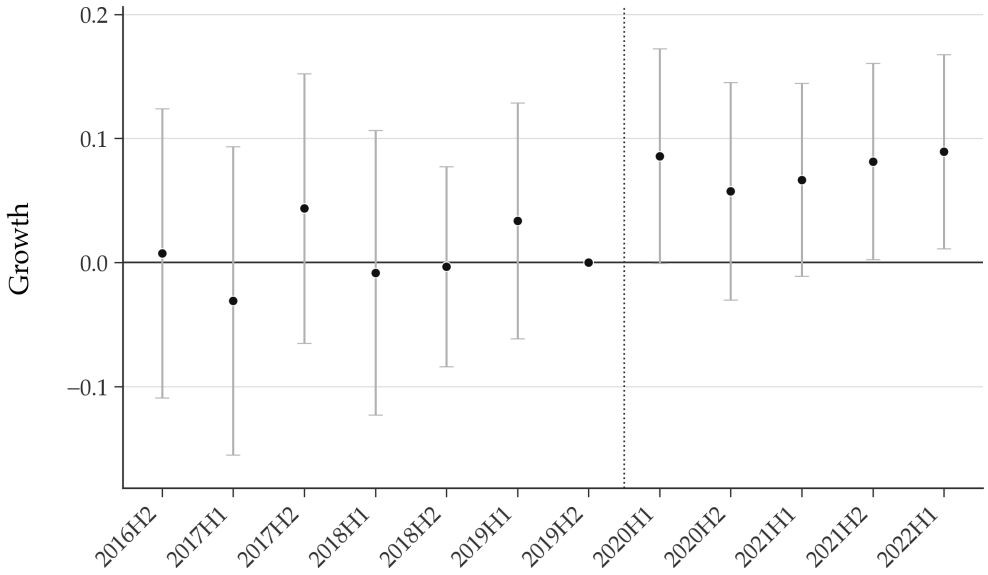
Notes: Occupation-level teleworkability scores, following Dingel and Neiman (2020), are aggregated to the firm using pre-pandemic Revelio employment weights (sourced from LinkedIn profiles) and paired with the remoteness score used in the analysis. Equal-frequency bins and the fitted line summarize the association between the ex-ante teleworkability index and subsequent remoteness, using the binscatter approach of Cattaneo et al. (2024).

Figure 3: Event Study of Key Outcomes

Panel A: User Productivity



Panel B: Firm Growth



Notes: Plots the coefficients β_τ from the worker- and firm-level OLS event-study $y_{it} = \sum_{\tau \neq 2019H2} \beta_\tau (\text{Remote}_j \times \text{Startup}_j \times \mathbb{1}\{t = \tau\}) + \sum_{\tau \neq 2019H2} \gamma_\tau (\text{Remote}_j \times \mathbb{1}\{t = \tau\}) + \sum_{\tau \neq 2019H2} \delta_\tau (\text{Startup}_j \times \mathbb{1}\{t = \tau\}) + \alpha_i + \phi_j + \theta_t + \varepsilon_{it}$, where Remote_j is the Scoop remoteness score and Startup_j flags firms aged $\leq$10 in 2020. Panel A uses the 2017H1–2022H1 Scoop–Revelio–GitHub worker-half-year panel with worker and firm fixed effects; Panel B uses the 2016H2–2022H1 Scoop–Revelio firm-half-year panel with firm fixed effects. Both specifications absorb half-year effects θ_t, omit 2019H2, and cluster standard errors at the unit level; shaded bands show 95% confidence intervals.

Tables

Table 1: Summary Statistics

	Startup	Established	All Firms
Panel A: Firm-level			
Growth	0.20	0.06	0.09
	(0.31)	(0.16)	(0.22)
Leave	0.26	0.21	0.22
	(0.31)	(0.28)	(0.29)
Join	0.35	0.17	0.22
	(0.32)	(0.18)	(0.24)
Teleworkable Score (0–1)	0.67	0.54	0.57
	(0.18)	(0.25)	(0.24)
Remote Score (0–1)	0.85	0.57	0.64
	(0.30)	(0.41)	(0.40)
Offers Equity Compensation (%)	18.22	16.39	16.85
	(38.63)	(37.02)	(37.43)
Employees (Count)	271	2740	2126
	(1432)	(9555)	(8380)
Age	7	43	34
	(2)	(34)	(33)
Centrality Score	1419	949	1066
	(1830)	(1309)	(1470)
Number of firms	878	2630	3508
Observations	10450	31530	41980
Panel B: Individual-level			
Total Contributions	362.95	192.73	225.70
	(817.44)	(522.28)	(594.89)
Restricted Contributions	320.05	139.96	174.84
	(746.65)	(359.56)	(466.14)
Number of firms	721	1503	2224
Number of individuals	8338	31993	37186
Observations	44801	186504	231305

Notes: Columns correspond to firm types: "Startup" covers employers aged $\leq$10 in 2020, "Established" covers the rest, and "All Firms" pools them; all entries report means with standard deviations in parentheses. Panel A summarizes the 2016H2–2022H1 Scoop–Revelio firm-half-year panel, where Growth/Join/Leave are winsorized ratios of Revelio inflows or outflows to the prior half-year headcount. Teleworkable is a 0–1 score derived from the pre-2020 occupational mix, Remote is a measure of how much firms allow employees to work remotely, and Offers Equity Compensation (%) is the share of firms in each column that ever display a positive equity-compensation signal in the LLM-coded postings panel over the sample window. The remaining rows describe firm size, age, location costs, and organizational centrality. Panel B repeats the exercise for the 2017H1–2022H1 Scoop–Revelio–GitHub worker-half-year panel, so the reported outcomes are workers' total and restricted GitHub contributions (counts per half-year) split by the startup status of their employer in 2020. "Number of firms" counts distinct employers within each column (so startup and established tallies need not sum to the pooled total), "Number of individuals" tracks distinct workers assigned to those employers, and "Observations" refers to firm- or worker-level half-year records in each panel.

Table 2: Remote Work and Individual Productivity – OLS

	Contribution Rank			Total
	(1)	(2)	(3)	(4)
Panel A: OLS				
Remote × 𝟙(Post)	-0.28	-1.03**	-1.23**	-19.74***
	(0.44)	(0.48)	(0.50)	(4.66)
Remote × 𝟙(Post) × Startup	–	5.18***	6.21***	59.23***
		(1.24)	(1.27)	(14.22)
Pre-Covid Mean	49.92	49.92	49.92	184.71
N	229,862	229,862	224,708	224,708
Fixed Effects				
Half-year	✓	✓	✓	✓
Firm	✓	✓		
Individual	✓	✓		
Firm × Individual			✓	✓

Notes: Columns (1)–(4) estimate the worker-level design on the 2017H1–2022H1 Scoop–Revelio–GitHub panel using OLS. Remote records the remoteness score (the share of the standard workweek that can be worked off-site); 𝟙(Post) equals one from 2020H1 onward and Startup equals one for firms aged ten years or less in 2020. The post×startup main effect enters every column. Column (1) absorbs separate firm and individual fixed effects, column (2) adds the startup interaction, and columns (3)–(4) replace those effects with firm×individual match fixed effects while column (4) switches the outcome to winsorized total contributions. All specifications include half-year fixed effects and cluster standard errors by worker.

Table 3: Remote Work and Individual Productivity – IV

	Contribution Rank			Total
	(1)	(2)	(3)	(4)
Panel B: IV				
Remote × 𝟙(Post)	-3.61	-7.15*	-9.26**	-124.39***
	(2.82)	(3.90)	(4.01)	(33.82)
Remote × 𝟙(Post) × Startup	–	9.94*	12.45**	130.92**
		(5.37)	(5.39)	(55.14)
Pre-Covid Mean	49.92	49.92	49.92	184.71
KP rk Wald F	543.26	140.60	123.43	123.43
N	229,862	229,862	224,708	224,708
Fixed Effects				
Half-year	✓	✓	✓	✓
Firm	✓	✓		
Individual	✓	✓		
Firm × Individual			✓	✓

Notes: Two-stage least squares counterpart to Table 2. Remote × 𝟙(Post) and Remote × 𝟙(Post) × Startup are instrumented with Teleworkable × 𝟙(Post) and Teleworkable × 𝟙(Post) × Startup, where the teleworkable index is computed once from the firm's 2019 occupational mix. Remote denotes the remoteness score. The post×startup main effect remains included. Column-specific fixed effects match Table 2, half-year fixed effects are always absorbed, and standard errors are clustered by worker. Reported Kleibergen–Paap Wald F statistics evaluate the joint relevance of the two instruments.

Table 4: Crunchbase Fundraising Outcomes

	Any USD raised	Series A+ round	USD raised rank	USD raised (mil)
	(1)	(2)	(3)	(4)
Panel A: OLS				
Remote $\times \mathbb{1}(\text{Post})$	0.008	0.027***	0.784	6.35**
	(0.006)	(0.006)	(0.558)	(2.65)
Pre-Covid Mean	0.080	0.067	8.662	4.03
N	26,439	26,439	26,439	26,439
Panel B: IV				
Remote $\times \mathbb{1}(\text{Post})$	0.022**	0.036***	2.060**	10.09**
	(0.010)	(0.011)	(0.971)	(4.48)
KP rk Wald F	748.54	748.54	748.54	748.54
N	26,439	26,439	26,439	26,439
Fixed Effects				
Time	✓	✓	✓	✓
Firm	✓	✓	✓	✓

Notes: Uses fundraising outcomes from Crunchbase, restricting to Crunchbase-matched private firms. Column outcomes are (1) indicator for any positive USD raised in the half-year, (2) indicator for any Series A+ round in the half-year, (3) within-half-year rank of USD raised, and (4) USD raised in levels (millions; includes zeros). Panels A and B report OLS and IV estimates of Remote $\times \mathbb{1}(\text{Post})$ with firm and half-year fixed effects and firm-clustered standard errors; the IV instruments Remote $\times \mathbb{1}(\text{Post})$ with Teleworkable $\times \mathbb{1}(\text{Post})$. This table uses the single-term specification and does not include startup-interaction terms.

