STAFF REPORTS

NO. 1061
MAY 2023

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Natalia Emanuel | Emma Harrington

FEDERAL RESERVE BANK of NEW YORK

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Federal Reserve Bank of New York Staff Reports, no. 1061
May 2023
JEL classification: J24, L23, L84, M54

Abstract

How does remote work affect productivity and how productive are workers who choose remote jobs? We estimate both effects in a U.S. Fortune 500 firm's call centers that employed both remote and on-site workers in the same jobs. Prior to COVID-19, remote workers answered 12 percent fewer calls per hour than on-site workers. When the call centers closed due to COVID-19, the productivity of formerly on-site workers declined by 4 percent relative to already-remote workers, indicating that a third of the initial gap was due to a negative treatment effect of remote work. Yet an 8 percent productivity gap persisted, indicating that the majority of the productivity gap was due to negative worker selection into remote work. Difference-in-differences designs also indicate that remote work degraded call quality—particularly for inexperienced workers—and reduced workers' promotion rates. In a model of the market provision of remote work, we find that firms were in a prisoner's dilemma: all firms would have gained from offering comparable remote and on-site jobs, but any individual firm was loathe to attract less productive workers.

Key words: remote work, work-from-home, worker productivity, selection

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Emanuel: Federal Reserve Bank of New York (email: natalia@nataliaemanuel.com). Harrington: Tippie College of Business, University of Iowa (email: emma.k.harrington4@gmail.com). The authors thank Lisa Abraham, Alex Albright, Jenna Anders, Alexander Bartik, Nick Bloom, Andrew Garin, Ruobing Han, Nathan Hendren, Claudia Goldin, Lawrence Katz, Edward Glaeser, Louis Kaplow, Jeff Liebman, Amanda Pallais, Dev Patel, Ashesh Rambachan, Ljubica Ristovska, Hannah Shaffer, Jan Sauermann, Elie Tamer, and participants at Stanford's Remote Work Conference, Princeton's JIM Seminar, Harvard's Public Finance and Labor Economics Workshop, ifo's Digital Transformation and Innovation Seminar, the UEA conference, the ASSA conference, and the AREUEA conference for helpful comments. They are also grateful to Lauren and Trevor, their colleagues at the firms who shared data, for their curiosity and commitment to research.

To view the authors' disclosure statements, visit
https://www.newyorkfed.org/research/staff_reports/sr1061.html.

Before the Covid-19 pandemic, less than a fifth of Americans worked remotely.[1] Even in seemingly remotable tasks like call-center work, remote work was uncommon.[2] This rarity was surprising since most workers were willing to take pay cuts to work at home (Mas and Pallais, 2017), and working remotely seemed to boost productivity in call-centers (Bloom et al., 2015).[3] It would seem that call-center firms could pay remote workers less to do more. So, were call-center firms making mistakes that the pandemic could correct? Or were there other pieces to the puzzle of remote work's rarity in remotable jobs?

We analyze remote work's impacts in the American call-centers of a Fortune 500 firm, which hired both remote workers (N=344) and on-site workers (N=1,592) before Covid-19. Pre-pandemic, managers expressed reservations about remote workers' productivity. This intuition was borne out in the data: remote workers answered 12 percent fewer calls per hour than on-site workers, despite handling calls randomly routed from the same queue.[4]

The source of the lower productivity, however, remained unclear. It's possible that in our setting remote work reduces productivity, and any worker would be less productive at home. Workers may struggle with low motivation and self-control problems out of the office, particularly under relatively modest incentive pay.[5]

Yet it's also possible that less productive workers choose remote jobs. Indeed,

[1] In the 2019 American Community Survey (ACS), 5.6% of workers reported working from home (U.S. Census Bureau, 2022). In the American Time-Use Survey between 2013 and 2017, 11.4% reported spending the entire day of the survey working at home (Bureau of Labor Statistics, 2022).

[2] In the 2019 ACS, 6.8 percent of phone workers worked at home, using Mas and Pallais (2017)'s occupational definition, and 12.4 percent of computer programmers did so.

[3] In real-stakes choices, Mas and Pallais (2017) find that American call-center workers were willing to take an 8 percent wage cut to work at home. In an experiment in a Chinese call-center, Bloom et al. (2015) find that remote work increased productivity by 13 percent.

[4] This gap appears immediately after workers were hired, suggesting that it did not purely reflect differences in learning on-site versus remote.

[5] In our setting, an average of 3 percent of annual compensation is in performance pay compared to over half of compensation in Bloom et al. (2015)'s setting.

even in the Chinese call-center where remote work boosted productivity, remote work unraveled (Bloom et al., 2015). Working at home halved workers' promotion chances so came to be seen as something only unproductive workers would choose. The firm subsequently discontinued remote work. Concerns about remote work's promotion consequences are widespread (Barrero et al., 2022) and may influence who chooses remote jobs. Thus, adverse selection could trap firms in a prisoner's dilemma: all firms might be better off offering remote work, but any individual firm might not do so out of fear of attracting less productive workers.

We use the office closures brought on by Covid-19 to help differentiate between remote work's impacts on worker productivity and worker selection in our American call-center context. If remote work reduces productivity, then transitioning to remote work will cause formerly on-site workers to be less productive, thereby narrowing the initial gap in productivity. If, however, less productive workers choose remote jobs, then the gap in productivity will persist (or potentially grow) once everyone is remote.[6]

Empirically, we find that the productivity gap narrowed but did not disappear in the months following the office closures. When the offices closed, the hourly calls of formerly on-site workers fell by 4 percent relative to that of already remote workers (p-value = 0.017) off of a base of 3.8 calls per hour.[7] Yet even when everyone was remote, workers who had originally chosen to be remote continued to be 8 percent less productive than those who had originally chosen to be on-site (p-value = 0.0002). Together, these results indicate a third of the initial productivity gap was due to the negative treatment effect of remote work, with the remaining two thirds due to the negative selection into remote work.

[6]A persistent gap could be due to other persistent factors, like accumulated skills. Yet pre-pandemic, remote and on-site workers had similar upward trajectories in productivity as they gained experience, suggesting that skill accumulation is not the main driver in our context.

[7]This change was due to both formerly on-site workers spending less of their time on the phone once they were remote and taking longer to answer each call.

We probe our parallel-trends assumption that remote and on-site hires were similarly affected by the shocks of the pandemic. Our results are robust to allowing for differential effects of the pandemic based on workers' demographics, parental responsibilities, and local geographic characteristics. In a placebo check, we find no similar differential changes in productivity around placebo periods, including the previous holiday rush, which saw similar fluctuations in consumer demand as those during the onset of the pandemic. In a complementary design, we find similar productivity declines around voluntary transitions from on-site to remote work before the pandemic.

Remote work not only reduces the quantity but also the quality of calls. In surveys we conducted, workers mentioned that working remotely made it harder to quickly consult with coworkers. This difficulty was reflected in an 11 percent increase in customer hold-times for workers who transitioned from on-site to remote work during the office closures, compared to those who were already remote (p-value = 0.028). Remote work also increased customer call-back rates by 3 percent, suggesting that workers were less likely to fully answer customers' initial questions when remote (p-value = 0.045). The negative effects are driven by less experienced workers, who might either wait longer for advice from more experienced colleagues when remote or forgo this advice and answer queries less completely. However, we do not find effects of remote work on customer satisfaction scores, suggesting that the degradation in call quality is meaningful but limited.

We find that remote work negatively impacts workers' career trajectories. Remote work reduces the frequency of one-on-one meetings with managers and training sessions devoted to developing workers' skills. These negative effects may have contributed to remote workers' lower promotion rates pre-pandemic. Before the offices closed, remote workers were promoted at less than half the rate of their on-site peers; once the offices closed, this difference in promotion rates disappeared.

Our estimate of remote work's effect on promotions is similar to that in Bloom et al. (2015)'s randomized control trial, where remote work halved workers' promotion chances despite improving productivity. Remote work's promotion penalty may contribute to negative selection if productive workers who anticipate on-site promotions shy away from remote jobs.

Our model suggests that call-center firms were trapped in a prisoner's dilemma with a low provision of remote work before the pandemic.[8] All call-center firms would have been better off offering remote work jobs at similar wages as on-site ones — since the costs of remote work's negative treatment effect would be offset by savings in office real-estate costs. Yet an individual firm hesitates to offer remote and on-site jobs at similar wages, due to concerns about attracting less productive workers into remote jobs.[9] Using Mas and Pallais (2017)'s estimates of workers' demand for remote work, we find firms employed 22 percent fewer remote workers due to concerns over negative selection.

The pandemic may have released firms from the initial prisoner's dilemma, by changing which workers choose remote jobs. If remote work carries less stigma, and workers now have stronger preferences for remote work, then a wider range of workers may choose remote jobs. This shift can alleviate firms' concerns about negative selection. Consistent with this possibility, the firm we study permanently shifted a large share of call workers to remote work, quadrupling the share of remote work among its call-center workforce. Nationally, twice as many workers expect to work remotely post-pandemic as did pre-pandemic (Barrero et al., 2022).

Our paper makes three contributions to the literature. First, we provide new evidence on the treatment effect of remote work in the US context. We find that

[8] Our model is most similar to Einav et al. (2010) but also shares features of classical labor market models of adverse selection (Salop and Salop, 1976; Miyazaki, 1977; Weiss, 1995).

[9] We assume there is imperfect screening of new hires for entry-level roles.

remote work takes a small toll on both call quantity and quality. Our findings land between the positive effects found in Bloom et al. (2015)'s experiment in a Chinese travel agency and the large 18 percent negative effect in Atkin et al. (2022)'s field experiment in India with workers in 6-week data-entry roles.[10] Our findings are consistent with the small negative effects found in Dutcher (2012)'s lab experiment with U.S. undergraduates doing data-entry tasks. Our suggestive evidence that remote work impedes communication is consistent with Battiston et al. (2021)'s study of emergency-phone operators and Yang et al. (2022)'s study of software engineers at Microsoft.[11] Our findings that remote work reduces training and promotion rates are consistent with Emanuel et al. (2023)'s study of software engineers and Bloom et al. (2015)'s promotion effects.

Second, we provide evidence on the selection effect of remote work. Our evidence bolsters the suggestive evidence in Linos (2018)'s analysis of the roll-out of the remote-work program at the US Patent Office. Linos (2018) finds remote workers were only less productive than on-site workers if they had been hired after the introduction of the remote-work program — and thus could have chosen the jobs because of their desire to work remotely. We offer a more direct test of adverse selection using the pandemic office closures.

Our evidence on negative selection into remote work contributes to the literature documenting how selection can limit the provision of desirable amenities, including maternity leave (Tô, 2018), workers' compensation (Cabral et al., 2022), unem-

[10]Researchers have also found positive productivity effects of other facets of flexibility over where to work. In an experiment with technology workers, Bloom et al. (2022) found hybrid work reduced attrition, without significantly reducing lines of code written. Choudhury et al. (2022) also found promising impacts of hybrid work on the depth and uniqueness of email exchanges in a Bangladeshi NGO. Relatedly, in an experiment in an Italian firm, Angelici and Profeta (2023) found that giving workers locational and temporal flexibility one day per week reduced absences and improved self-perceived productivity and well-being. Choudhury et al. (2021) found that giving remote workers flexibility over where to live improved productivity at the US Patent Office.

[11]Relatedly, time-series analyses around Covid-19 show declines in productivity of software engineers (Gibbs et al., 2023) and chess-players (Künn et al., 2022).

ployment insurance (Hendren, 2017), and short hours (Landers et al., 1996; Anger, 2008). Unraveling in these markets can create a key role for government mandates (Summers, 1989; Nekoei, 2022).

Finally, our analysis helps diagnose the puzzling rarity of remote work before the pandemic in remotable tasks. While workers had a high willingness to pay for remote work (Mas and Pallais, 2017; He et al., 2021; Maestas et al., Forthcoming), firms have been loathe to offer remote work (Barrero et al., 2022; Lewandowski et al., 2022). The negative selection that our paper documents offers one explanation, and points to a different set of reasons why remote work may or may not stick in a post-pandemic world (Bartik et al., 2020; Brynjolfsson et al., 2020; Morales-Arilla and Daboín, 2021; Barrero et al., 2022).

The rest of the paper proceeds as follows. Section I describes our empirical setting. Section II details how we use the office closures due to Covid-19 to separately identify remote work's impacts on worker productivity and worker selection. Section III presents empirical findings on treatment effects, while Section IV focuses on selection effects. Section V analyzes our findings' market implications and discusses implications for the post-pandemic world. Section VI concludes.

I Data & Setting

Our data include the daily call logs and daily schedules of call-center workers at a Fortune 500 firm between January 2019 and October 2021.[12] Personnel data identifies whether workers were hired into remote or on-site jobs, their pay rates, and their job titles. We supplement these data with two surveys: the firm conducted a caregiving survey in June 2020 that we supplemented in April 2021.[13]

[12] Previous drafts also included data from 2018, but information on workers' schedules only becomes available in 2019.

[13] Together, these surveys give us caregiving information for 43 percent of workers in our sample.

Timeline of the Firm's Remote Work Policies. The firm hired both remote and on-site call-center workers prior to Covid-19 and went entirely remote during the pandemic.[14] On March 15, 2020, the firm allowed on-site hires to work from home, and on April 6, 2020, the firm closed down its on-site call-centers. On-site workers were able to take their headsets and computers home with them, so they answered the same sorts of calls with the same equipment but now at home.