Table 5: Firm Scaling

	Growth		Join	Leave
	(1)	(2)	(3)	(4)
Panel A: OLS				
Remote $\times$ $\mathbb{1}$(Post)	0.01**	0.00	0.01**	0.02***
	(0.01)	(0.00)	(0.00)	(0.00)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	–	0.07***	0.05*	-0.01
		(0.02)	(0.03)	(0.01)
Pre-Covid Mean	0.11	0.11	0.24	0.14
N	41,980	41,980	41,980	41,980
Panel B: IV				
Remote $\times$ $\mathbb{1}$(Post)	0.02*	0.01	0.04***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	–	0.21**	0.23**	0.09
		(0.10)	(0.11)	(0.06)
KP rk Wald F	839.65	16.53	16.53	16.53
N	41,980	41,980	41,980	41,980
Fixed Effects				
Time	✓	✓	✓	✓
Firm	✓	✓	✓	✓

Notes: Columns (1)–(4) use the 2016H2–2022H1 Scoop–Revelio firm-half-year panel in which LinkedIn flows are scaled by the prior headcount to form winsorized growth, join, and leave measures. Remote is the remoteness score observed in the Scoop snapshot paired with these outcomes; $\mathbb{1}$(Post) equals one from 2020H1 onward and Startup marks firms aged ten years or less in 2020. Column (1) omits the startup interaction, while columns (2)–(4) include it and vary the outcome across net growth, joins, and leaves. The remote interactions are instrumented with teleworkability-based counterparts formed from 2019 occupations, the post$\times$startup main effect is always included, firm and half-year fixed effects are absorbed, and standard errors are clustered by firm.

Table 6: Job Posting Outcomes

	Postings	Hires/ Posting	Any Postings
	(1)	(2)	(3)
Panel A: OLS			
Remote $\times$ $\mathbb{1}$(Post)	-0.91***	0.09	0.06***
	(0.23)	(0.08)	(0.01)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	0.63**	0.37*	-0.02
	(0.27)	(0.22)	(0.04)
Pre-Covid Mean	0.50	2.15	0.76
N	37,681	27,790	37,681
Panel B: IV			
Remote $\times$ $\mathbb{1}$(Post)	-3.58***	0.38**	0.06**
	(0.61)	(0.17)	(0.02)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	1.60*	2.16**	0.23
	(0.96)	(1.09)	(0.18)
KP rk Wald F	10.66	9.04	10.66
N	37,681	27,790	37,681
Fixed Effects			
Time	✓	✓	✓
Firm	✓	✓	✓

Notes: Estimates same firm-level design from Table 5 on job-posting outcomes. Column (1) reports the raw Scoop posting counts (expressed in thousands). Column (2) tracks hires-per-posting ratios that are constructed from Revelio join flows divided by postings, require at least three postings in the half-year, and are winsorized at the 5th/95th percentiles. Column (3) is an indicator for any postings. Remote, Startup, the teleworkability-based instruments, fixed effects, and firm-clustered standard errors mirror the growth specification.

Table 7: Individual Productivity Mechanisms

	Contribution Rank				
	(1)	(2)	(3)	(4)	(5)
Panel B: IV					
Remote $\times \mathbb{1}(\text{Post})$	-9.26**	-9.43	0.82	-6.57	3.77
	(4.01)	(9.53)	(8.53)	(5.39)	(16.35)
Remote $\times \mathbb{1}(\text{Post}) \times$ Startup	12.45**	7.79	4.96	14.68**	0.09
	(5.39)	(6.53)	(8.95)	(5.76)	(9.68)
N	224,708	224,708	179,510	224,596	179,510
KP rk Wald F	123.43	36.59	13.18	58.91	14.36
Fixed Effects					
Time	✓	✓	✓	✓	✓
Firm $\times$ Individual	✓	✓	✓	✓	✓
Controls					
Firm Growth		✓			✓
Equity Compensation			✓		✓
Centrality Score				✓	

Notes: All columns estimate the worker-level productivity design from Table 3 on the 2017H1–2022H1 Scoop–Revelio–GitHub panel, absorbing worker$\times$firm match effects and half-year fixed effects and clustering standard errors by worker. The table reports only the two-stage least squares estimates of Remote $\times \mathbb{1}(\text{Post})$ and Remote $\times \mathbb{1}(\text{Post}) \times$ Startup. Whenever a competing channel Z is added, the regression also includes the lower-order $\mathbb{1}(\text{Post}) \times Z$ term, and the IV augments the excluded-instrument set with Teleworkable $\times \mathbb{1}(\text{Post}) \times Z$. Column (1) reports the baseline specification. Column (2) adds the firm-growth channel, Remote $\times \mathbb{1}(\text{Post}) \times \mathbb{1}(\text{Firm growth above median})$, together with $\mathbb{1}(\text{Post}) \times \mathbb{1}(\text{Firm growth above median})$; firm growth is defined from post-2020 averages of winsorized firm-half-year headcount growth. Column (3) adds the equity channel, Remote $\times \mathbb{1}(\text{Post}) \times \mathbb{1}(\text{Offers equity})$, together with $\mathbb{1}(\text{Post}) \times \mathbb{1}(\text{Offers equity})$. Column (4) adds the centrality-score channel, a Herfindahl-based measure of occupational-role concentration, on the remote margin. Column (5) includes both the firm-growth and equity channels. Equity-bearing columns use the postings-covered equity support: firms with postings but no equity-related keyword hits are coded as zero, firms with keyword hits but no parse-successful LLM output are excluded, and firms with no postings coverage are excluded. The checkmarks indicate which channels are active in each column.

Table 8: Remote Work and Imputed Wages

	Log salary		
	(1)	(2)	(3)
Panel A: OLS			
Remote $\times$ $\mathbb{1}$(Post)	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	-0.00	-0.02**	-0.00
	(0.01)	(0.01)	(0.01)
Pre-Covid Mean	11.49	11.49	11.49
N	229,862	224,708	222,531
Panel B: IV			
Remote $\times$ $\mathbb{1}$(Post)	-0.01	0.03	0.08***
	(0.02)	(0.02)	(0.01)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	-0.04	-0.05*	-0.08***
	(0.03)	(0.03)	(0.02)
KP rk Wald F	140.60	123.43	127.26
N	229,862	224,708	222,531
Fixed Effects			
Time	✓	✓	✓
Firm	✓		
Individual	✓		
Firm $\times$ Individual		✓	✓
Title			✓

Notes: Dependent variable is log(salary) for the Scoop–Revelio–GitHub worker panel restricted to positive Revelio wage imputations. Panels A and B report, respectively, the OLS benchmark and its IV counterpart that instruments Remote $\times$ $\mathbb{1}$(Post) and Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup with the teleworkability$\times$post (and $\times$startup) interactions described in Table 3. Every specification retains the post$\times$startup main effect. Column (1) absorbs half-year, firm, and individual fixed effects; column (2) swaps the separate firm and individual effects for firm$\times$worker match effects; and column (3) augments the match specification with job-title fixed effects. Standard errors are clustered by worker.

Table 9: Fully Remote Versus Hybrid Productivity

	Contribution Rank			
	Hybrid		Hybrid / In-Person	
Sample Filter	(1)	(2)	(3)	(4)
Panel A: OLS				
$\mathbb{1}(\text{Remote}) \times \mathbb{1}(\text{Post})$	-0.97***	-0.76**	-0.86***	-0.67**
	(0.32)	(0.31)	(0.31)	(0.30)
$\mathbb{1}(\text{Remote}) \times \mathbb{1}(\text{Post}) \times \text{Startup}$	2.97***	2.44***	3.39***	2.81***
	(0.72)	(0.71)	(0.71)	(0.69)
Pre-Covid Mean	50.04	50.04	49.92	49.92
N	210,721	215,332	224,708	229,862
Panel B: IV				
$\mathbb{1}(\text{Remote}) \times \mathbb{1}(\text{Post})$	-3.72**	-2.71*	-4.11**	-3.09*
	(1.74)	(1.64)	(1.77)	(1.68)
$\mathbb{1}(\text{Remote}) \times \mathbb{1}(\text{Post}) \times \text{Startup}$	4.37*	3.39	5.61**	4.38*
	(2.50)	(2.45)	(2.45)	(2.41)
KP rk Wald F	292.85	351.60	311.51	371.38
N	210,721	215,332	224,708	229,862
Fixed Effects				
Time	✓	✓	✓	✓
Firm		✓		✓
Individual		✓		✓
Firm × Individual	✓		✓	

Notes: Uses the Scoop–Revelio–GitHub worker panel and recasts the remoteness score into the binary treatment $\mathbb{1}(\text{Remote}_j)$, which equals one for Scoop-classified fully flexible firms and zero otherwise. Column (1) restricts the sample to structured-hybrid firms ($0 < \textit{Remote} < 1$) plus the $\mathbb{1}(\text{Remote}_j)$ group; column (2) retains all observations so the control group pools hybrid and in-person firms. Panels A and B follow the OLS/IV setup described for Table 3, including the teleworkability-based instruments, firm×individual and half-year fixed effects, and worker-clustered standard errors.

Table 10: Remote Work and Geography

	Keep CSAs Ranked 1–5	Keep CSAs Ranked 1–10	Drop CSAs Ranked 1–5	Drop CSAs Ranked 1–10
	(1)	(2)	(3)	(4)
Panel A: OLS				
Remote × 𝟙(Post)	-1.54**	-1.34**	-0.58	-1.48
	(0.68)	(0.57)	(0.75)	(1.00)
Remote × 𝟙(Post) × Startup	5.59***	5.50***	7.44***	8.62***
	(1.64)	(1.45)	(2.16)	(2.73)
Pre-Covid Mean	49.73	50.50	50.28	48.03
N	151,831	174,809	71,906	49,211
Panel B: IV				
Remote × 𝟙(Post)	-4.43	-7.52	-14.02*	-7.81**
	(5.20)	(9.92)	(7.52)	(3.45)
Remote × 𝟙(Post) × Startup	-10.35	8.43	22.97***	13.06**
	(15.95)	(10.91)	(8.74)	(5.47)
Pre-Covid Mean	49.73	50.50	50.28	48.03
KP rk Wald F	9.60	20.76	26.29	185.42
N	151,831	174,809	71,906	49,211
Fixed Effects				
Time	✓	✓	✓	✓
Firm × Individual	✓	✓	✓	✓

Notes: Displays the firm×individual fixed-effect specification of the worker regressions from Tables 2–3 under alternative CSA scopes, using each firm's primary CBSA (the Scoop-identified location with the most employment spells in a half-year). The "top CSAs" refer to the fourteen large software hubs. Column (1) keeps CSAs ranked 1–5; column (2) keeps CSAs ranked 6–14; column (3) drops CSAs ranked 1–5; column (4) drops CSAs ranked 1–14. Panels A and B follow the Table 3 OLS/IV setup with teleworkability-based instruments, half-year fixed effects, the post×startup main effect, and worker-clustered standard errors.

Table 11: Productivity Heterogeneity by Worker Traits

	Contribution Rank	
	(1)	(2)
Panel A: OLS		
Remote × 𝟙(Post)	-1.03**	-1.23**
	(0.48)	(0.50)
Remote × 𝟙(Post) × Female	-2.54***	-2.18***
	(0.62)	(0.66)
Remote × 𝟙(Post) × Startup	5.18***	6.21***
	(1.24)	(1.27)
Remote × 𝟙(Post) × Female × Startup	3.49	6.42*
	(3.41)	(3.85)
Panel B: IV		
Remote × 𝟙(Post)	-7.15*	-9.26**
	(3.90)	(4.01)
Remote × 𝟙(Post) × Female	-2.75***	-2.37***
	(0.72)	(0.77)
Remote × 𝟙(Post) × Startup	9.94*	12.45**
	(5.37)	(5.39)
Remote × 𝟙(Post) × Female × Startup	5.67	21.98*
	(10.98)	(11.66)
Fixed Effects		
Time	✓	✓
Firm	✓	
Individual	✓	
Firm × Individual		✓

Notes: Modifies the worker-level design from Tables 2–3 by augmenting the specification with 𝟙(Post) × Female, *Remote* × 𝟙(Post) × Female, and *Remote* × 𝟙(Post) × Female × Startup; Female equals one when Revelio's résumé classifier infers a woman. Panels A and B follow the Table 3 OLS/IV setup, so the remote terms are instrumented with their teleworkability-based counterparts. The checkmarks indicate whether we absorb firm×individual or separate firm and individual effects (half-year fixed effects are always included), and standard errors are clustered by worker.

Table 12: Remote Work Outside Software and Hub Markets

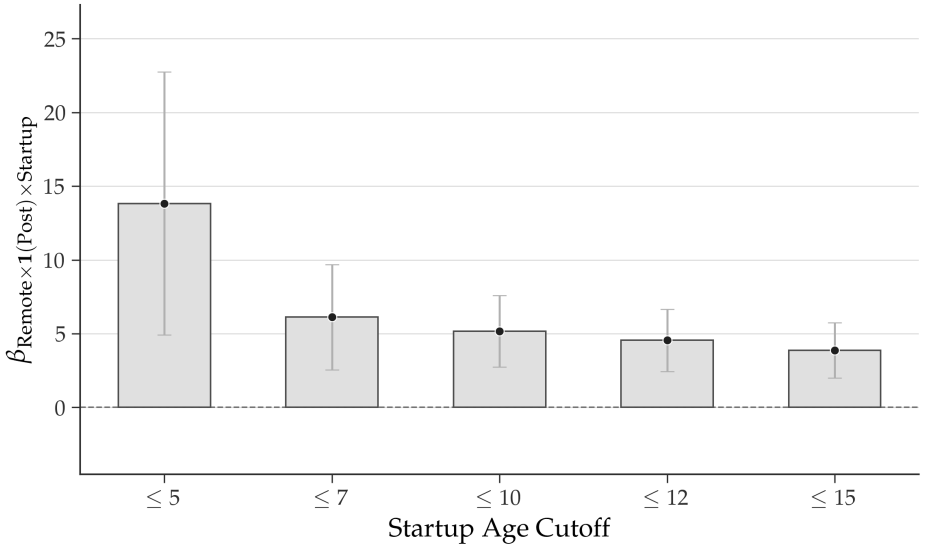
	Contribution Rank		
	Drop NAICS	Drop SOC	Drop CA/NY
Sample Filter	(1)	(2)	(3)
Panel A: OLS			
Remote × 𝟙(Post)	-1.16**	-0.58	-0.85
	(0.52)	(0.70)	(0.69)
Remote × 𝟙(Post) × Startup	7.08***	6.47***	6.47***
	(1.38)	(1.89)	(2.08)
Pre-Covid Mean	49.11	48.47	49.01
N	207,857	123,122	109,314
Panel B: IV			
Remote × 𝟙(Post)	-10.47**	-2.58	-12.70**
	(4.25)	(5.17)	(5.18)
Remote × 𝟙(Post) × Startup	13.07**	7.78	30.95***
	(5.63)	(7.45)	(10.98)
KP rk Wald F	111.33	70.48	72.09
N	207,857	123,122	109,314
Fixed Effects			
Time	✓	✓	✓
Firm × Individual	✓	✓	✓

Notes: Re-estimates the worker-level specification from Tables 2–3 on three filtered versions of the 2017H1–2022H1 Scoop–Revelio–GitHub panel to demonstrate robustness outside software-intensive settings. Column (1) drops workers employed by NAICS 54151x software-development firms, column (2) excludes jobs whose current SOC is on the canonical software list, and column (3) omits observations located in California or New York. Panels A and B follow the Table 3 OLS/IV setup. All regressions absorb firm×individual and half-year fixed effects, include the post×startup main effect, and cluster standard errors by worker.

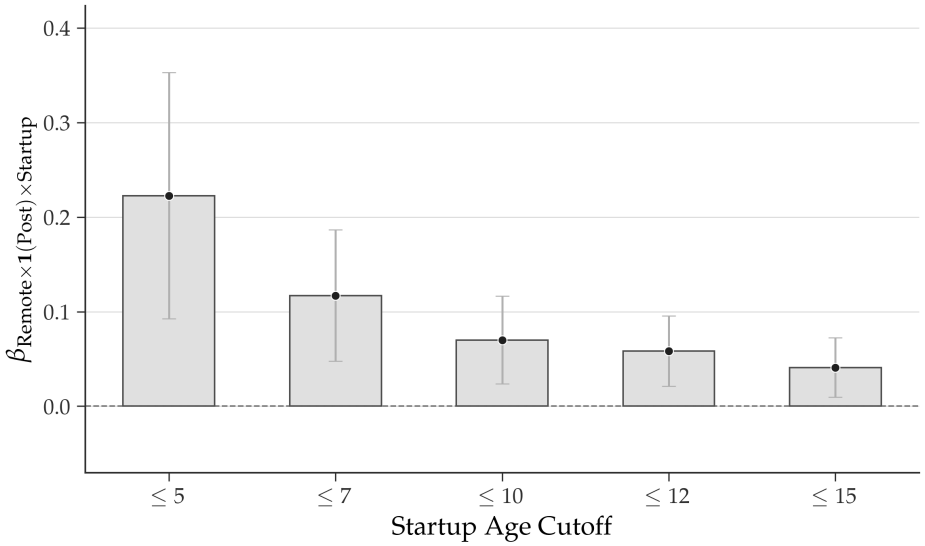
A Appendix: Additional Results

Figure A1: Startup Cutoffs

Panel A: Contribution Rank (Total)