At the time that the offices closed, the firm employed 1,965 call-center workers — 344 of whom were hired to work remotely and 1,592 of whom were hired to work on-site but now had to work at home. At that time, the firm also employed 229 workers who had been hired to be on-site but had received permission to go remote prior to the office closures. We use these workers in supplementary analyses that evaluate the effects of remote work on productivity.

Routing of Calls. The firm's call-center workers handle incoming calls from customers. Most calls fall into three queues that vary in their complexity. Workers on the simplest queue of calls handle questions such as "When will my couch arrive?" Workers on the most complex queue of calls handle questions such as "Only half my couch arrived — what should we do?!" Within each of these queues, calls are randomly routed to workers on the same queue at the same time, regardless of whether they are remote or on-site. We exclude workers who handle calls outside these queues for specialized products or specific customers like firms.

Workers are almost always scheduled for 8-hour shifts that are from 9am to 5pm local time: 8-hour shifts were standard for both remote and on-site workers, both before and after the pandemic (Figure A.1(a)).[15] The firm covers service hours

[14]The firm started to hire remote workers in July 2018, and so we limit our sample to workers who were recruited after July 2018.

[15]When the offices were open, on-site workers had marginally more absent time than remote workers (45 min. vs. 40 min., p-value of difference = 0.085, Figure A.1(b)). Once the offices closed, this gap became smaller and insignificant (73 min. vs. 71 min., p-value of difference = 0.69). These patterns suggest that remote work reduces absenteeism, but the effect is small and insignificant.

from 8am ET to midnight ET by having call-center workers spread across every time-zone. We account for workers' time-zones in our analyses.

Call Logs. The firm's routing system tracks the number of calls that each worker handled each day. We focus on the number of calls that the worker handled herself, excluding calls transferred to another worker.[16] The firm's software also records the amount of time that each worker spent talking to customers on the phone and the amount of time that she kept customers waiting on hold each day.

Scheduling Data. The firm tracks workers' daily schedule in fifteen-minute increments, showing the total minutes workers were scheduled to answer customers' calls each day. Our primary outcome measure is calls handled per hour that she was scheduled to be on the phone. Crucially, in the denominator, we exclude time that the worker was scheduled to answer customers' emails or chat messages, attend meetings, go to training sessions, and do other productive tasks for the firm.[17]

Call Quality Metrics. The firm tracks three proxies of call quality. The retail records how long customers' waited on hold, whether or not customers call back within two days (often indicating that the initial question went unanswered),[18] and customers' ratings of the satisfaction with their calls from one to five stars. Reassuringly, call-back rates and hold-times are predictive of customer satisfaction scores: customers are less satisfied when their questions are incompletely answered, or they must wait longer when speaking to a customer service representative.[19]

[16] We use she/her/hers pronouns since 73 percent of workers identify as female in our sample.

[17] Pre-pandemic, the schedules of remote and on-site workers were indistinguishable (Figure A.2). During the pandemic, there was an uptick in customer emails and chat messages, and workers who were initially remote were slightly more likely to be rescheduled to answer these messages instead of answering calls. The scheduling data is consequently key for analyzing calls per hour. We show robustness to controlling for hours spent on calls to account for fatigue.

[18] Tables 3 and 6 consider calls defined as calls that do not lead to a callback within two days.

[19] On average, a standard deviation increase in call-back rates (of 11 percentage points) is associated with a 0.013 standard-deviation reduction in satisfaction scores (p-value < 0.0001). A standard deviation increase in hold time (of 1.8 minutes) is associated with a 0.024 standard-deviation reduction in satisfaction scores (p-value < 0.0001). Call-back rates and hold times are not significantly

These quality metrics are imperfect. Customers rarely review calls (the participation rate is 11 percent) and, when they do, they tend to be polite (the mean review is 4.8 out of 5).[20] The challenges of monitoring quality have two implications. First, the firm does not pay piece-rates and instead primarily bases annual compensation on hourly wages ($\geq$83% of annual compensation). As a result, workers have limited incentive to trade quality for quantity, suggesting call quantity may be a useful barometer of productivity.[21] Second, being on-site can impact managers' information about workers and the likelihood of promotion to higher-stakes' roles.

Promotions. When workers are promoted to handling more complex or specialized calls, their pay increases by $2 per hour or 13 percent. Remote and on-site workers are on different teams with different managers so do not directly compete for promotions. Nonetheless, remote workers had half the promotion rates as on-site workers prior to the pandemic, as investigated in Section III.A.

The Sample. Table 1 provides summary statistics on our primary sample.[22] The first column describes our full sample. The subsequent columns split workers based on whether they chose remote or on-site jobs and whether we observe them before or after the pandemic closure of the on-site locations in April 2020.

Productivity Differences. Before the pandemic, the firm's remote workers answered fewer calls than the firm's on-site workers in each hour that they were scheduled to

correlated with one another so are independently predictive of satisfaction scores.

[20]The audio of each call is recorded for quality-assurance checks. However, managers have limited time to review calls and, thus, may fail to catch calls that go awry.

[21]As Goodhart's Law warns, a useful number can cease to be useful once it is a measure of success: thus, call quantity can be a useful measure of productivity that is nonetheless problematic to use as the basis of pay.

[22]Our primary sample limits to workers hired between July 1, 2018 — when the firm starting hiring remote workers directly – and March 15, 2020 — when on-site workers were allowed to work at home. We further exclude workers who were hired to be on-site and then were permitted to transition to remote work before the pandemic. We separately consider these workers in supplementary analyses. Throughout, we exclude workers who handle calls for specialized products or specific customers because these calls are not randomly assigned.

answer calls (row 1 in columns 2−4 of Table 1). The gap in calls per hour increases to 12 percent when controlling for the queue of calls and worker demographics (Table B.1). The productivity differences between remote and on-site workers are present when workers first start at the firm, suggesting that these gaps are not due to differential learning (Figure A.3).

Remote workers answered fewer calls because they spent less of their time on the phone (row 2) and answered each call more slowly (row 3). The differences in call quantity were not offset by differences in call quality, which were similar for remote and on-site workers before the pandemic (rows 4−6).

Once everyone worked remotely due to the Covid-19 office closures, the gap in calls per hour narrowed but much of the gap persisted (row 1, columns 5−7). Sections II-IV make sense of these patterns and probe their robustness.

Pay & Outside Options. On average, remote workers were paid one dollar less than on-site workers at the firm (row 7): all the firm's remote workers had entry pay of $14 per hour, while some on-site locations had entry pay of $16 per hour.

Remote workers also had marginally better outside options. We use data on each worker's home address to characterize each worker's local labor market. Remote workers tend to live in metropolitan statistical areas (MSA) where the average customer-service worker earns about thirty cents more per hour (row 8).[23]

While remote workers at the firm are paid less than on-site workers both in abso-

[23] This gap in workers' alternatives is similar for adjacent occupations to customer-service — such as bookkeeping and clerical tasks (see Table B.3 for common occupational transitions). We characterize adjacent occupations using data on past occupations in the Current Population Survey (U.S. Census Bureau, 2021), as in Schubert et al. (2021)'s methodology. We then construct a more general measure of workers' outside options that weights each occupation by the likelihood of a transition between that occupation and customer service. We find a similar gap in outside options in this broader measure ($17.29 per hour for remote workers vs. $16.93 per hour for on-site workers pre-pandemic). Given the similarity of these measures, we focus on the customer-service wage, but results are similar when we control for the broader measure of outside options.

lute and relative terms, these differences are comparable to the value that workers place on working from home (Mas and Pallais, 2017). Thus, after adjusting for amenities, the remote and on-site jobs offered by the firm are similarly attractive. Further, our results are similar when limiting the sample to workers with the same wages and when controlling for geographic differences in where remote and on-site workers are drawn (Tables 5, B.1, B.2, and B.17).

Worker Traits. Before the Covid-19 office closures, workers had been at the firm about 8 months (row 10). The majority of the firm's call-center workers identify as female (row 11). The average age of workers is 35 (row 12). A substantial share of workers report being parents in the caregiving surveys (row 13). Remote workers tend to be a few years older and are more likely to report being female and parents.

Attrition. Call-center jobs feature high churn both at this firm and nationally. In the six months before the offices closed, fully 20 percent of workers left the firm, which is in the typical range for the industry (Reynolds, 2015). Pre-pandemic, on-site workers were more likely to leave the firm, but this gap persisted unchanged when the offices closed (Table B.4). This differential was driven by quits and not involuntary terminations. Reassuringly, quit rates for personal reasons (including moves and family sicknesses) did not differentially change after the offices closed.

II Empirical Framework

This section uses the potential outcomes framework to illustrate how the office closures due to Covid-19 can separately identify remote work's impacts on worker productivity and worker selection.

Let $Y_{i,j}$ denote the potential outcome of worker i in job j, which can be remote ($j = r$) or on-site ($j = o$). Let R denote the set of workers who choose remote jobs and O, the set of workers who choose on-site jobs.

A worker's potential outcome might differ in a remote and on-site job, $Y_{i,r} \neq Y_{i,o}$ if, for example, a worker i is more distracted by family at home (so $Y_{i,r} < Y_{i,o}$) or coworkers in the office (so $Y_{i,r} > Y_{i,o}$). The sets of workers who choose remote and on-site jobs might also differ in their potential outcomes if, for example, more productive workers are more deterred by remote work's promotion penalties (so $\mathbb{E}[Y_{i,j} \mid R] < \mathbb{E}[Y_{i,j} \mid O]$).

The productivity difference before the offices closed is given by:

$$\mathbb{E}[Y_{i,r} \mid i \in R] - \mathbb{E}[Y_{i,o} \mid i \in O].$$

The challenge is that we observe different potential outcomes for different sets of workers.[24] Thus, the productivity difference combines differences in worker selection (R vs. O) with differences in treatment ($Y_{i,r}$ vs. $Y_{i,o}$ for each worker):

$$\begin{aligned}\mathbb{E}[Y_{i,r} \mid i \in R] - \mathbb{E}[Y_{i,o} \mid i \in O] = &\underbrace{(\mathbb{E}[Y_{i,r} \mid i \in R] - \mathbb{E}[Y_{i,r} \mid i \in O])}_{\text{Selection}} \\ &+ \underbrace{(\mathbb{E}[Y_{i,r} \mid i \in O] - \mathbb{E}[Y_{i,o} \mid i \in O])}_{\text{Treatment}}.\end{aligned}$$

Remote workers might be less productive than on-site workers because the treatment effect caused them to be less productive. If so, on-site hires would be as unproductive at home as remote hires ($\mathbb{E}[Y_{i,r} \mid i \in O] - \mathbb{E}[Y_{i,o} \mid i \in O] < 0$). Alternatively, remote work could select for less productive workers. If so, workers who chose to be remote would be less productive than workers who chose to be on-site even if all workers were working at home ($\mathbb{E}[Y_{i,r} \mid i \in R] - \mathbb{E}[Y_{i,r} \mid i \in O] < 0$).

Without a shock to work arrangements, we could not disentangle treatment from

[24]This is a canonical challenge in markets for credit (Karlan and Zinman, 2009), health insurance (Einav et al., 2010), and labor (Lazear, 2000), where contracts can have causal effects on behavior and contracts can differ in who selects into them.

selection because we would never observe the potential outcome of workers who chose to be on-site in remote jobs ($\mathbb{E}[Y_{i,r} \mid i \in O]$). The office closures of Covid-19 reveal this missing potential outcome.

II.A The Treatment Effect of Remote Work

When the offices closed due to Covid-19, on-site workers transitioned to remote work but were also impacted by the pandemic. Indexing potential outcomes by time t and letting t_0 denote the pre-pandemic period and t_{+1} denote the lockdown:

$$\mathbb{E}[Y_{i,r,t_{+1}} - Y_{i,o,t_0} \mid i \in O] = \underbrace{\mathbb{E}[Y_{i,r,t_0} - Y_{i,o,t_0} \mid i \in O]}_{\text{Treatment Effect}} + \underbrace{\mathbb{E}[Y_{i,r,t_{+1}} - Y_{i,r,t_0} \mid i \in O]}_{\text{Pandemic Effect}}.$$

In contrast, workers who were already working remotely were affected only by the pandemic, not by the office closure. We use the already-remote workers as a control group to net out the pandemic's effect in a difference-in-differences design:

$$\begin{aligned}&\mathbb{E}[Y_{i,r,t_{+1}} - Y_{i,o,t_0} \mid i \in O] - \mathbb{E}[Y_{i,r,t_{+1}} - Y_{i,r,t_0} \mid i \in R] \\ &= \underbrace{\mathbb{E}[Y_{i,r,t_0} - Y_{o,r,t_0} \mid i \in O]}_{\text{Treatment Effect}} + \left[\underbrace{\mathbb{E}[Y_{i,r,t_{+1}} - Y_{i,r,t_0} \mid i \in O]}_{\text{Pandemic Effect} \mid \text{i} \in O} - \underbrace{\mathbb{E}[Y_{i,r,t_{+1}} - Y_{i,r,t_0} \mid i \in R]}_{\text{Pandemic Effect} \mid \text{i} \in R}\right], \qquad (1)\end{aligned}$$

which nets out pandemic shocks to both workers and to consumers, who may call in at different rates (and with different courtesy) during the pandemic.

This design identifies the treatment effect of remote work under the parallel-trends assumption that workers who chose to be on-site face similar pandemic shocks as those who chose to be remote. We probe this assumption in a few ways. First, we show robustness to controls described in Section II.C. Second, in a placebo check, we do not find similar changes in the relative productivity of on-site and remote hires in other periods with similar swings in consumer demand as the pandemic. Third, we do not find any differential trends in productivity between remote and on-site hires leading up to the closures, nor any differential changes in the likelihood of departing the firm, particularly due to personal reasons like family sickness (Table B.4). Finally, we find similar results using an event study around voluntary transitions to remote work that occurred before the pandemic.[25]

II.B Selection Effect of Remote Work

During the Covid-19 office closures, all workers were remote, allowing us to observe the same potential outcome for workers, regardless of their initially chosen job. Thus, to assess the selection effect of remote work, we can simply compare the productivity of workers who originally chose remote jobs and workers who originally chose on-site jobs:

$$\mathbb{E}[Y_{i,r,t_{+1}} \mid i \in R] - \mathbb{E}[Y_{i,r,t_{+1}} \mid i \in O]. \tag{2}$$

For this comparison to isolate remote work's impact on worker selection, workers who initially chose remote and on-site jobs must face similar pandemic shocks. Further, other potential determinants of worker selection — such as the attractiveness of the posted job and the conditions in the local labor market — must be as good as constant. We probe these assumptions in two ways. First, we consider robustness to controls described in Section II.C. Second, we consider a placebo check that tests whether differences in worker selection persist among workers hired when the offices were closed due to Covid-19. During the pandemic, the firm continued to advertise on-site jobs that would require a return to in-person work once it was safe to do so. This promise lost teeth as the pandemic dragged out. Consistent with the differences in selection being due to on-site versus remote work, we find that differences in selection dissipate over the course of the pandemic (Section IV).

[25] We also find similar estimates when we use these pre-Covid switchers as an alternative control group for our difference-in-differences design.

II.C ESTIMATING EQUATIONS

Our estimating equation for remote work's treatment effect is the empirical analogue of Equation 1:

$$\frac{\text{Calls}}{\text{Hour}}_{i,t} = \beta \,\text{Initially On-Site}_i \times \text{Post}_t + \psi \,\text{Initially On-Site}_i + \rho \,\text{Post}_t + X'_{i,t}\kappa + \epsilon_{i,t}, \quad (3)$$

and our estimating equation for the selection effect of remote work is the empirical analogue of Equation 2:

$$\frac{\text{Calls}}{\text{Hour}}_{i,t} = \theta \text{Initially Remote}_i + X'_{i,t}\alpha + u_{i,t} \text{ in the closures,} \quad (4)$$

where the observation is at the worker-day level and standard errors are clustered by worker. Our primary sample limits to a six month bandwidth around the office closures, excluding the three weeks between March 15, 2020 and April 6, 2020 when on-site hires could work from home but did not yet have to do so.

The controls in $X_{i,t}$ relax the identifying assumption that remote and on-site hires faced similar pandemic shocks. Our preferred set of controls include call-queue fixed effects and demographic controls. Call-queue fixed effects control for the day of the call interacted with the worker's time-zone and call-type (routine, standard, or complex). Demographic controls allow workers of different ages and genders to face different pandemic shocks, by interacting a worker's gender and age with the post-period indicator (Post_t). When estimating the treatment effect in Equation 3, our preferred specification also includes worker fixed effects.

We consider robustness to including additional demographic and geographic controls. We control for local Covid-19 case counts, unemployment rates, and wages in other call-center jobs. We further allow for differential pandemic shocks for mothers and fathers in the subsample of workers who responded to the caregiver surveys. We test whether we arrive at similar conclusions in the subsample of workers with $14/hour entry wages. We finally consider the inclusion of fixed effects for hours scheduled for various tasks to account for fatigue.

III Results: The Treatment Effect of Remote Work

Our difference-in-differences design around the Covid-19 office closures compares the change in productivity of formerly on-site workers who went remote to the change in productivity of already-remote workers.

Once on-site hires started to work remotely due to Covid-19, their productivity declined relative to that of already-remote workers. Figure 1(a) plots the average volume of calls that workers handle each hour that they are scheduled to handle calls without controls. Initially, there is a sizable gap in productivity between remote and on-site hires, which narrows once on-site hires also work at home.

Figure 1(b) illustrates the conditional differences between on-site and remote hires, using our preferred controls (see Section II.C). Our difference-in-differences estimate indicates that working remotely decreased productivity by 0.15 calls per hour or 3.9 percent (p-value = 0.017, column 4 of Table 2). The effects of remote work are persistent: we find similar estimated impacts with a post-period of one to twelve months (Figure A.4).

The control group of already-remote workers is pivotal for making accurate inferences about remote work's causal effect. During the pandemic, many consumers switched from brick-and-mortar shopping to online retail, increasing the volume of calls to the firm's service lines. This uptick caused all workers to handle more calls per hour. Only by comparing the productivity of on-site hires to that of already-remote workers can we see the relative decline in on-site hires' productivity when they started to work at home. Other similar fluctuations in customer calls did not lead to differential changes in productivity: in a placebo check, we find no significant effect in a 2-month bandwidth for any month other than the treated ones (Figure A.5), despite similar upticks in customer call volumes during the previous holiday season.

We show robustness of these results to alternative specifications. We find consistent results when including a variety of controls (Table 2), a stability that is notable given the increase in the variation explained (R^2) from 5 to 45 percent. The results are also robust to including the omitted period around the office closures when on-site workers could choose whether to work on-site (Table B.5). We find no significant differences in pre-trends prior to the office closures: a Wald test of the joint difference of the pre-period gaps from the reference period has a p-value of 0.38 with our preferred controls. Our estimates are similar if we consider only those call-centers with entry pay of $14 per hour (Table B.6, Figure A.6). Moreover, the results remain stable and significant if we include controls for the hours scheduled for calls or other tasks (Table B.7) or additional geographic controls such as the unemployment rate (Table B.8). Finally, we consider an alternative control group composed of the workers who were permitted to go remote pre-Covid and find a similar decline in calls per hour of 3.5 to 5.7 percent (Table B.9).

On-site hires answered fewer calls after going remote both because they spent relatively less time on the phone and because they answered each call more slowly. Prior to the pandemic, on-site hires spent three-quarters of their scheduled calling time actually on the phone. Once the offices closed and on-site hires started to work remotely, they spent 2 percentage points (or 2.7 percent) less time on the phone (p-value = 0.0002, column 1 of Table 3 and Figure A.7(a)). In addition to spending less time on the phone, on-site hires took 0.37 minutes longer to answer customers' questions once they were remote, an increase of 2.8 percent relative to their pre-period mean (p-value = 0.093, column 2 of Table 3(a) and Figure

A.7(b)).[26] These effects are similar for more and less experienced workers at the firm (Table 3(b) for the decomposition and Figure A.8 for calls per hour).

Call Quality. In addition to reducing the quantity of calls, remote work reduced their quality. Once on-site hires started to work at home, they kept customers waiting on hold for longer, increasing customers' hold time by 0.12 minutes per call or 10.6 percent (p-value = 0.028, column 3 of Table 3(a) and Figure A.9(a)). The increase in hold times is driven by workers who were in their first six months at the firm when the offices closed, who increase hold times by 24.2 percent when they go remote (p-value = 0.0003, column 3 of Table 3(b) and Figure A.9(b)).[27]

When asked about remote work's impact in our survey, many workers noted challenges communicating with colleagues.[28] One respondent said her biggest challenge was "people not answering you in chat and managers not being readily available." Another said she missed "having neighbors to turn to for assistance." Our empirical results suggest that some inexperienced workers wait longer for their colleagues' digital input once they are remote and consequently keep customers on hold for longer.[29]

In addition to waiting longer for advice from their colleagues, inexperienced workers may simply forgo such advice once they are remote and consequently answer

[26]In Bloom et al. (2015)'s experiment, remote work's productivity advantages primarily came from workers spending more time on the phone although call speeds also became marginally faster.

[27]Once on-site hires were remote, they were also more likely to keep customers waiting on hold for more than two minutes. Hold-times in excess of two minutes increased by 4.28 percentage points (p-value = 0.004, Figure A.10). This increase is also driven by less experienced workers: those in their first six months when the offices closed became 9.1 percentage points more likely to keep customers on hold (p-value = 0.0000087).

[28]Specifically, we asked: "If you would like to share any challenges that you have faced when working from home during the pandemic, we would love to hear them."

[29]Experienced workers — who may give more advice than they receive — keep customers on hold for marginally less time once remote (Figure A.9(b)). These heterogeneous effects of remote work mimic the baseline differences between remote and on-site workers: when the offices were open, the least experienced remote workers kept customers on hold longer than their on-site peers, while the most experienced remote workers kept customers on hold for less time (Figure A.11).

customer calls less completely. Indeed, when on-site hires transitioned to remote work, they were 0.40 percentage points or 2.5 percent more likely to have customers call back within two days, suggesting that their initial question went unanswered (p-value = 0.045, column 4 of Table 3(a) and Figure A.12(a)). The increase in call-back rates is concentrated among less experienced workers, who see a 5.3 percent increase in call-back rates when they go remote (p-value = 0.007, column 4 of Table 3(b) and Figure A.12(b)).

We do not see significant effects on customer satisfaction scores (column 5 of Table 3(a)): while the onset of the pandemic led to poorer reviews (Figure A.13(a)), the difference-in-differences design suggests that this was due to the strains of the pandemic (on workers and customers), rather than the effects of remote work.

A composite measure of quantity and quality — the number of calls that do not result in a call back — shows a significant decrease (column 6 of Table 3(a)).

Heterogeneous Treatment Effects by Distractions at Home. Our results offer suggestive evidence that remote work's negative treatment effect is not primarily driven by workers facing more distractions while at home. We find a negative treatment effect on calls per hour for workers who do not have children — and, indeed, no significant heterogeneity in remote work's effects by parental status (Table B.12).[30] We also find that most workers have a private workspace to take calls,[31] and the negative treatment effect on calls per hour is statistically indistinguishable for

[30]Since we do not have data on all workers' parental status, and women are more likely to be primary caregivers, we also consider gender differences in our effects. We find no difference in the treatment effect of remote work on either call quantity or quality (Table B.14). This result differs from Dutcher (2012)'s finding of a particularly negative treatment effect of remote work for male undergraduates in a lab-based experiment and from Adams-Prassl (2020)'s finding that female MTurkers with children at home have more interruptions.

[31]We asked respondents, "During the past week, what room have you typically worked in?" We limited this question to non-parents to minimize the burden of the childcare-oriented survey on parents. Fully 56 percent of respondents had a home office, another 23 percent worked in a private bedroom, and 21 percent worked in a shared space (typically a living room or kitchen). We categorize those taking calls in a home office or bedroom as having a private workspace.

workers who have a private workspace like a home office and those who work in a shared space like a living room (Table B.13).

Pre-Covid Switches to Remote Work. In a complementary design, we estimate changes in workers' productivity around voluntary transitions from on-site to remote work that occurred before the pandemic.[32] Even among those who chose to go remote and were granted permission to do so by the firm, we find a negative treatment effect akin to those in our main estimation: workers answered 3.5 percent fewer calls after they went remote (Table 4; Figure A.14). The decrease in calls handled is driven by a decrease in the time spent on the phone and an increase in the duration of any given call. We find no significant changes in call quality with remote work, which is consistent with our findings that the adverse effects on call quality are driven by less experienced workers who were not allowed to transition to remote work before the pandemic.

III.A Treatment Effects on Workers' Career Trajectories

In addition to analyzing remote work's immediate effects on productivity, we can investigate its effects on workers' career trajectories.

We find that remote work reduces training time and manager one-on-one meet-

[32] We use the estimation approach proposed by Dube et al. (2023) to handle concerns over staggered difference-in-differences designs (De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). We create a stacked dataset that includes a separate dataset s for each individual who switches to remote work. Each dataset includes the individual who switches to remote work and a set of control individuals who handle calls of the same type (c), in the same time-zone (ℓ), and at the same time (t) but who stay on-site until Covid-19. We fully interact our controls with the dataset s so that we effectively estimate the effect of each switch to remote work separately and then aggregate these effects in a single estimate. We use the following specification:

$$\text{Calls/Hour}_{i,t,s} = \phi \mathbb{1}[\text{Remote}_{i,t,s}] + \mu_{i,s} + \mu_{t,\ell(i),c(i,t),s} + v_{i,t,s}. \tag{5}$$

We give a weight of 1 to observations of workers who switch to remote work and a weight of $\frac{1}{N_{t,\ell(i),c(i,t),s}}$ for control observations where $N_{t,\ell(i),c(i,t),s}$ denotes the number of control observations for each treated observation.

ings. When the offices were open, on-site hires spent more time in training sessions devoted to developing new skills and in one-on-one meetings with their managers planning their short-term path to promotion over the next 30, 60, and 90 days. Once the offices closed, both advantages disappeared. The difference-in-differences estimates indicate that remote work reduced training time by 19.1 minutes per month or 26.3 percent (p-value = 0.022, leftmost plot in Figure 2(a)) and manager one-on-one time by 10.2 minutes or 34.1 percent (middle plot).[33]

Consistent with remote work reducing workers' opportunities to pick up skills and bond with managers, we see stark differences in promotions prior to the pandemic: a year after hire, 44.0 percent of on-site hires had been promoted compared to just 20.9 percent of remote hires (Figure 2(b)).[34] The gap in monthly promotion rates disappears once the offices close (the rightmost plot in Figure 2(a)): thus, the difference-in-differences estimate indicates that remote work decreases promotion rates by 3.6 percentage points or 58.7 percent, similar to the effect in Bloom et al. (2015)'s experiment.[35] If workers anticipate this promotion penalty, more ambitious workers may gravitate away from remote jobs. The next section investigates the consequences for worker selection.