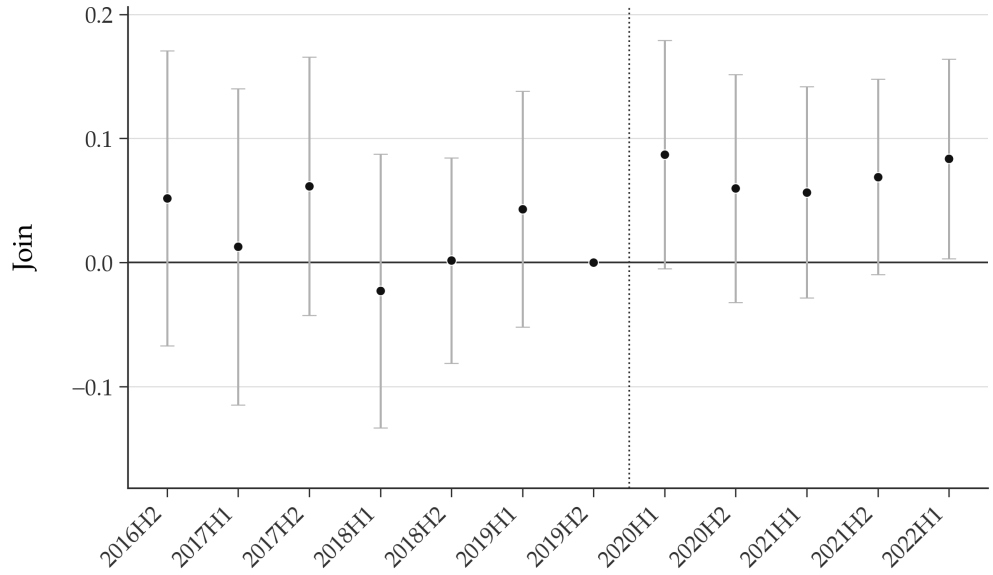
Panel B: Firm Growth Rate



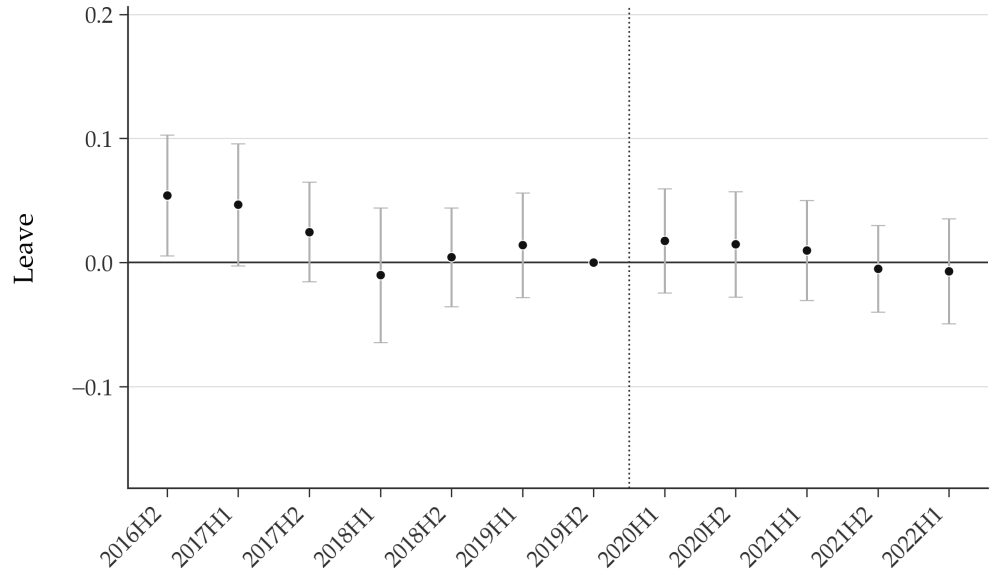
Notes: Bars report the estimated coefficient on Remote $\times$ $\mathbb{1}(\text{Post}) \times$ Startup when the startup indicator is redefined using alternative 2020 age cutoffs ($\leq 5, 7, 10, 12$, and 15 years). Panel A draws on the 2017H1–2022H1 Scoop–Revelio–GitHub worker panel with the baseline OLS specification from Table 2 and the total contribution rank outcome; Panel B repeats the OLS firm-scaling design from Table 5 on the 2016H2–2022H1 Scoop–Revelio panel with growth as the outcome. Error bars denote 95% confidence intervals with standard errors clustered at the worker (Panel A) or firm (Panel B) level, and every specification includes the post$\times$startup main effect plus the usual fixed effects.

Figure A2: Event Study of Labor Market Flows

Panel A: Firm Join



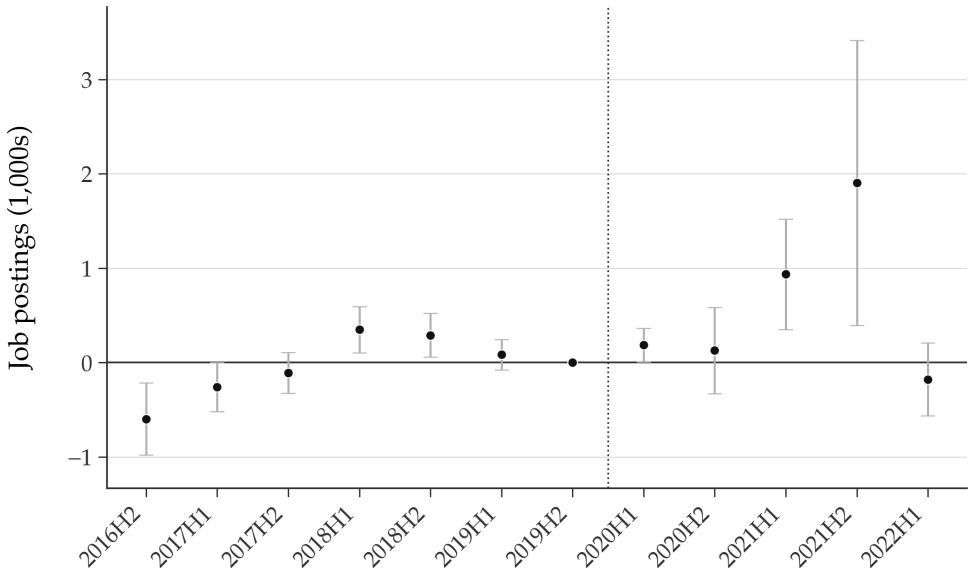
Panel B: Firm Leave



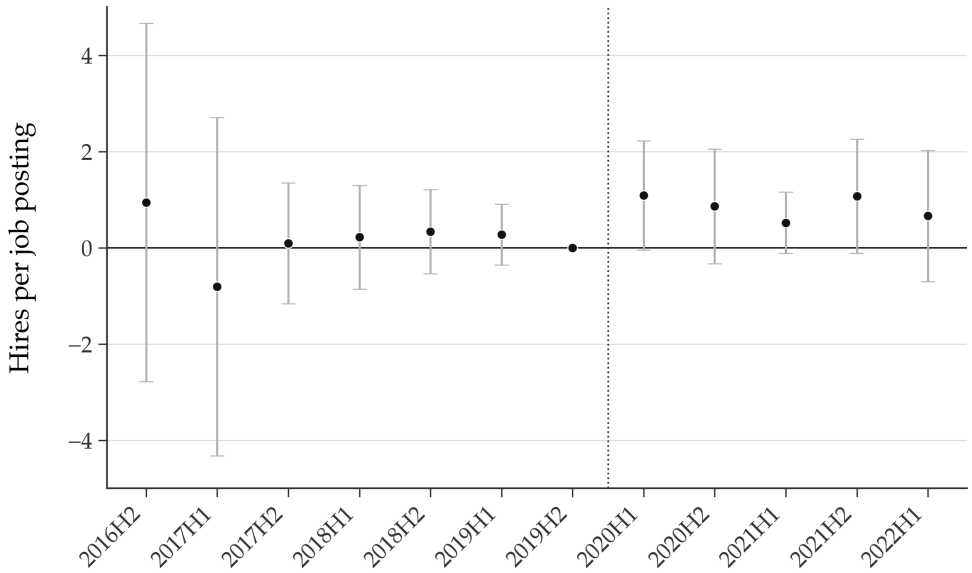
Notes: Uses the specification in Figure 3 but switches the outcomes to winsorized join and leave rates (LinkedIn flows divided by the prior headcount) in the 2016H2–2022H1 Scoop–Revelio firm panel. Remote remains the continuous remoteness score, Startup flags firms aged $\leq$10 in 2020, firm and half-year fixed effects and the 2019H2 baseline are unchanged, and standard errors are clustered by firm; the lines plot the Remote$\times$Startup$\times\mathbb{1}\{t = \tau\}$ coefficients with 95% confidence intervals.