IV RESULTS: THE SELECTION EFFECT OF REMOTE WORK

During Covid-19's office closures, all workers — regardless of whether they were originally on-site or remote — worked remotely. So any remaining productivity differences reflect only differential selection into remote work. Those who had originally chosen to be remote continue to be less productive, averaging 0.30 (or

[33] The time-series patterns reveal consistent levels of training but a precipitous decline in manager one-on-one meetings for all workers once the offices close (Figure A.15).

[34] Figure 2(b) plots unconditional promotion rates. If we instead condition on persisting in the firm, the share of workers who have been promoted starts to approach one, so remote workers catch up to their on-site counterparts by about 15 months at the firm (Figure A.16).

[35] Our results are particularly striking because remote and on-site workers are on different teams so do not directly compete for promotions.

7.8 percent) fewer calls per hour with our preferred controls (p-value $= 0.00004$ in column 3 of Table 5). Indeed, the entire productivity distribution of originally remote workers is lower than that of originally on-site workers even though all workers are at home (Figure 3). These results are robust to the inclusion of our standard controls (Table 5), fixed effects for the number of hours that workers are scheduled to answer calls (Table B.18), and additional geographic controls (Table B.19), as well as to limiting to locations with entry pay of $14 per hour (Table B.17).

Originally remote workers answered fewer calls per hour primarily because they took longer to answer each call (column 2 of Table 6). They kept customers on hold for similar durations and had similar customer ratings as workers who were initially on-site (column 3 and 5 of Table 6).

Originally remote workers are more likely to forward challenging calls, transferring fully 4.0 percentage points (or 15.3 percent) more calls to other workers (p-value < 0.00001 in column 3 of Table B.23). Consistent with this, they are less likely to have customers call back to the service line (column 4 of Table 6). If we consider a composite measure of productivity — the number of calls that the worker answers each hour that do *not* yield a call-back within two days — our results continue to indicate negative selection into remote work in column 5 of Table 6.[36]

We do not find any meaningful differences in worker selection based on gender, parental status, or tenure (Table B.25). We do not see negative selection among workers who were permitted to go remote before Covid-19 (Table 4, Figure A.14), consistent with the firm selectively granting approval to only some workers to go remote.

Location Expectations and Selection. We find that differences in selection move in lockstep with expectations about returning to the office. For all the cohorts hired

[36]We show robustness tables for these outcomes in Table B.20 -B.23.

before the offices closed, remote hires were less productive than on-site hires — even after the offices closed (Figure 4). This pattern persists largely unchanged soon after the offices close, when workers may have still expected on-site jobs to quickly require a return to the office. However, as the return to the office came to seem like a distant possibility, the differences in productivity narrowed. Indeed, during the winter of 2021 — when 61 percent of Americans believed that a return to normal pre-Covid life was at least 6 months away (Ipsos, 2021) — we see no appreciable productivity difference between new remote and on-site hires in Figure 4. Once remote and on-site work became a distinction without a difference, there ceased to be a difference in worker selection. The fact that selection changes with expectations about the pandemic's duration suggests that the initial gap was due to the jobs being remote versus on-site and not differences in geography or compensation that did not change over the course of the pandemic.

V Market Implications

Before the pandemic, call-center firms were trapped in a prisoner's dilemma that led to an underprovision of remote work. We develop a demand and supply framework for remote work that is microfounded in Appendix C where adverse selection arises because workers expect their productivity to be rewarded more on-site than at home. The equilibrium provision of remote work is determined by the intersection of workers' demand for remote work and firms' supply of remote work (Figure 5).

Workers' demand for remote work reflects their willingness to accept lower wages to work at home: Mas and Pallais (2017) find that call-center workers were willing to take an eight percent wage cut on average for the option to work at home. This high willingness to pay is corroborated in large-scale surveys (Maestas et al., Forthcoming; Barrero et al., 2022; Lewandowski et al., 2022).

On the supply side, firms weigh three factors: the savings in office space, the treatment effect of remote work, and differential selection into remote jobs.

Our estimates suggest that the cost of the negative treatment effect of remote work is more than offset by the savings on office real estate. To quantify the savings on real estate, we do a back-of-the-envelope calculation that suggests that our firm spent \$0.96 per worker-hour on office overhead for on-site workers.[37] To quantify the costs of remote work's negative treatment effect, we note that our firm pays \$4.78 on average for a call that does *not* lead to a call-back (given an average wage of \$15.69 per hour and average throughput of 3.3 calls per hour): thus, a negative treatment effect of 0.13 fewer calls per hour costs the firm \$0.63 per worker-hour (column 5 of Table 3). Thus, the savings in office real-estate from remote work (of \$0.96 per worker-hour) exceed the costs of remote work's negative treatment effect (of \$0.63 per worker-hour). As a result, all call-center firms would jointly be best off setting a wage *premium* of \$0.33/hour for remote work (the green line in Figure 5), which would lead to 84 percent of workers working remotely.

Negative selection into remote work makes it individually costly for firms to offer remote jobs. Our estimate indicates workers who choose remote jobs answer 0.24 fewer calls successfully per hour (column 5 of Table 6), which would cost the firm \$1.16 per worker-hour if it cannot screen for more productive workers.[38] Our estimates suggest that firms would only be willing to hire remote workers at a 5.3 percent wage penalty, which would offset the expected costs (of \$1.16 - \$0.33 =

[37] Typical office space needs run about 100 square feet per worker (Colacino, 2017). Firms like this one pay approximately \$20/square foot per year in rent and utilities for office space, using real-estate prices in the low-rent locations of the firm's call-centers. For a full-time worker, this amounts to \$0.96/hour in office costs (at \$20/square foot per year $\times$ 100 square feet per worker $\div$ 2,080 hours per worker-year = \$0.96/worker-hour in rent).

[38] Limited screening is reasonable given the high turnover and low work experience of call-center workers in entry-level jobs. Our model also assumes that workers fully internalize the benefits of promotion. Thus, our model overestimates remote work's costs to the extent that firms effectively screen out unproductive remote workers and underestimates remote work's costs to the extent that firms share in the benefits of developing and promoting workers.

$0.83 per worker-hour). Given Mas and Pallais (2017)'s estimates, 62 percent of workers would be willing to make this sacrifice.

Each individual firm acting rationally and in its own self-interest would set a sizeable wage penalty for remote work, even though all firms would collectively be better off if they did not penalize remote work.Our model suggests that this prisoner's dilemma is persistent: the selection effect of remote work drives a constant wedge in the market because, as the share of remote work rises, the pool of remote workers becomes less adversely selected, but the pool of on-site workers simultaneously becomes more *advantageously* selected.[39]

This prisoner's dilemma for firms leads to a deadweight loss for society. From society's perspective, attracting latently less productive workers into remote jobs at firm x does not impact overall output, since these workers would also be less productive in on-site jobs at firm y. Thus, attracting less productive workers is costly to any individual firm but not to society, causing the private costs of offering remote work to exceed the social costs. Our estimates suggest that the selection effect of remote work deters 22 percent of workers from working remotely. The distortion leads to a deadweight loss of $288 per year averaged over all workers.[40]

V.A Implications for a Post-pandemic World

Our findings suggest several reasons why the mass experiment with remote work during Covid-19 will permanently affect the market provision of remote work.

[39] A marginal worker switching from on-site to remote work simultaneously improves the productivity of on-site workers (who lose a worker with relatively low productivity and low returns to on-site work) and improves the productivity of remote workers (who gain a worker with relatively high productivity and high returns to on-site work). Under reasonable assumptions about the distributions of preferences and latent productivities, these two forces cancel out (Appendix C).

[40] Adverse selection into remote work may also be socially costly if society is particularly concerned about inframarginal remote workers, who choose remote work (1) because of latently low-ability or (2) because of strong tastes, such as those arising from caregiving responsibilities.

First, this mass experiment may have changed *who* choose remote jobs. Workers may increasingly sort into remote and on-site jobs on the basis of their preferences for working at home rather than their concerns about promotion. Workers may have learned more about their preferences for remote work, as seen in the increasing variance in workers' stated willingness to pay for remote work (Barrero et al., 2022) (Figure A.18). At the same time, stigma associated with remote work has fallen (Barrero et al., 2022), which may reduce workers' incentives to choose on-site jobs to improve their career opportunities. However, workers who anticipate on-site promotions may continue to shy away from remote work if there continue to be fewer opportunities for training and bonding with managers at home.

Second, the pandemic may have improved the treatment effect of remote work. Firms may have invested in management practices and informational technologies that mitigate remote work's negative productivity effects (Kwan, 2022).

Third, the mass experiment with remote work could have corrected firms' misperceptions about the productivity costs of remote work or overcome fixed adoption costs that previously depressed the supply of remote work below our model's predicted levels.

Consistent with these factors, the firm we study has chosen to close some but not all of its on-site call-centers over the course of the pandemic. In this firm, remote hires composed only 17.5 percent of the sample before the offices closed but 64.5 percent by April 2021. Similarly, in the American Community Survey, just 6.8 percent of phone workers were fully remote in 2019 but 32.7 percent were in 2021 (Figure A.17). These patterns suggest that the mass experiment with remote work may have — at least partially — freed firms from a prisoner's dilemma that led to an underprovision of remote work.

VI CONCLUSION

We consider why so few Americans worked remotely prior to Covid-19 even in remotable jobs. In our call-center context, the rarity of remote work seemed particularly puzzling since (1) workers expressed strong tastes for remote work (Mas and Pallais, 2017) and (2) existing evidence indicated that working remotely made workers more productive in call-center jobs (Bloom et al., 2015).

We ask two questions: how does remote work affect productivity, and how productive are the workers who choose remote jobs? To quantify each factor, we use data from an American Fortune 500 firm that hired both remote and on-site workers prior to Covid-19. Around the office closures of Covid-19, the hourly calls of on-site workers going remote fell by 4 percent relative to that of already-remote workers, indicating that negative treatment effects accounted for one third of the productivity gap. After the offices were closed, workers who initially chose remote jobs were 8 percent less productive than those who initially chose on-site jobs, even though all workers were working at home. Thus, two thirds of the initial productivity gap was due to worker selection.

Adverse selection consequently offers an important missing piece to the puzzle of remote work's rarity prior to Covid-19. Our estimates suggest that adverse selection distorts the decisions of 22 percent of call-center workers who do not choose to be remote because they do not want to pool with less productive types. There is promise that the pandemic could nudge the market into a more efficient equilbrium. Yet distortions will likely persist unless career opportunities can be equalized. Indeed, pre-pandemic remote workers were half as likely to be promoted as on-site workers, consistent with Bloom et al. (2015)'s RCT evidence.

Our paper has a few important limitations. We identify a negative but small treatment effect of remote work for relatively autonomous tasks but cannot speak to intensely collaborative tasks, where more negative effects have been found (Battiston et al., 2021; Gibbs et al., 2023). We also cannot directly assess why our treatment effect differs from those in other papers: understanding the role of performance pay, management practices, site selection (Allcott, 2015), or other contextual forces will be an important area of future work. Further, we cannot directly speak to hybrid-work arrangements, which may achieve the flexibility of work-from-home without some of the drawbacks of never going into the office (Bloom et al., 2022; Choudhury et al., 2022). Finally, while we hypothesize that the estimated selection effect stems from remote work's promotion penalty — which likely generalizes to other settings — we cannot test this conjecture.[41] Investigating the effects of remote work on worker productivity and worker selection in other contexts would help to diagnose the rarity of remote work in the past and predict its prevalence in the future.

[41] We also do not pinpoint the sources of lower promotion rates in remote jobs, which could reflect biased beliefs about remote workers' productivity (Dutcher and Saral, 2022), lesser opportunities to learn from coworkers (Emanuel et al., 2023), or fewer chances to schmooze with bosses (Cullen and Perez-Truglia, 2023).

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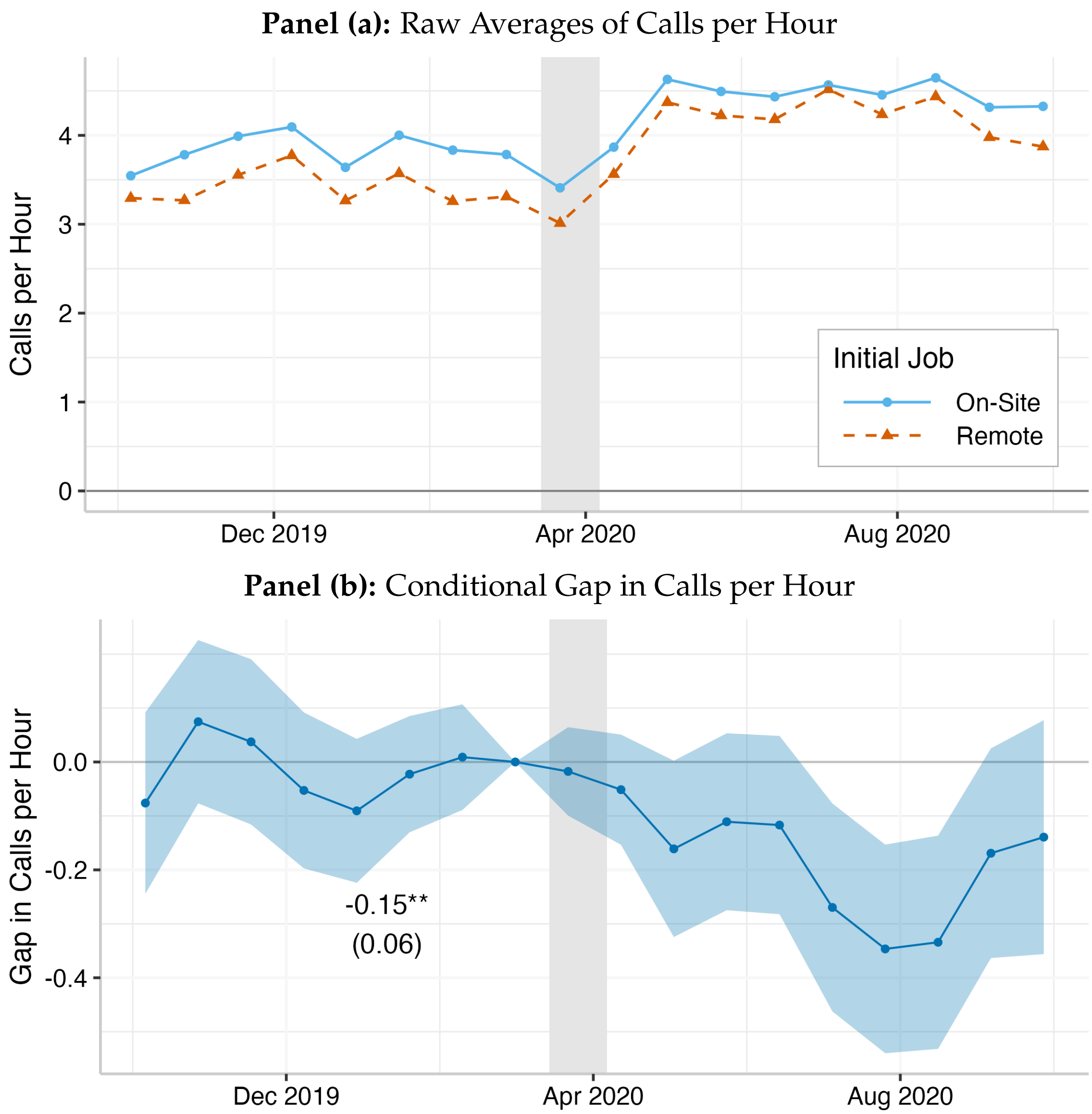
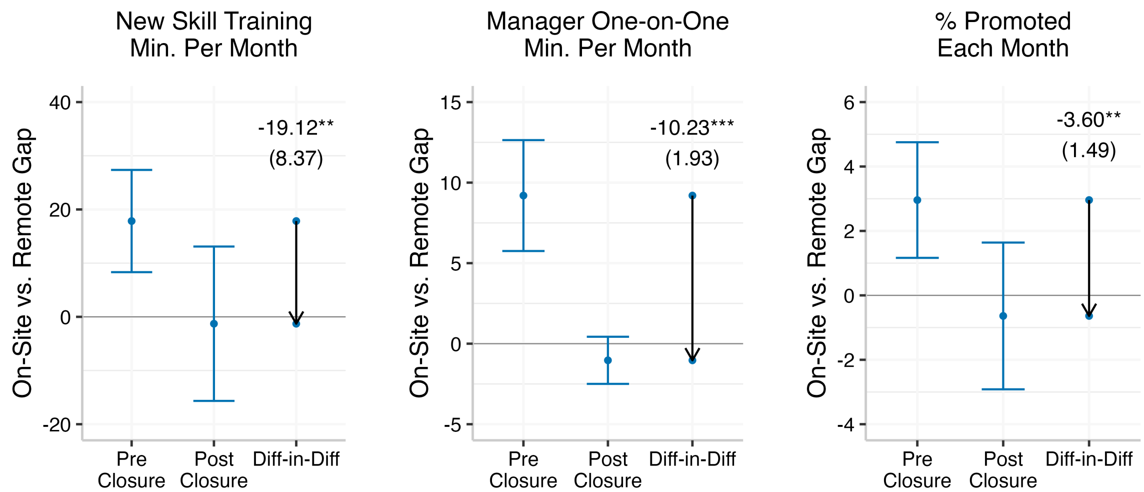
Figure 1: Difference-in-Differences Around Covid-19 Office Closures



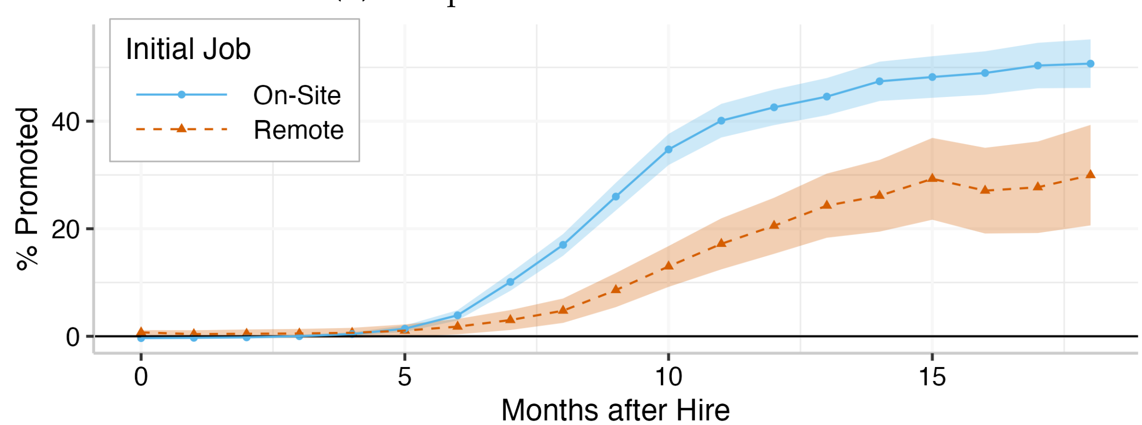
Note: This figure illustrates the difference-in-differences in calls taken per hour between on-site workers who went remote during the Covid-19 office closures (N=1,592) and remote workers who were already working from home (N=344). Panel (a) plots raw three-week averages. Panel (b) plots conditional gaps relative to February 16 to March 7, 2020, using our preferred set of controls for worker fixed effects, call-queue fixed effects, and time-varying effects of worker demographics (see Section II.C). The annotated coefficient indicates the difference-in-differences estimate of the effect of going remote from Equation 3, with a six month bandwidth excluding the grey shaded region, which spans from March 15, 2020 — when on-site workers could start working remotely — to April 6, 2020 — when the offices fully closed. Calls per hour is computed as the ratio of the number of calls answered over the number of hours scheduled for answering calls (as opposed to, e.g., answering emails or chat messages). The sample is our primary sample summarized in footnote 22. Ribbons reflect 95% confidence intervals. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure 2: Effect of Remote Work on Workers' Careers

Panel (a): Difference-in-Differences in Investments & Promotions

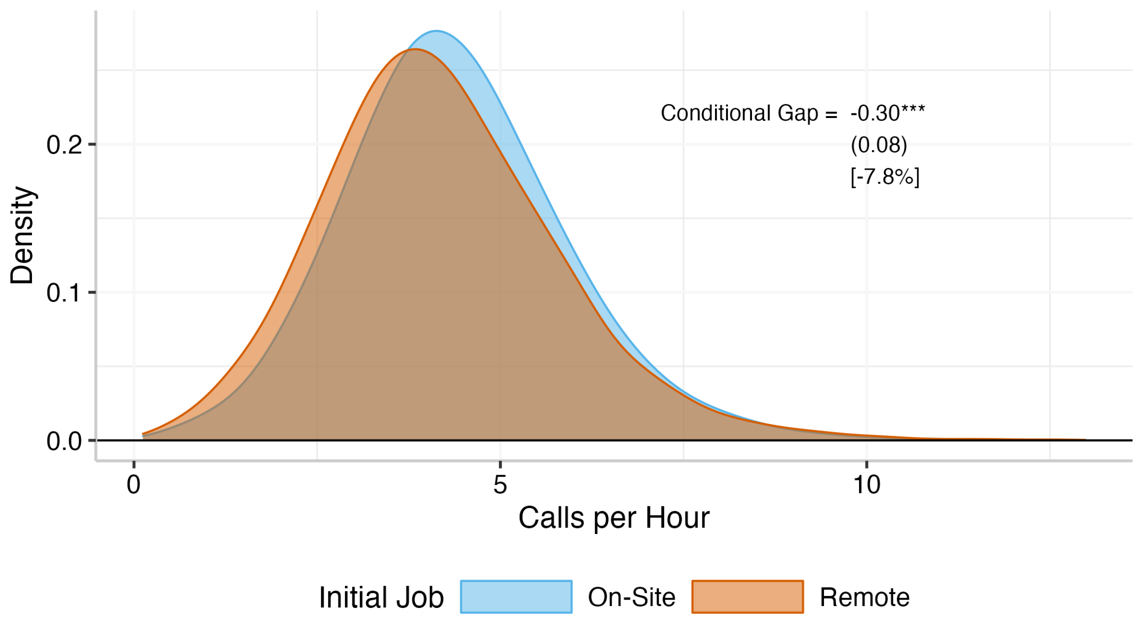


Panel (b): Pre-pandemic Promotion Differences



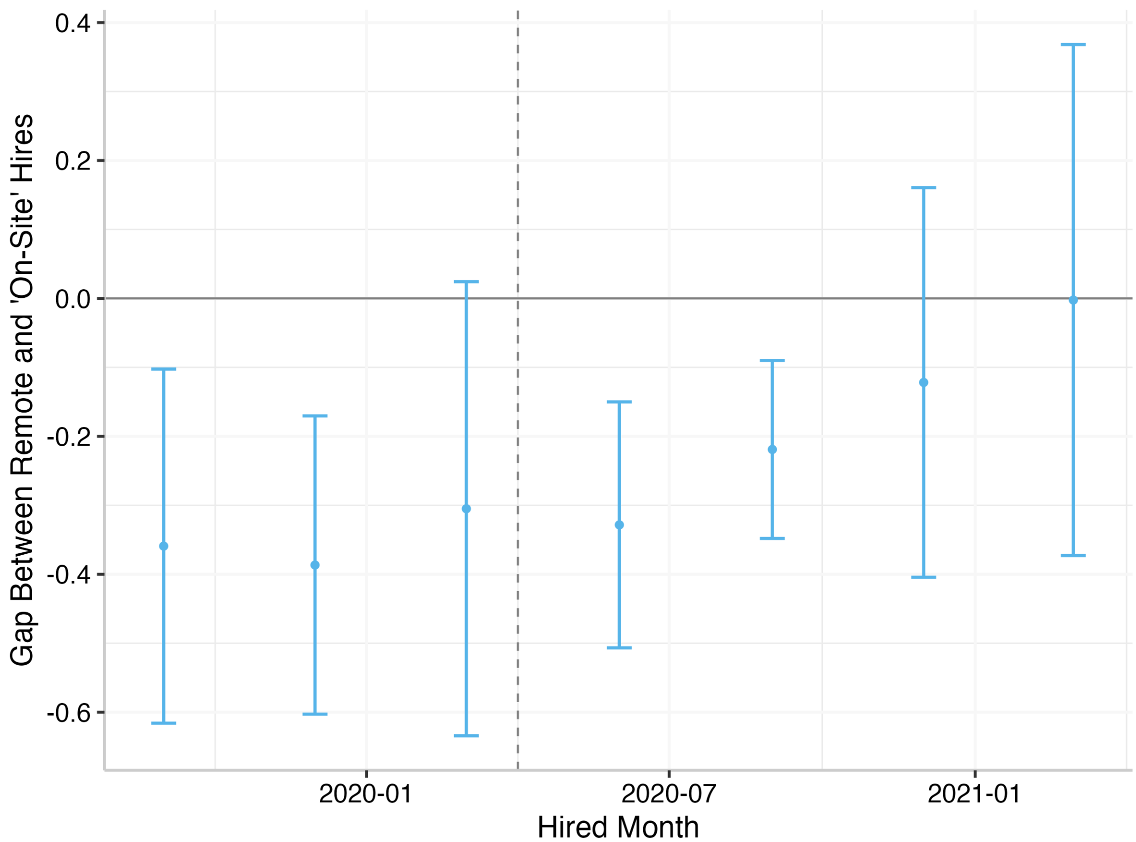
Note: This figure investigates remote work's impact on workers' careers. Panel (a) considers difference-in-differences in career investments and promotion outcomes. The left plot captures time spent on training for new skills each month; the middle plot captures time spent attending one-on-one meetings with managers; the right plot presents the percent of workers who are promoted to higher-stakes roles that feature 13-percent pay raises. In each plot, the first coefficient reflects the pre-period difference between remote and on-site workers; the second coefficient reflects the post-period differences; and the final arrow and coefficient reflect the difference-in-differences estimate. Each estimate includes call-queue fixed effects and date-by-hire-month to compare workers with similar tenure. Tables B.15-B.16 show robustness to alternative controls. Figure A.15 show the time-series averages. Panel (b) presents the share of workers who have been promoted as a function of the months since their hire date in the pre-pandemic period; Figure A.16 shows promotions conditional on persisting in the firm. Ribbons and error bars reflect 95 percent confidence intervals. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure 3: Productivity Differences When All Workers are Remote Due to Covid-19