Figure A3: Event Study of Job Postings and Fill Rates

Panel A: Job Postings



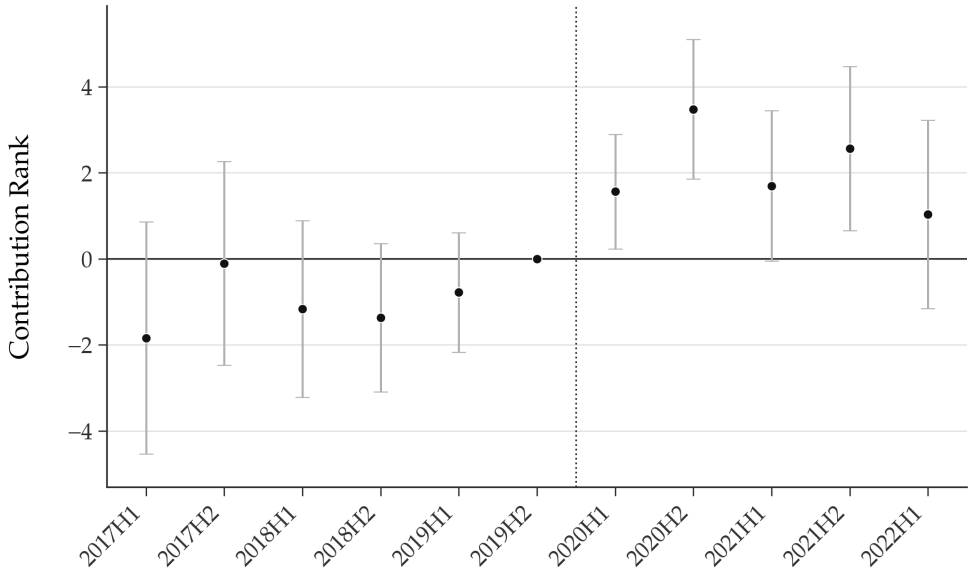
Panel B: Hires per Posting



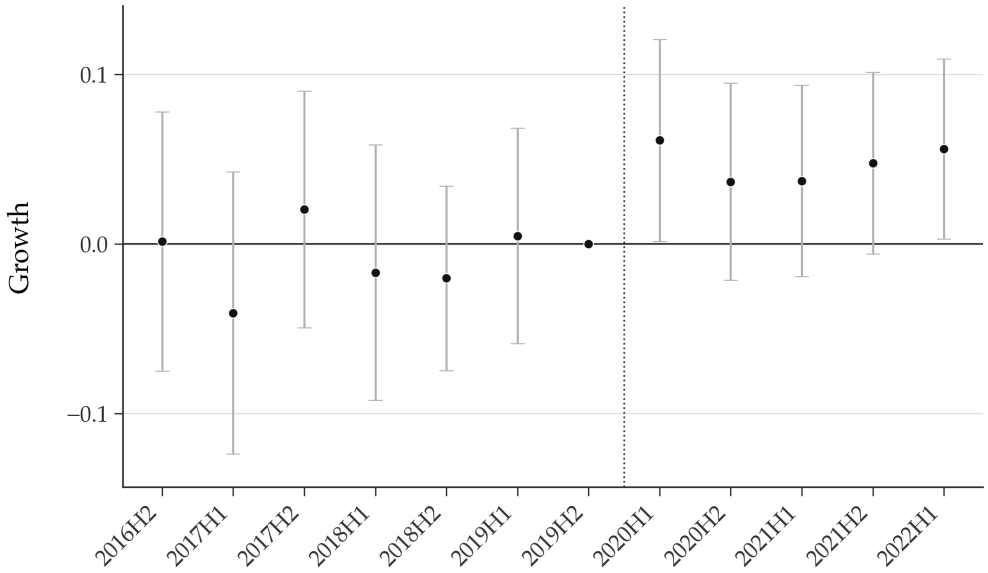
Notes: Again applies the Figure 3 design to the 2016H2–2022H1 Scoop–LinkedIn firm panel, but the outcomes are Scoop job postings (thousands) and LinkedIn hires-per-posting ratios, each winsorized at the 1st/99th percentiles. Remote and Startup are defined as before, firm and half-year fixed effects plus the 2019H2 baseline remain in place, and standard errors are clustered by firm; the plotted coefficients are Remote$\times$Startup$\times\mathbb{1}\{t=\tau\}$ with 95% confidence intervals.

Figure A4: Event Study of Fully Remote Key Outcomes

Panel A: User Productivity



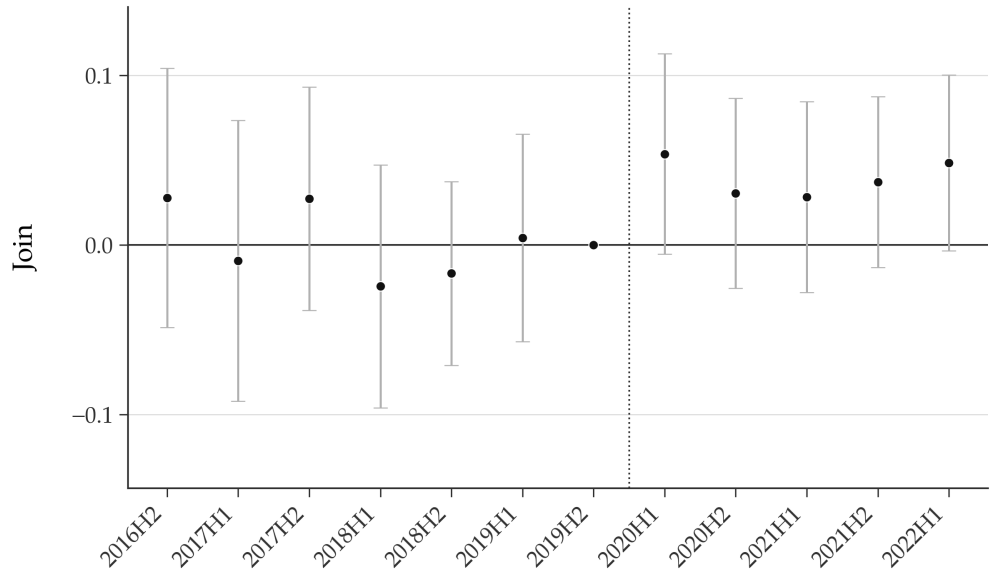
Panel B: Firm Growth



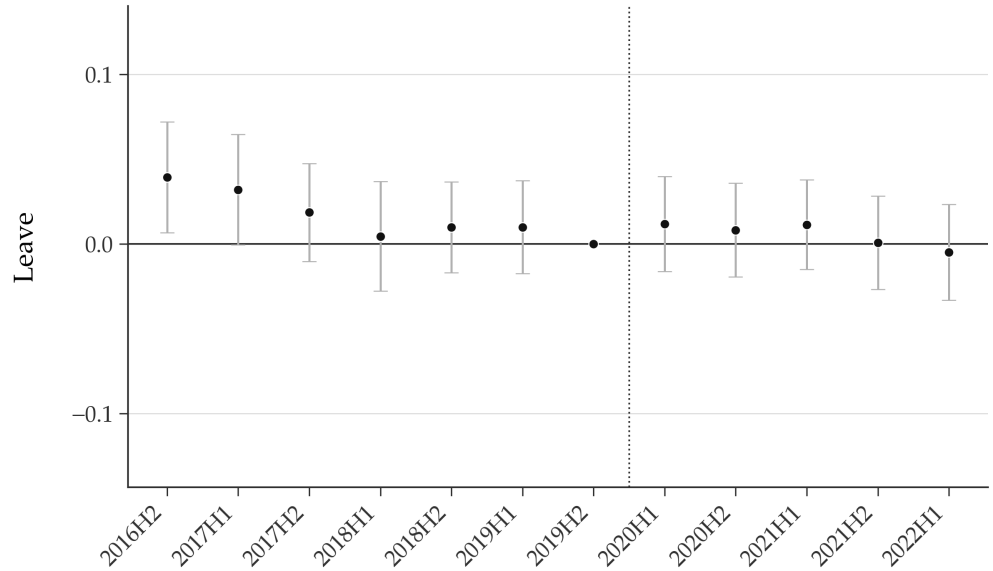
Notes: Repeats the Figure 3 specification but recodes treatment as $\mathbb{1}(\text{Remote}_j)$, equal to one for Scoop-classified fully flexible firms (Remote score = 1) in 2019. The plotted coefficients are $\mathbb{1}(\text{Remote}_j) \times \text{Startup}_j \times \mathbb{1}\{t = \tau\}$, estimated separately on the 2017H1–2022H1 Scoop–Revelio–GitHub worker panel (Panel A, worker and firm fixed effects) and the 2016H2–2022H1 Scoop–Revelio firm panel (Panel B, firm fixed effects). Half-year effects, the 2019H2 baseline, and unit-level clustering all mirror Figure 3; shaded bands show 95% confidence intervals.

Figure A5: Event Study of Fully Remote Labor Flows

Panel A: Firm Join



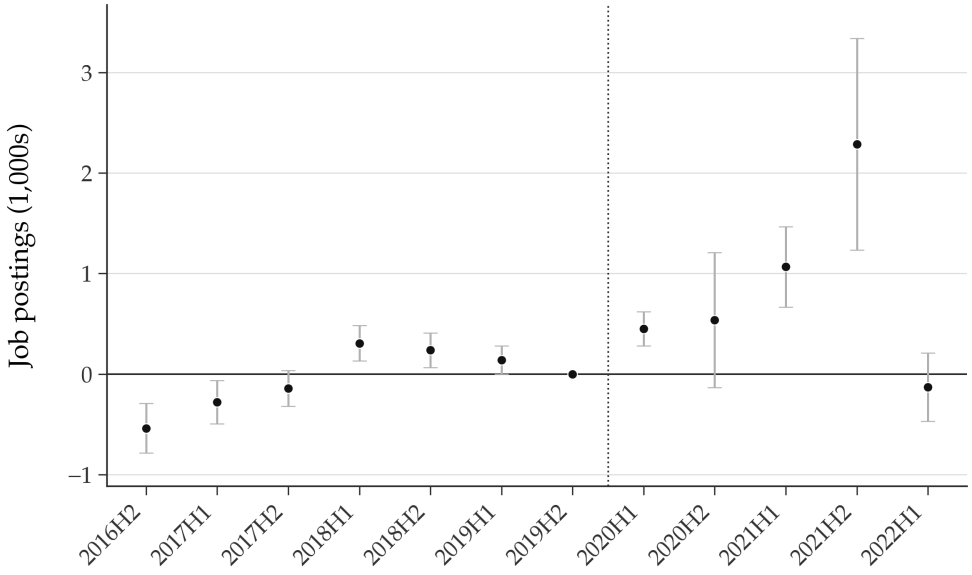
Panel B: Firm Leave



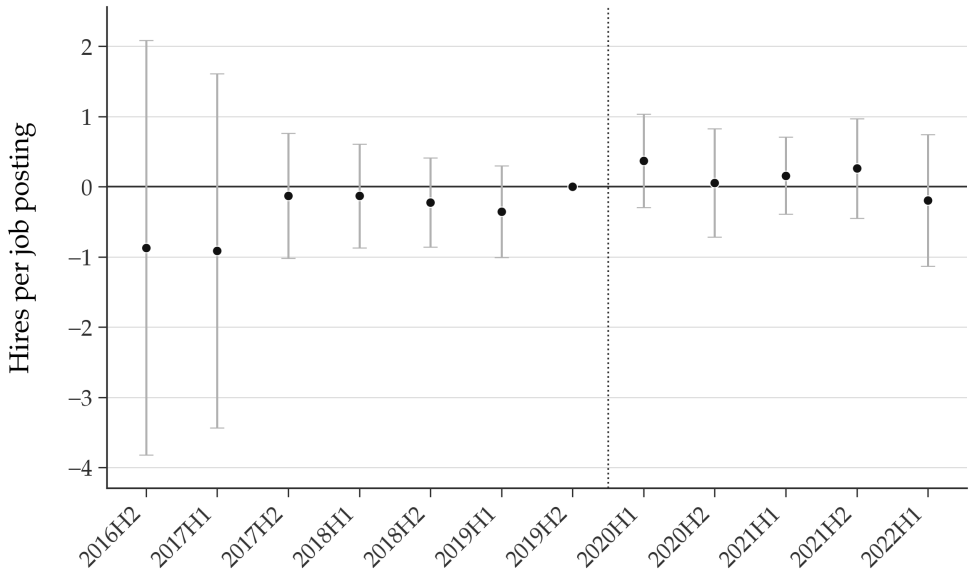
Notes: Applies the $\mathbb{1}(\text{Remote}_j)$ treatment from Figure A4 to the 2016H2–2022H1 Scoop–Revelio firm panel but swaps the outcomes for winsorized join and leave rates. Startup definitions, firm and half-year fixed effects, the 2019H2 baseline, and firm-level clustering are unchanged; the series again reports $\mathbb{1}(\text{Remote}_j) \times \text{Startup}_j \times \mathbb{1}\{t = \tau\}$ with 95% confidence intervals.