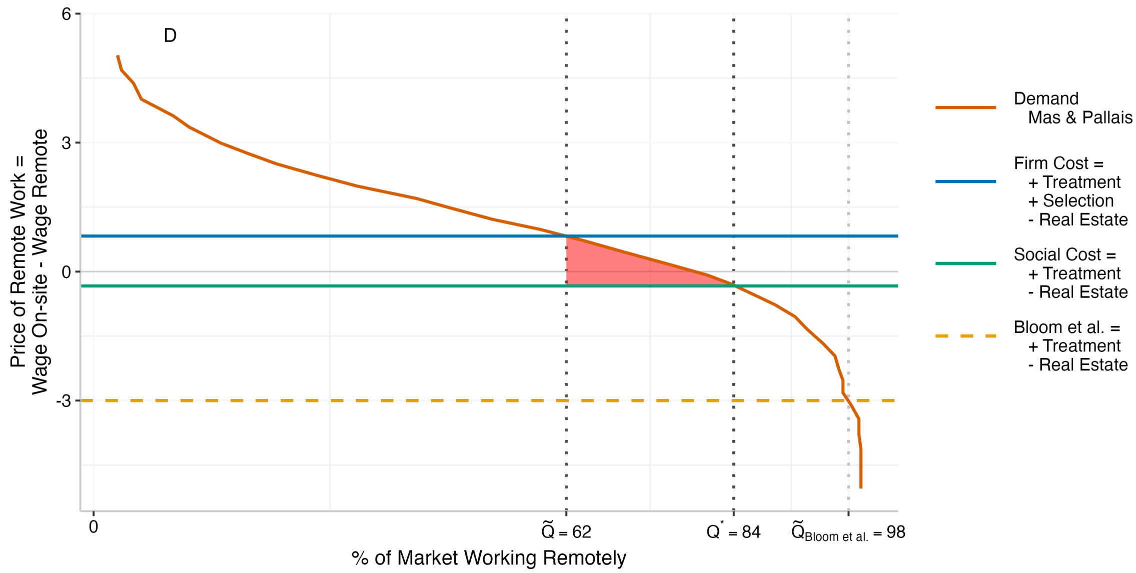
Note: This figure illustrates the differences in calls taken per hour between workers who initially chose on-site jobs (N=1,391) and those who initially chose remote jobs (N=242) in the six months after the offices closed (April 2020 to October 2020). The densities show the distribution of calls taken per hour on each worker-day. The annotated coefficient estimates Equation 4 using our preferred set of controls of call-queue fixed effects and worker age and gender. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). The sample is our primary sample summarised in footnote 22. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure 4: Productivity Differences Between Remote and On-Site Hires Based on Evolving Expectations of On-Site Work



Note: This figure illustrates the productivity gap between workers hired into on-site and remote jobs when everyone was working remotely due to Covid-19 between April 2020 and April 2021. Differences are shown separately for workers hired in different seasons. The sample is limited to seasons with at least 25 remote and 25 on-site hires and excludes workers who handle specialized calls. The vertical line highlights the office closures of Covid-19. On-site workers hired before the office closures (N = 741) expected to work on-site. Workers hired into on-site jobs after the office closures (N = 336) were told that they would eventually need to return to the office but would initially work remotely. Workers hired into remote jobs before the offices closed (N = 182) and after they closed (N = 1,549) never expected to work on-site. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). We include our preferred set of controls of call-queue fixed effects and worker age and gender (see Section II.C). Error bars reflect 95% confidence intervals, with standard errors clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure 5: Prisoner's Dilemma in the Market for Remote Work



Note: This figure illustrates how call-center firms could have been trapped in a prisoner's dilemma before the pandemic. All call-center firms would have better off offering remote work with no wage penalty, but any individual firm that did so would have disproportionately attracted less productive workers. In this demand and supply framework, the x-axis represents the percent of the market working remotely. The y-axis represents the price of remote work to workers or the wage gap between on-site and remote jobs. The estimated demand curve for remote work (in red) comes from Mas and Pallais (2017)'s real-stakes choice experiment. The estimated private cost of remote work (in dark blue) comes from our estimates of the treatment and selection effects of remote work on our composite measure of quantity and quality, net of the savings in office real-estate costs (explained in footnote 37). The estimated social cost of remote work (in green) comes from our estimate of remote work's treatment effect net of the savings in office real-estate costs. For reference, the orange dashed line uses Bloom et al. (2015)'s estimate of the treatment effect of remote work. The deadweight loss integrates over the losses of all the workers who work on-site in the market but would work remotely in the efficient solution. Appendix C microfounds the model, by assuming that workers have private information about their ability and know that their productivity will be more likely to be rewarded on-site than at home. In this model, the selection effect of remote work is constant because as the share of remote work rises, the pool of remote workers becomes less adversely selected at the same rate as the pool of on-site workers becomes more *advantageously* selected.

Table 1: Summary Statistics

			Before the Closures			After the Closures			
		All Workers	Initially On-Site	Initially Remote	Δ_0	Initially On-Site	Initially Remote	Δ_1	$\Delta_1 - \Delta_0$
1	Calls/Scheduled Hour	4.0	3.8	3.4	0.39***	4.2	4.0	0.22***	-0.18***
					(0.06)			(0.07)	(0.06)
Call Quantity Components									
2	% On Phone when Scheduled	76.8	74.3	71.8	2.53***	79.7	79.4	0.27	-2.26***
					(0.61)			(0.47)	(0.57)
3	Min. Per Call	13.0	13.2	14.3	-1.08***	12.5	13.3	-0.78***	0.30
					(0.26)			(0.21)	(0.22)
Call Quality Metrics									
4	Hold Min. Per Call	1.2	1.1	1.1	0.02	1.3	1.1	0.14***	0.12**
					(0.04)			(0.05)	(0.05)
5	% Call Back within Two Days	14.1	15.9	15.8	0.01	12.5	12.1	0.41**	0.40**
					(0.19)			(0.17)	(0.19)
6	Satisfaction Rating	4.8	4.9	4.9	-0.00	4.8	4.8	-0.00	-0.00
					(0.01)			(0.01)	(0.01)
Local Traits									
7	Wage	15.0	15.1	14.0	1.14***	15.3	14.0	1.26***	0.12***
					(0.03)			(0.03)	(0.02)
8	MSA CSR Wage	17.2	16.9	17.3	-0.35***	17.4	17.5	-0.18	0.17**
					(0.12)			(0.13)	(0.09)
9	Covid Cases Per 10K	0.3	0.0	0.0	0.00***	0.5	1.0	-0.48***	-0.48***
					(0.00)			(0.04)	(0.04)
Worker Traits									
10	Firm Tenure	248.1	194.1	190.3	3.82	297.5	303.1	-5.62	-9.44
					(9.61)			(12.29)	(7.52)
11	% Female	72.8	70.3	88.2	-17.87***	68.4	88.8	-20.37***	-2.50
					(2.42)			(2.56)	(1.70)
12	Age	34.6	33.5	37.9	-4.48***	34.1	38.3	-4.17***	0.31
					(0.71)			(0.81)	(0.46)
13	% Parent	42.3	39.9	54.7	-14.75***	40.2	50.4	-10.15**	4.60
					(4.95)			(4.78)	(2.82)
14	% Mother	35.4	32.6	52.5	-19.90***	31.8	48.2	-16.36***	3.55
					(4.92)			(4.73)	(2.80)
15	# Workers	1965	1592	344		1218	282		
16	# Caregiving Respondents	727	561	151		540	147		
17	# Our Survey Respondents	414	330	82		312	81		
18	# With Parenting Info	840	663	162		636	158		

Notes: This table characterizes the firm's on-site and remote call-center workers. The sample is limited to workers hired between July 1, 2018 — when the firm started hiring remote workers — and March, 15 2020 — when the firm let on-site workers start to work at home. The sample excludes workers who were hired to be on-site and then were permitted to transition to remote work before the pandemic, whom we analyze separately. The sample excludes workers who handle specialized calls for specific products or specific customers (like firms or non-English speakers), since these calls are not randomly assigned. Data on the mean wage in customer-service (CSR) in the worker's metropolitan statistical area (MSA) comes from the Occupational Employment and Wage Statistics (OES) (Bureau of Labor Statistics, 2021b). Data on Covid-19 cases and deaths come from data compiled in NYT (2021). Parenting information comes from a June 2020 survey conducted by the firm that we supplemented with our own survey in April 2021. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 2: Treatment Effect of Remote Work on Productivity: Difference-in-Differences Around Covid-19 Office Closures

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	-0.19^{***}	-0.14^{**}	-0.16^{*}	-0.15^{**}	-0.15^{**}	-0.21^{***}
	(0.07)	(0.07)	(0.08)	(0.06)	(0.06)	(0.08)
Initially On-Site	0.39^{***}	0.45^{***}	0.45^{***}			
	(0.06)	(0.06)	(0.08)			
Post	0.79^{***}					
	(0.06)					
County Covid Cases/10K					0.02	0.01
					(0.01)	(0.02)
Mother x Post						-0.04
						(0.06)
Father x Post						-0.14
						(0.13)
Pre Dependent Mean On-Site	3.8	3.8	3.8	3.8	3.8	3.8
Initially On-Site x Post in %	-5.1%	-3.6%	-4.1%	-3.9%	-3.9%	-5.5%
	(1.80)	(1.80)	(2.20)	(1.60)	(1.60)	(2.00)
Age x Gender x Post FE		✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓
Worker FE				✓	✓	✓
# Workers	1,965	1,965	1,965	1,965	1,965	712
# Initially On-site	1,621	1,621	1,621	1,621	1,621	566
# Already Remote	344	344	344	344	344	146
# Worker Days	224,447	224,447	224,447	224,447	224,447	126,603
R^2	0.05	0.08	0.17	0.44	0.44	0.45

Note: This table presents a difference-in-differences design that compares the change in productivity of on-site workers who went remote during the Covid-19 office closures to that of already-remote workers. The dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. Each specification estimates Equation 3 in a six month bandwidth excluding the period from March 15, 2020, when on-site workers could work from home, to April 6, 2020, when remote work was required. Table B.5 includes the full period and defines the post date as March 15, 2020. The call queue fixed effects specify the date, time-zone, and call-type. Covid-19 cases come from NYT (2021). Parenting characteristics in the fifth column come from a caregiving survey that the firm fielded in June of 2020 and that we supplemented in April of 2021. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 3: Treatment Effect of Remote Work on Call Quality

Panel (a): Difference-in-Differences Around Covid-19 Office Closures

	Decomposition		Call Quality			
	% On Phone	Min. Call	Hold Min. Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back Hour
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	-1.99^{***}	0.37^{*}	0.12^{**}	0.40^{**}	-0.002	-0.13^{**}
	(0.54)	(0.22)	(0.05)	(0.20)	(0.01)	(0.05)
R^2	0.63	0.38	0.18	0.13	0.09	0.42
Pre Mean On-Site	74.3	13.2	1.1	15.8	4.9	3.2
Initially On-Site x Post in %	-2.7%	2.8%	10.6%	2.5%	-0.03%	-4%
	(0.7)	(1.7)	(4.8)	(1.3)	(0.20)	(1.7)

Panel (b): Heterogeneity by Tenure

	(1)	(2)	(3)	(4)	(5)	(6)
Low Tenure x Initially On-Site x Post	-1.36^{*}	0.24	-0.04	-0.03	0.01	-0.11
	(0.78)	(0.30)	(0.06)	(0.25)	(0.01)	(0.07)
High Tenure x Initially On-Site x Post	-2.68^{***}	0.45	0.29^{***}	0.85^{***}	-0.01	-0.13^{*}
	(0.64)	(0.32)	(0.08)	(0.31)	(0.01)	(0.07)
R^2	0.63	0.38	0.18	0.13	0.09	0.42
Pre Mean On-Site, Low Tenure	71.7	12.9	1.2	16.1	4.9	3.2
Pre Mean On-Site, High Tenure	76.1	13.4	1.1	15.7	4.9	3.2
Percentage Effects						
Low Tenure x Initially On-Site x Post	-3.7%	3.5%	24.2%	5.3%	-0.18%	-4.11%
	(0.9)	(2.5)	(6.7)	(1.9)	(0.30)	(2.30)
High Tenure x Initially On-Site x Post	-1.8%	1.8%	-3.8%	-0.2%	0.12%	-3.44%
	(1.0)	(2.2)	(5.9)	(1.6)	(0.30)	(2.20)
Preferred Controls	✓	✓	✓	✓	✓	✓
# Workers	1,965	1,965	1,965	1,965	1,954	1,965
# Initially On-site	1,621	1,621	1,621	1,621	1,610	1,621
# Already Remote	344	344	344	344	344	344
# Worker Days	215,101	215,101	215,101	222,782	187,877	222,782

Note: This table presents difference-in-differences designs that compare the change in productivity metrics of on-site workers who went remote during the Covid-19 office closures to that of already-remote workers Panel (a) shows this for all workers. Panel (b) shows this separately for workers with low and high tenure, where we split by the median tenure of six months before the offices closed. Using a continuous measure of tenure yields similar heterogeneity (Table B.11). Each column estimates the preferred specification in column 4 of Table 2. Columns 1−2 decompose the change in call volumes into (1) the percent of workers' scheduled call time that they spend on the phone and (2) the average duration of each call in minutes. Columns 3−5 consider three metrics of call quality: (3) minutes that customers are kept waiting on hold; (4) the rate at which customers call back to the service line within two days, likely with unanswered questions; and (5) average customer satisfaction scores on a five-point scale. Column 6 considers a composite measure that captures the number of customer calls that do not lead to a call back that the worker answers each hour. Standard errors are clustered by worker. Data on call-time and hold-time is missing for 3.5 percent of observations. Satisfaction ratings are missing for 15.7 percent of worker-days because none of the worker's customers filled out the rating form. Results for the other outcomes are similar when limiting to these subsamples (Table B.10). ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 4: Treatment Effect From Switches to Remote Work Before Covid-19

		Decomposition		Call Quality			
	Calls / Hour	% On Phone	Min. / Call	Hold Min. / Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back / Hour
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Remote	-0.14^{***}	-1.18^{*}	0.94^{***}	0.01	-0.15	-0.001	-0.13^{***}
	(0.03)	(0.67)	(0.30)	(0.06)	(0.22)	(0.01)	(0.05)
Pre Mean for Switchers	4.0	74.3	12.6	1.0	15.5	4.9	3.3
Remote in %	-3.5%	-1.6%	7.5%	0.7%	-0.9%	-0.02%	-4%
	(0.8)	(0.9)	(2.4)	(5.8)	(1.4)	(0.20)	(1.5)
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Call-Queue Fixed Effects	✓	✓	✓	✓	✓	✓	✓
# Workers	2,570	2,570	2,570	2,570	2,570	2,555	2,570
# Switch to Remote	163	163	163	163	163	162	163
# Stay On-Site	2,407	2,407	2,407	2,407	2,407	2,393	2,407
# Worker Days	130,649	130,645	130,645	130,645	130,649	112,292	130,649
R^2	0.67	0.67	0.41	0.18	0.15	0.09	0.51

Note: This table presents difference-in-differences designs that compare the change in productivity of on-site workers who were permitted to go remote to that of workers who stayed on-site until the offices closed for Covid-19. Column 1 shows calls answered per hour that the worker is scheduled to answer customers' calls. Columns 2−3 decompose the change in call volumes into (2) the percent of workers' scheduled call time that she spends on the phone and (3) the average duration of each call in minutes. Columns 4−6 consider three metrics of call quality: (4) minutes that customers are kept waiting on hold; (5) the rate at which customers call back to the service line within two days, likely with unanswered questions; (6) average customer satisfaction scores on a five-point scale. The final column considers an alternative measure of productivity that considers the number of calls handled per hour that do not lead to a call back. Each specification estimates Equation 5 in a six-month bandwidth. As summarized in footnote 32, we follow the approach of (Dube et al., 2023) to limit the control group to workers who took calls from the same queue but stayed on-site until the pandemic. The call queue fixed effects specify the date, time-zone, and call-type. The sample excludes workers who handle specialized calls. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 5: Selection Effect of Remote Work: Productivity Differences When All Workers are Remote Due to Covid-19

	Calls per Hour						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Initially Remote	−0.20***	−0.31***	−0.30***	−0.30***	−0.24***	−0.27**	−0.21
	(0.07)	(0.07)	(0.08)	(0.08)	(0.09)	(0.11)	(0.13)
County Covid Cases/10K				0.01	0.02	0.02	0.02
				(0.02)	(0.02)	(0.02)	(0.02)
Base Pay					0.06	0.04	0.07
					(0.04)	(0.04)	(0.05)
Local Outside Option Pay in MSA						0.03	0.04
						(0.03)	(0.03)
Unemployment Rate in MSA						−0.01	−0.004
						(0.02)	(0.02)
Mother							0.07
							(0.08)
Father							−0.04
							(0.15)
Pre Dependent Mean On-Site	3.8	3.8	3.8	3.8	3.8	3.8	3.8
Initially Remote in %	-5.3%	-8.2%	-7.8%	-7.9%	-6.4%	-7.2%	-5.5%
	(1.9)	(1.9)	(2.1)	(2.1)	(2.4)	(2.9)	(3.4)
Age x Gender FE		✓	✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436	1,436	1,436	666
# Initially On-site	1,174	1,174	1,174	1,174	1,174	1,174	529
# Already Remote	262	262	262	262	262	262	137
# Worker Days	108,174	108,174	108,174	108,174	108,174	108,174	70,453
R^2	0.002	0.03	0.13	0.13	0.13	0.13	0.16

Notes: This table presents the differences in calls taken per hour between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months after the offices closed. Each specification estimates Equation 4. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). Call queue fixed effects specify the date of the call, the worker's time-zone, and the call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020 and that we supplemented with a survey run in April of 2021. The sample is our primary sample summarized in footnote 22. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 6: Selection Effect of Remote Work: Auxiliary Measures

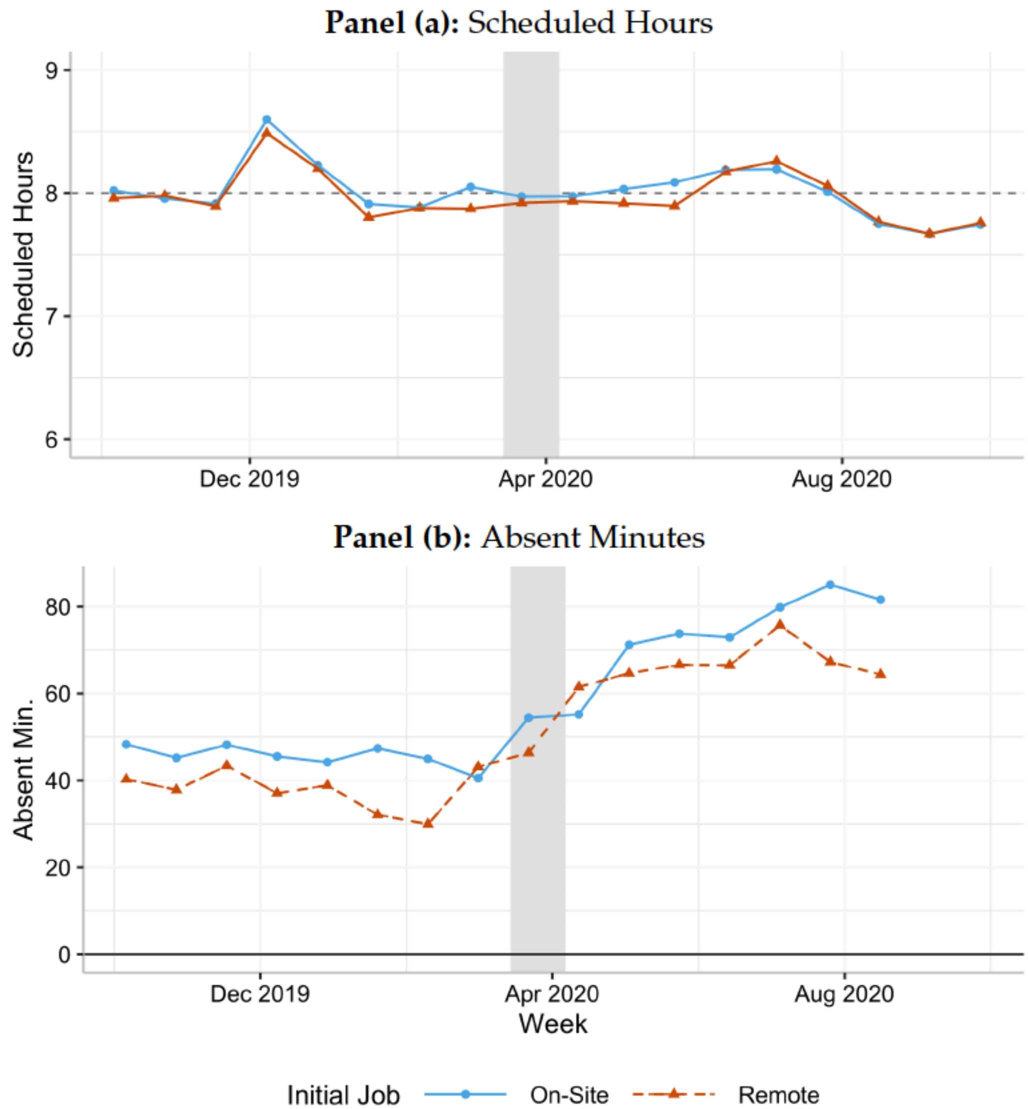
	Decomposition		Call Quality			
	% On Phone	Min. Call	Hold Min. Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back Hour
	(1)	(2)	(3)	(4)	(5)	(6)
Initially Remote	-0.54	0.95^{***}	-0.02	-0.62^{***}	0.01	-0.24^{***}
	(0.50)	(0.25)	(0.06)	(0.20)	(0.01)	(0.07)
Pre Mean On-Site	74.3	13.2	1.1	15.9	4.9	3.2
Initially Remote in %	-0.7%	7.2%	-2.2%	-3.9%	0.25%	-7.4%
	(0.7)	(1.9)	(5.2)	(1.3)	(0.23)	(2.20)
Preferred Controls	✓	✓	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436	1,429	1,436
# Initially On-site	1,174	1,174	1,174	1,174	1,168	1,174
# Initially Remote	262	262	262	262	261	262
# Worker Days	100,414	99,503	100,414	108,174	89,143	108,174
R^2	0.46	0.08	0.12	0.08	0.08	0.13

Note: This table presents the differences between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months after the offices closed. Columns $1-2$ decompose the difference in call volumes into (1) the percent of workers' scheduled call time that she spends on the phone and (2) the average duration of each call in minutes. Columns $3-5$ consider three metrics of call quality: (3) minutes that customers are kept waiting on hold; (4) the rate at which customers call back to the service line within two days, likely with unanswered questions; and (5) average customer satisfaction scores on a five-point scale. Column 6 considers an alternative measure of productivity that considers the number of customer calls that do not lead to a call back that the worker answers each hour. Each specification estimates Equation 4, including our preferred set of controls for demographics and call-queue fixed effects. The sample is our primary sample summarized in footnote 22. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Appendix For Online Publication

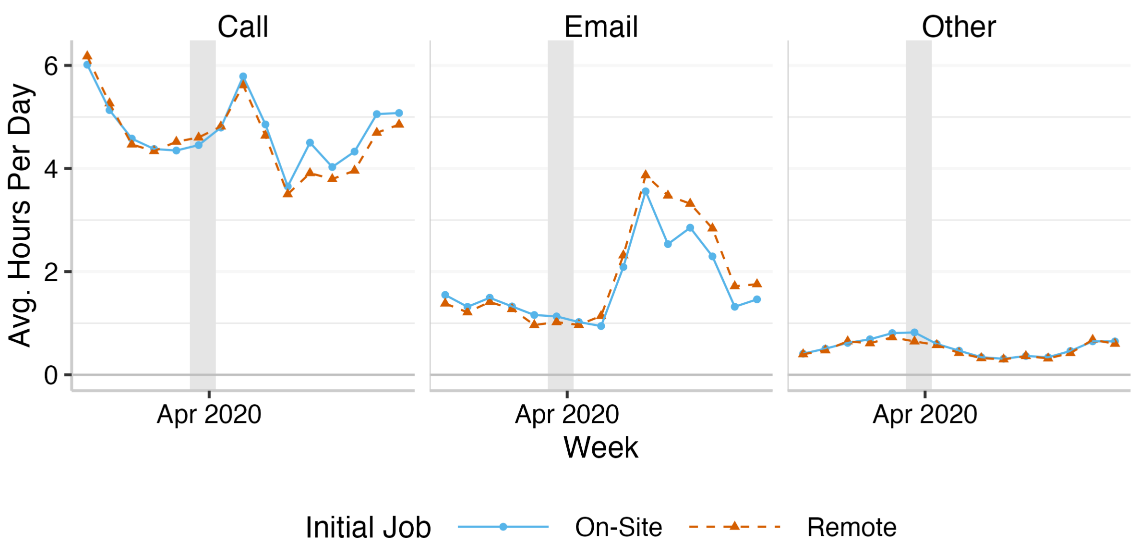
A Appendix Figures

Figure A.1: Schedules and Absenteeism for Initially Remote and On-Site Workers Around the Covid-19 Office Closures



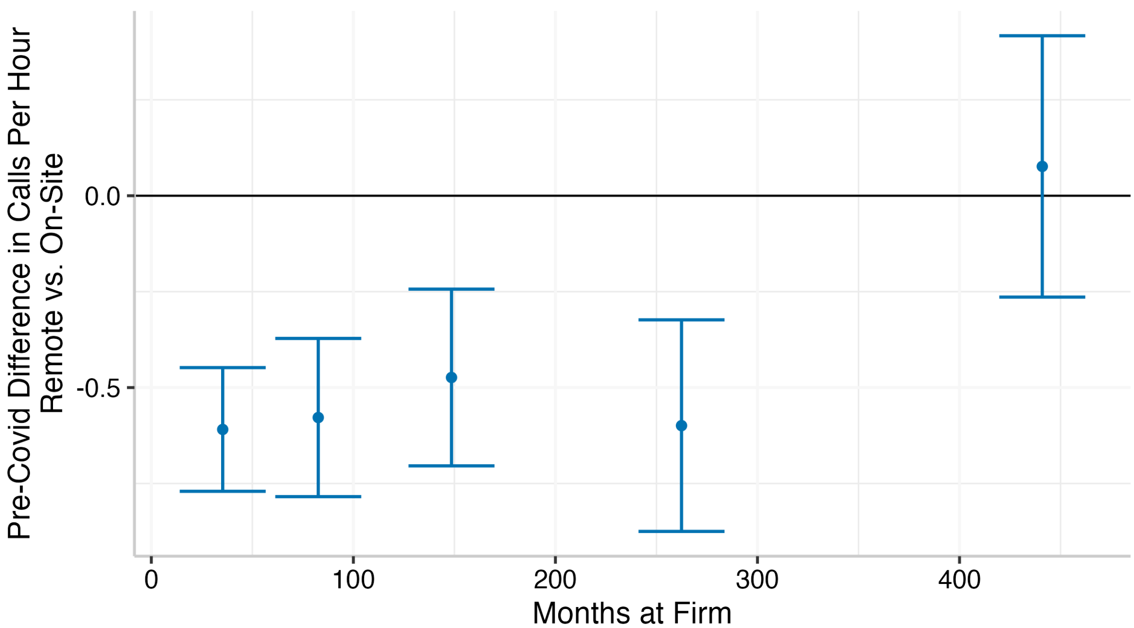
Note: This figure illustrates the patterns of (a) scheduled hours and (b) absenteeism of on-site workers who went remote during the Covid-19 office closures (N=1,592) and workers who were already remote (N=344). The shaded region spans from March 15, 2020 — when on-site workers could start working remotely — to April 6, 2020 — when the offices fully closed. The sample is our primary sample summarized in footnote 22.