Figure A6: Event Study of Fully Remote Hiring Channels

Panel A: Job Postings

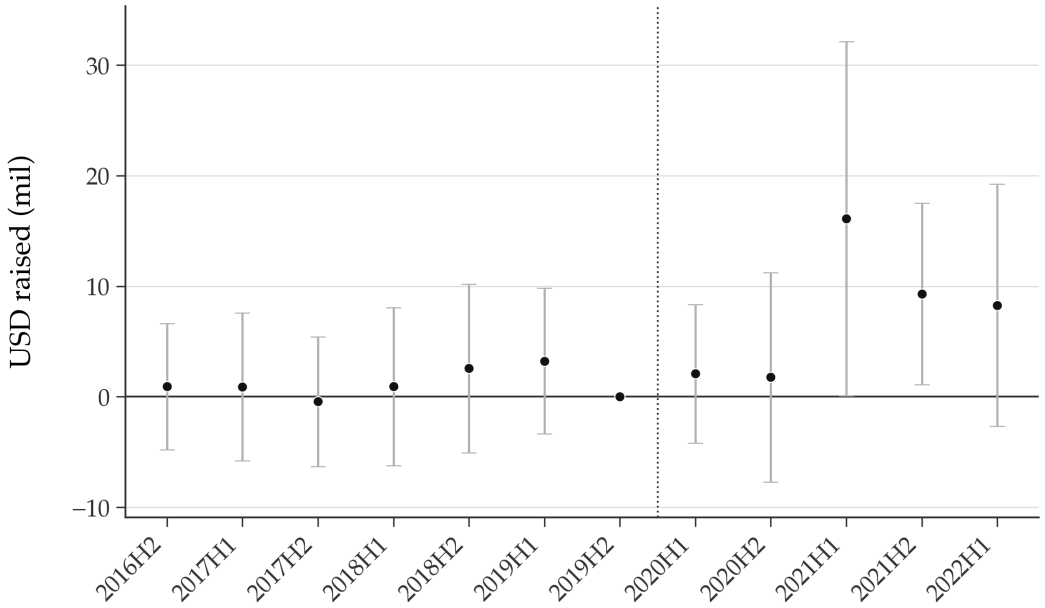


Panel B: Hires per Posting



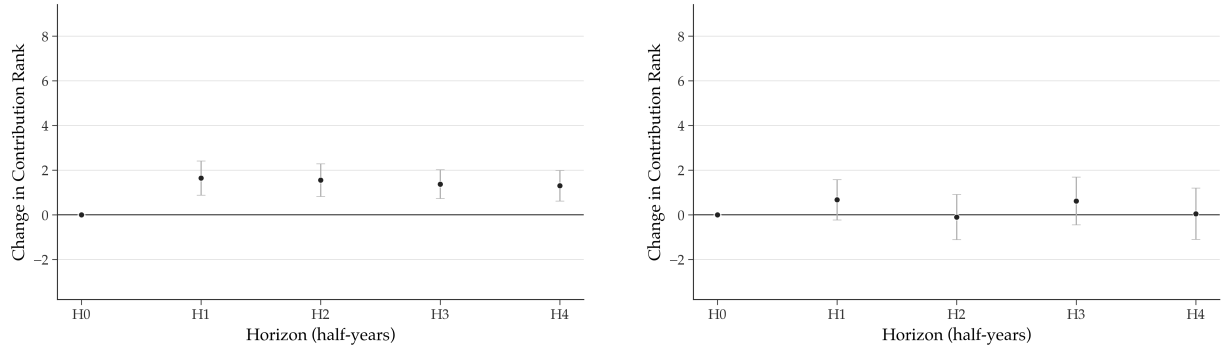
Notes: Parallels Figure A3 but uses the $\mathbb{1}(\text{Remote}_j)$ treatment from Figure A4. Outcomes remain Scoop job postings (thousands) and LinkedIn hires-per-posting ratios (winsorized at the 1st/99th percentiles) in the 2016H2–2022H1 Scoop–LinkedIn firm panel; firm and half-year fixed effects, the 2019H2 baseline, and firm clustering match the earlier figures. Lines plot the $\mathbb{1}(\text{Remote}_j) \times \text{Startup}_j \times \mathbb{1}\{t = \tau\}$ coefficients with 95% confidence intervals.

Figure A7: Event Study of Crunchbase Fundraising: USD Raised (Millions)



Notes: Uses Crunchbase fundraising outcomes, restricting to Crunchbase-matched private firms (public firms excluded). The outcome is USD raised in the half-year (millions; includes zeros). The figure plots OLS event-study coefficients from regressions that absorb firm and half-year fixed effects and allow the slope on Remote (a time-invariant firm remoteness score) to vary flexibly by half-year by interacting Remote with half-year indicators, omitting 2019H2 as the reference period. Error bars denote 95% confidence intervals with standard errors clustered by firm; the vertical dotted line marks the boundary between the omitted baseline (2019H2) and the next period (2020H1).

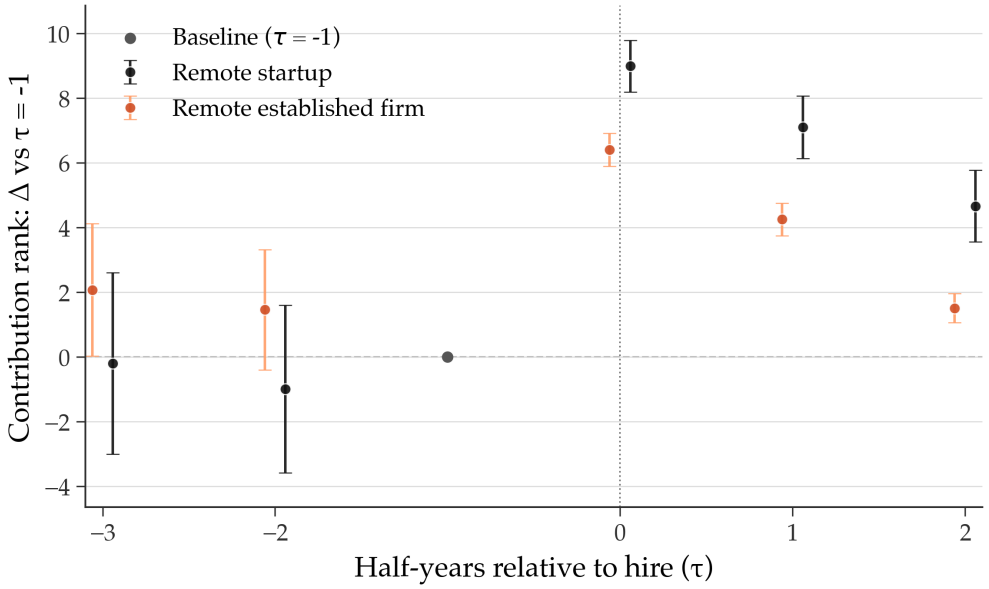
Figure A8: Hiring Shocks and Subsequent Productivity



Engineer hiring shocks | Non-Engineer hiring shocks

Notes: We start from the user–firm panel that links individual contribution ranks to the hiring mix of their employer in each half-year. For every firm, engineer and non-engineer headcount growth is constructed from the underlying LinkedIn spells, and employees are subset to fully remote firms. For each group we estimate local projections of future contribution ranks on contemporaneous role-specific hiring growth, including user×firm and half-year fixed effects and clustering by worker. Coefficients are rebased so horizon 0 equals zero; the y-axis therefore reports the change in contribution-rank percentile relative to the pre-shock period. Within each row, the left panel traces productivity responses to engineer hiring shocks and the right panel to non-engineer shocks. Dots denote point estimates, vertical lines are 95% confidence intervals, and all panels share a common scale, so differences across rows reflect the remote regime while differences across columns reflect the role of the hiring shock.

Figure A9: Event Study: Remote Hires into Startups vs. Established Firms



Notes: This figure plots event study coefficients around the hire date for remote movers, comparing destination startups to established (non-startup) firms. The sample is estimated on the 2017H1–2022H1 user-half-year panel using the percentile rank of total contributions as the outcome. Event times are $\tau \in \{-3, -2, 0, 1, 2\}$ with the baseline period prior to hire $\tau = -1$ omitted: For worker i in destination firm f and half-year t, we estimate:

$$y_{ift} = \sum_{\tau \in T} \beta_\tau \mathbf{1}\{\tau_{it} = \tau\} + \sum_{\tau \in T} \delta_\tau \left[\mathbf{1}\{\tau_{it} = \tau\} \times \mathbf{1}\{\text{dest startup}_i = 1\}\right] + \alpha_i + \delta_f + \lambda_t + \varepsilon_{ift},$$

where $T = \{-3, -2, 0, 1, 2\}$ and τ_{it} is event time relative to the hire date; α_i are worker fixed effects, δ_f destination-firm fixed effects, and λ_t half-year fixed effects. Standard errors are clustered by worker. Group-specific effects plotted are β_τ for established firms and $\beta_\tau + \delta_\tau$ for startups, each shown relative to $\tau = -1$ with small horizontal offsets and group-colored 95% confidence intervals.

Table A1: First-Stage Diagnostics

	Remote × 𝟙(Post)		Remote × 𝟙(Post) × Startup	
Panel A: Individual-level				
Teleworkable × 𝟙(Post)	0.23***	0.24***	-0.00*	-0.00***
	(0.01)	(0.02)	(0.00)	(0.00)
Teleworkable × 𝟙(Post) × Startup	0.16***	0.16***	0.39***	0.40***
	(0.02)	(0.03)	(0.02)	(0.02)
𝟙(Post) × Startup	0.09***	0.09***	0.60***	0.59***
	(0.02)	(0.02)	(0.01)	(0.01)
Fixed Effects				
Time	✓	✓	✓	✓
Firm	✓		✓	
Individual	✓		✓	
Firm × Individual		✓		✓
Partial F	325.46	295.11	186.55	175.46
KP rk Wald F	140.60	123.43	140.60	123.43
N	229,862	224,708	229,862	224,708
Panel B: Firm-level				
Teleworkable × 𝟙(Post)	0.83***	–	-0.00	–
	(0.03)		(0.00)	
Teleworkable × 𝟙(Post) × Startup	-0.41***	–	0.41***	–
	(0.08)		(0.07)	
𝟙(Post) × Startup	0.46***	–	0.58***	–
	(0.06)		(0.05)	
Fixed Effects				
Time	✓		✓	
Firm	✓		✓	
Partial F	437.86	–	16.54	–
KP rk Wald F	16.53	–	16.53	–
N	41,980	–	41,980	–

Notes: Panel A lists the worker-level first stages corresponding to Tables 2 and 3, estimated on the Scoop–Revelio–GitHub panel. The endogenous variables are Remote × 𝟙(Post) and its startup interaction; excluded instruments are the teleworkability interactions, and we report results under separate firm and individual fixed effects as well as under firm×individual match effects. Panel B reports the analogous first stages for the firm-level scaling regressions that feed Table 5, estimated on the Scoop–Revelio panel with firm and half-year fixed effects. Partial *F* and Kleibergen–Paap Wald *F* statistics evaluate instrument relevance, and the reported sample sizes match those used in the second-stage tables.

Table A2: Remote Work and Restricted Contributions

	Contribution Rank			Total
	(1)	(2)	(3)	(4)
Panel A: OLS				
Remote × 𝟙(Post)	-0.51	-1.44***	-1.48***	-17.33***
	(0.46)	(0.50)	(0.52)	(3.91)
Remote × 𝟙(Post) × Startup	–	6.37***	7.31***	55.41***
		(1.24)	(1.27)	(12.74)
Pre-Covid Mean	48.48	48.48	48.48	138.15
N	229,862	229,862	224,708	224,708
Panel B: IV				
Remote × 𝟙(Post)	-3.36	-6.60	-6.75	-105.59***
	(2.91)	(4.08)	(4.19)	(28.11)
Remote × 𝟙(Post) × Startup	–	9.07*	10.52*	108.20**
		(5.43)	(5.42)	(48.92)
KP rk Wald F	543.26	140.60	123.43	123.43
N	229,862	229,862	224,708	224,708
Fixed Effects				
Half-year	✓	✓	✓	✓
Firm	✓	✓		
Individual	✓	✓		
Firm × Individual			✓	✓

Notes: Re-estimates the worker specification on the same 2017H1–2022H1 Scoop–Revelio–GitHub panel but with the dependent variable equal to percentile ranks of restricted (private) GitHub contributions. Panels A and B follow the Table 3 OLS/IV convention. Fixed effects (firm/individual or firm×individual, depending on the column), half-year dummies, and worker-clustered standard errors follow Tables 2–3.

Table A3: Remote Work and Individual Productivity – Stayers Only

	Contribution Rank		Total
	(1)	(2)	(3)
Panel A: OLS			
Remote $\times$ $\mathbb{1}$(Post)	-0.32	-1.22**	-19.67***
	(0.46)	(0.50)	(4.71)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	–	6.27***	59.56***
		(1.28)	(14.35)
Pre-Covid Mean	52.74	52.74	210.76
N	172,056	172,056	172,056
Panel B: IV			
Remote $\times$ $\mathbb{1}$(Post)	-4.92*	-9.87**	-130.65***
	(2.88)	(4.05)	(34.25)
Remote $\times$ $\mathbb{1}$(Post) $\times$ Startup	–	13.52**	140.75**
		(5.44)	(55.67)
KP rk Wald F	479.37	121.28	121.28
N	172,056	172,056	172,056
Fixed Effects			
Time	✓	✓	✓
Firm	✓	✓	✓
Individual	✓	✓	✓

Notes: Boundary-stayer sample only: workers whose last pre-2020 employer equals their first post-2020 employer, keeping only the spell that spans the boundary. Columns (1)–(2) use contribution-rank outcomes; column (3) uses winsorized contribution levels. Column (1) omits the startup interaction; columns (2)–(3) include it. All specifications absorb firm and worker fixed effects plus half-year fixed effects; standard errors are clustered by worker. Instruments mirror Table 3, using teleworkability interactions for Remote $\times$ $\mathbb{1}$(Post) (and its startup interaction where present).

Table A4: Robustness to Industry and State Shocks

	Contribution Rank		
	(1)	(2)	(3)
Panel A: OLS			
Remote × 𝟙(Post)	-1.16**	-0.92*	-0.81
	(0.51)	(0.52)	(0.53)
Remote × 𝟙(Post) × Startup	6.55***	5.93***	6.20***
	(1.33)	(1.32)	(1.37)
Pre-Covid Mean	50.08	50.17	50.08
N	222,714	224,679	222,685
Panel B: IV			
Remote × 𝟙(Post)	-5.58	-7.87**	-5.60
	(5.53)	(3.67)	(5.76)
Remote × 𝟙(Post) × Startup	10.34**	11.76**	12.52**
	(5.16)	(5.92)	(5.84)
Pre-Covid Mean	50.08	50.17	50.08
KP rk Wald F	90.72	155.25	89.62
N	222,714	224,679	222,685
Fixed Effects			
Firm × Individual	✓	✓	✓
Industry × Half-Year	✓		✓
HQ State × Half-Year		✓	✓

Notes: Re-estimates the worker-level match fixed-effect specification from Column (3) of Table 3 while absorbing time-varying industry and/or headquarters-state shocks. Column (1) adds industry×half-year fixed effects (industry data taken from Scoop), column (2) adds HQ state×half-year fixed effects (HQ state defined at the firm level), and column (3) includes both. Panels A and B follow the OLS/IV setup described in Table 3, so the remote interactions are instrumented with teleworkability×𝟙(Post) (and ×Startup) terms computed from the firm's pre-2020 occupational mix. The post×startup main effect is included and standard errors are clustered by worker.

Table A5: Crunchbase Fundraising Outcomes: Robustness to Industry and HQ-State Shocks (USD Raised)

	USD raised (mil)			
	(1)	(2)	(3)	(4)
Sample				Age < 20
Panel A: OLS				
Remote $\times \mathbb{1}(\text{Post})$	6.11 (4.71)	4.15 (3.16)	5.03 (4.59)	-3.48 (7.21)
Pre-Covid Mean	4.06	4.04	4.06	5.60
N	26,199	26,379	26,139	15,068
Panel B: IV				
Remote $\times \mathbb{1}(\text{Post})$	20.12** (8.59)	7.47 (6.13)	19.98** (9.09)	10.17 (19.94)
KP rk Wald F	116.24	548.83	103.34	81.10
N	26,199	26,379	26,139	15,068
Fixed Effects				
Firm	✓	✓	✓	✓
Time				✓
Industry $\times$ Half-Year	✓		✓	
HQ State $\times$ Half-Year		✓	✓	

Notes: Uses fundraising outcomes from Crunchbase, restricting to Crunchbase-matched private firms. Outcome is USD raised in levels in the half-year (millions; includes zeros). Columns (1)–(3) absorb firm fixed effects and replace half-year fixed effects with industry$\times$half-year shocks, HQ-state$\times$half-year shocks, or both. Column (4) returns to firm and half-year fixed effects and restricts the sample to firm-half-year observations with age < 20. Panels A and B report OLS and IV estimates of Remote $\times \mathbb{1}(\text{Post})$ with firm-clustered standard errors; the IV instruments Remote $\times \mathbb{1}(\text{Post})$ with Teleworkable $\times \mathbb{1}(\text{Post})$. This table uses the single-term specification and does not include startup-interaction terms.

B Appendix: GitHub Data Validation

GitHub is an online platform that supports version control and collaborative software development. It organizes code into repositories, enabling distributed teams to contribute while maintaining a complete history of changes. Each modification generates a time-stamped snapshot and associated metadata, including the author, content, and timing of the change. Proposed updates must be reviewed and approved by repository maintainers before integration, ensuring version integrity and controlled collaboration.

GitHub's detailed contribution records offer a systematic measure of software developer output. The high frequency and granularity of contributions, combined with GitHub's extensive coverage, make it a useful proxy for the productivity of highly skilled professionals.

This section examines the relationship between GitHub activity and both individual- and firm-level productivity outcomes, and whether these cross-sectional relationships differ between the pre-period (2017–2019) and the post-period (2020–2022). The analysis serves two purposes. First, it assesses whether GitHub activity correlates with independent measures of productivity outside the platform. Second, it evaluates whether the strength of this relationship changes over time, which would inform whether GitHub contributions remain a stable proxy for productivity.

We begin by analyzing individual-level correlations. The sample consists of GitHub–LinkedIn matched employees in the United States who joined GitHub and made at least one contribution prior to May 2016. We relate employees' GitHub activity to two independent proxies for individual productivity: career promotions, measured as within-firm promotions, and publications, measured using output recorded on Google Scholar, DevTo, and ORCiD over the 2017–2022 period. Row 1 of Columns 1 and 2 of Table B1 shows that employees with higher GitHub activity rank higher in both career progression and knowledge production.

We then examine whether these relationships change in the post-period (2020–2022). Row 2 of Table B1 reports the interaction between GitHub contributions and the post-

period indicator. The interaction term is statistically insignificant and smaller in magnitude than the main effect for promotions, statistically significant but economically small for publications, and both statistically insignificant and economically negligible for the firm-level outcomes. Overall, these results suggest little evidence of a meaningful change in the relationship between GitHub activity and productivity across periods, with only modest strengthening for publications.

In the second step, we test if this relationship changes in our post period (2020 to 2022), and if we can continue to understand GitHub contributions as a predictor of individual and firm productivity. Row 2 of Table B1 shows the interaction term. We find that the the interaction term between GitHub contributions is statistically insignificant and half the size of the main coefficient for promotions, and statistically significant but a tenth of the size of the main coefficient for publications. The term is both statistically insignificant and a magnitude smaller than the main coefficient for both firm level outcomes. Hence overall, there seems to be no statistically significant increase in the importance of GitHub contributions across both period, except for in publications where contributions become a stronger predictor in the post period, albeit economically small.

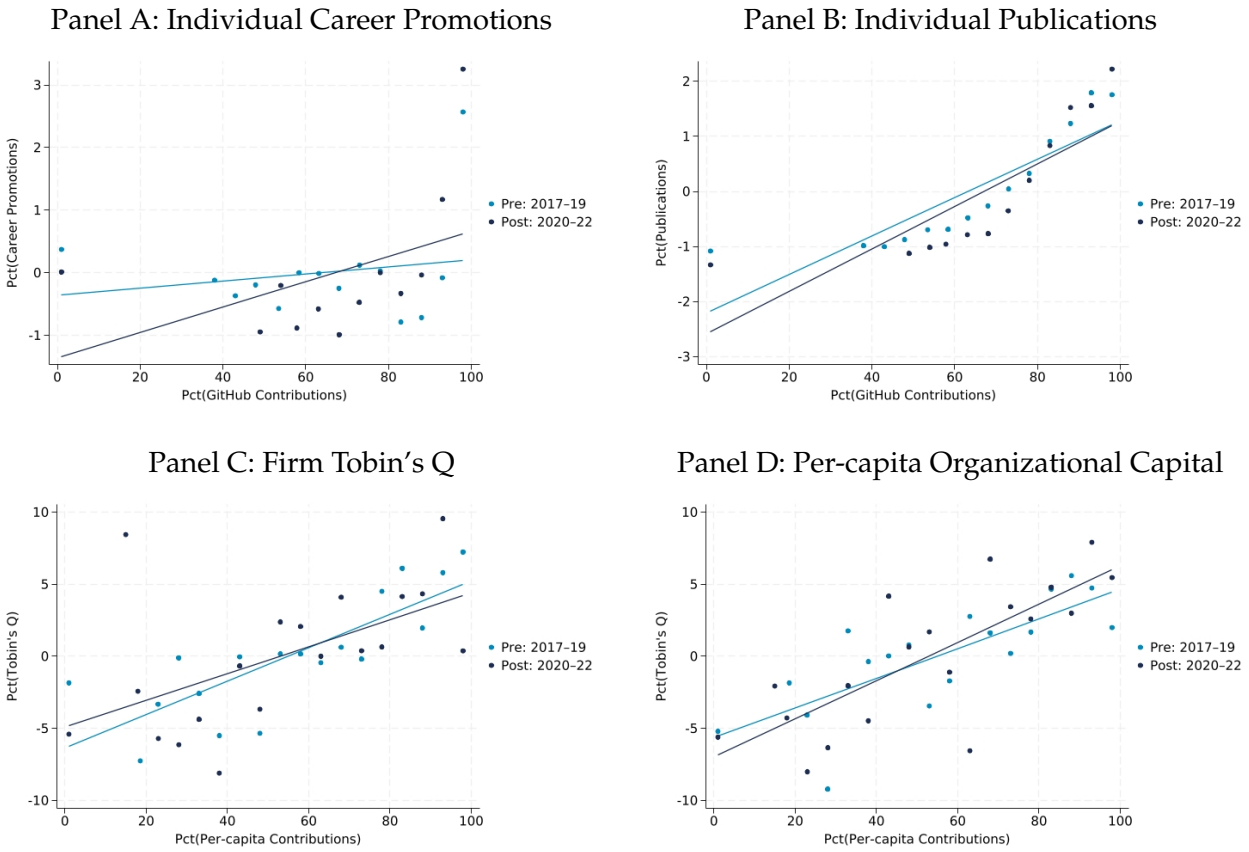
Figure B1 provides complementary visual evidence. To isolate within-period cross-sectional relationships, outcomes in each panel are demeaned separately in the pre- and post-periods prior to plotting binned averages. Across all four outcomes, the relationship with GitHub activity is positive and statistically significant. However, consistent with the regression results, there is little visible change in these relationships between the pre- and post-periods.

Table B1: GitHub Activity and Individual- and Firm-Level Productivity Outcomes

	(1)	(2)	(3)	(4)
Outcome:	*Individual-level*		*Firm-level*	
	Career Promotions	**Publications**	**Tobin's Q**	**Per-capita Org Capital**
Pct(GitHub Contributions)	0.0061***	0.0201***	0.1036***	0.0967***
	(0.0020)	(0.0007)	(0.0238)	(0.0258)
Pct(GitHub Contributions) × 1(Post)	0.0031	0.0024***	0.0189	0.033
	(0.0026)	(0.0008)	(0.0250)	(0.0269)
Cohort × Role × Sal Bucket × Sen Bucket × Period F.E.	Yes	Yes	No	No
Firm F.E.	Yes	Yes	No	No
Period F.E.	No	No	Yes	Yes
Observations	563,784	563,784	3,803	3,234
#Unique Employees/ Firms	309,853	309,853	2,139	1,814
R-Squared	0.228	0.188	0.011	0.013
Y-Mean	17.928	2.273	50.476	50.461
Magnitude - Main (%)	0.03	0.89	0.21	0.21
Magnitude - Main + Inter (%)	0.05	0.99	0.24	0.27

Notes: This table presents the cross-sectional relation between GitHub activity and individual- and firm-level productivity outcomes. In Columns 1 and 2, the unit of observation is an employee-period. The sample includes all GitHub-LinkedIn matched employees in the US who joined GitHub and made at least one GitHub contribution prior to May 2016. In Columns 3 and 4, the unit of observation is a firm-period, where firm-level measures are constructed from the corresponding sample of matched employees. In both cases, the data consist of two periods: 2017–2019, the pre-period, and 2020–2022, the post-period. The independent variable is GitHub Contributions and its interaction with the post-period indicator. In the individual-level specifications, GitHub Contributions denote the employee's percentile rank in terms of their total GitHub contributions during the period. In the firm-level specifications, GitHub Contributions denote the firm's percentile rank in terms of per-capita GitHub contributions, defined as the total GitHub contributions made by the firm's employees during the period divided by the average number of matched employees at the firm over that period. In Columns 1 and 2, the outcomes are the employee's percentile rank in Career Promotions and Publications, respectively, where Career Promotions are measured as the total number of within-firm promotions received during the period and Publications are measured as the total number of publications on Google Scholar, DevTo, and ORCiD during the period. In Columns 3 and 4, the outcomes are the firm's percentile rank in Tobin's Q and Per-capita Organizational Capital, respectively, where Tobin's Q is the firm's average annual Tobin's Q over the period and Per-capita Organizational Capital is the firm's average annual organizational capital, based on Eisfeldt and Papanikolaou (2013), divided by the number of employees reported in Compustat. Columns 1 and 2 include cohort × role × salary bucket × seniority bucket × period fixed effects as well as firm fixed effects. Columns 3 and 4 include period fixed effects. Standard errors are clustered at the employee level in Columns 1 and 2 and at the firm level in Columns 3 and 4, and are reported in parentheses. *** denotes significance at the 1% levels.

Figure B1: GitHub Activity and Individual- and Firm-Level Productivity Outcomes



Notes: This figure presents the cross-sectional relation between GitHub activity and individual- and firm-level productivity outcomes. Panels (a) and (b) use employee-period data and plot the relation between an employee's percentile rank in GitHub contributions and in Career Promotions and Publications, respectively. Career Promotions are measured as the total number of within-firm promotions received during the period and Publications are measured as the total number of publications on Google Scholar, DevTo, and ORCiD during the period. Panels (c) and (d) use firm-period data and plot the relation between a firm's percentile rank in per-capita GitHub contributions and in Tobin's Q and Per-capita Organizational Capital, respectively. Tobin's Q is the firm's average annual Tobin's Q over the period and Per-capita Organizational Capital is the firm's average annual organizational capital (based on Eisfeldt and Papanikolaou (2013)) divided by the number of employees reported in Compustat. The employee sample includes all GitHub-LinkedIn matched employees in the US who joined GitHub and made at least one GitHub contribution prior to May 2016. The firm-level measures are constructed from the corresponding sample of matched employees. In all panels, the data consist of two periods: 2017–2019, the pre-period, and 2020–2022, the post-period. To focus on the cross-sectional relationship within each period rather than differences in average outcome levels across periods, the outcome variable in each panel is demeaned separately in the pre-period and post-period. Each panel collapses the data into equal-sized bins of the x-axis variable and plots the mean y-value within each bin separately for the two periods, along with the corresponding fitted lines.