Figure A.2: Scheduled Time Per Day for Initially Remote and On-Site Workers Around the Covid-19 Office Closures



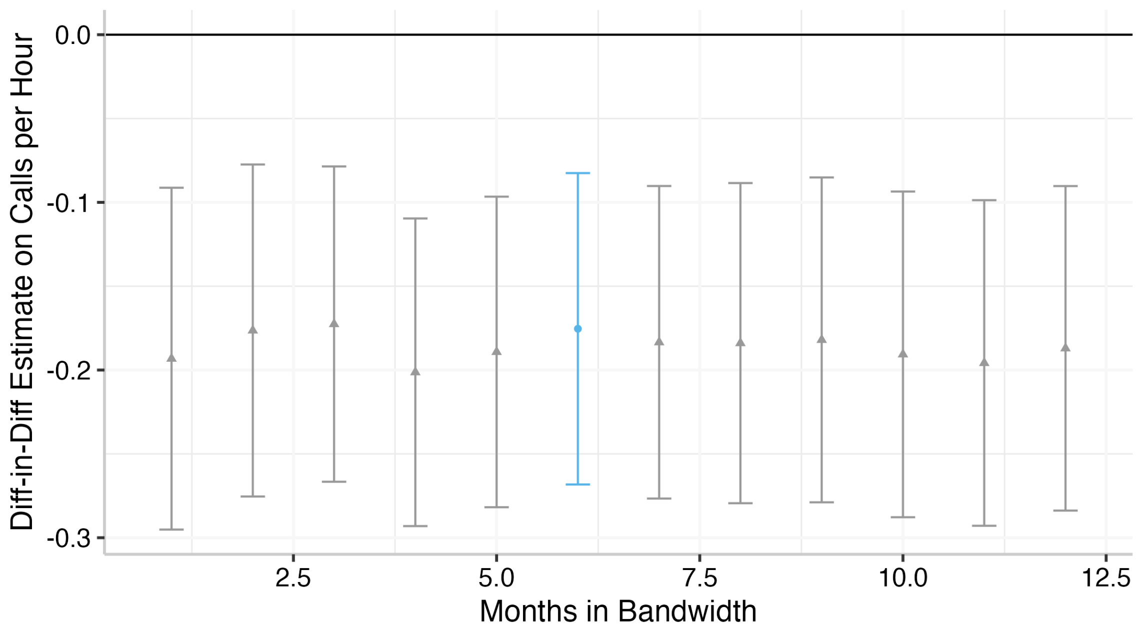
Note: This figure illustrates the changes in the scheduled time of on-site workers who went remote during the Covid-19 office closures (N=1,592) and workers who were already remote (N=344). The left plot shows hours scheduled for answering customer calls. The middle plot shows hours scheduled for answering customer emails or instant messages. The right plot shows hours scheduled for other activities, such as training, meetings, and breaks. The shaded region spans from March 15, 2020 — when on-site workers could start working remotely — to April 6, 2020 — when the offices fully closed. The sample is our primary sample summarized in footnote 22.

Figure A.3: Pre-pandemic Differences in Performance



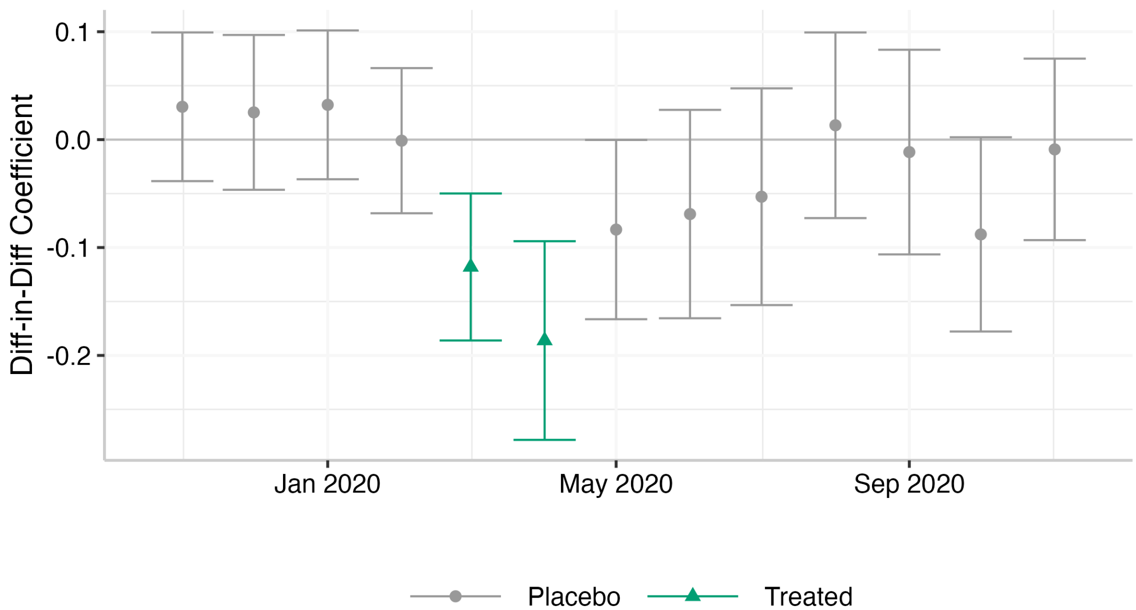
Note: This figure illustrates the differences in call quantity for on-site and remote workers as a function of their time at the firm. The sample is our preferred sample summarised in footnote 22. Each point represents a different quintile of firm tenure. The differences condition on the queue of the call, determined by the call-level, time-zone, and date. Error ribbons reflect 95 percent confidence intervals, with standard errors clustered by worker.

Figure A.4: Robustness of Diff-in-Diff Estimate to Alternative Bandwidths



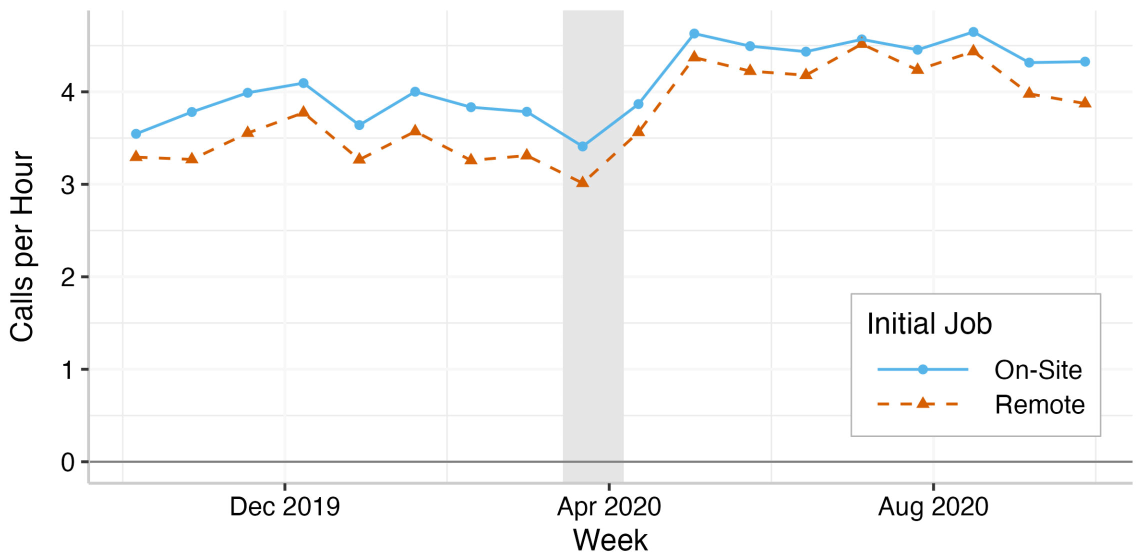
Notes: This figure illustrates difference-in-differences estimates that compare the change in calls per hour for on-site and remote hires around the office closures within various bandwidths. The blue circle shows the estimate with our preferred six-month bandwidth. The grey triangles show estimates with alternative bandwidths. All regressions estimate Equation 3 with our preferred controls for worker fixed effects, call-queue fixed effects, and time-varying effects of worker demographics (see Section II.C). The error bars are 95% confidence intervals with standard errors clustered by worker.

Figure A.5: Placebo Treatment Dates



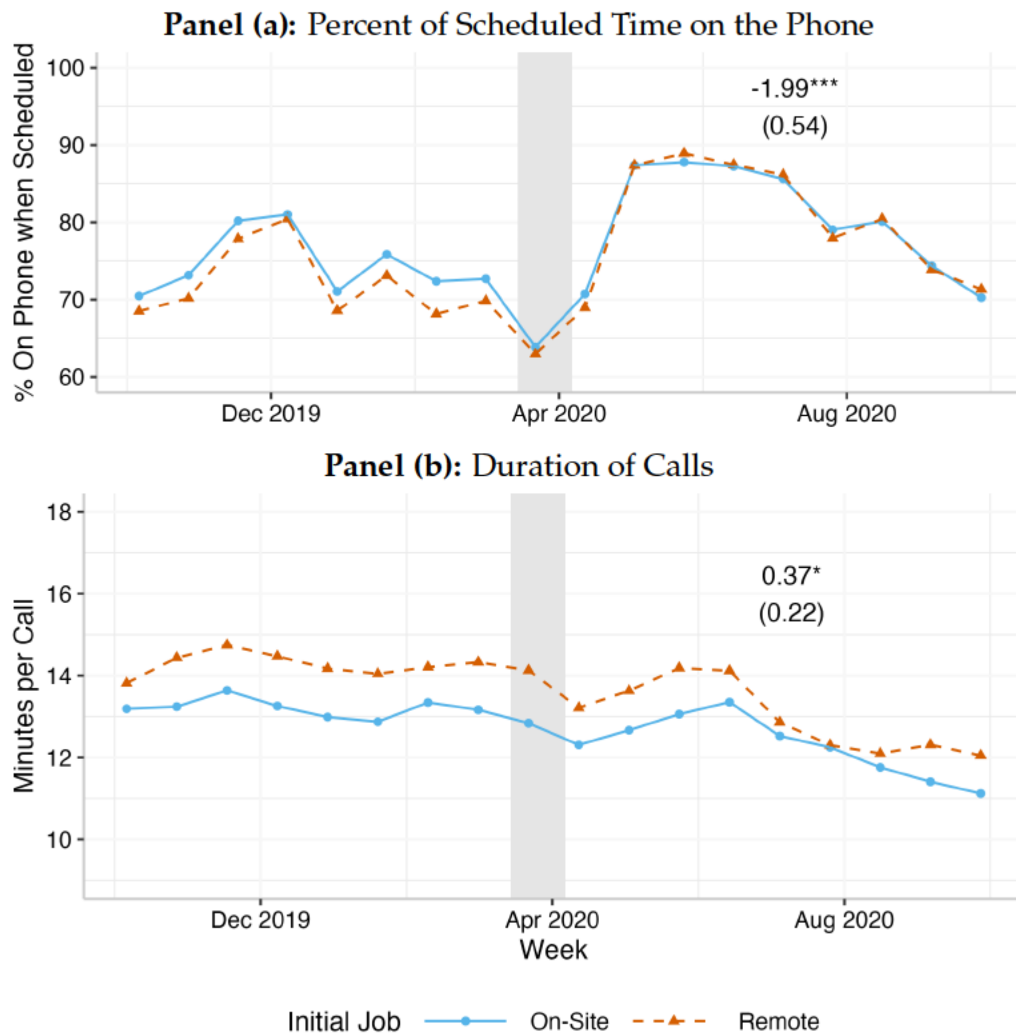
Notes: This figure illustrates difference-in-differences estimates that compare the change in calls per hour for on-site and remote hires within two-month bandwidths. The grey circles show periods that do not include the treated window; the green triangles include the treated window. All regressions estimate Equation 3 using our preferred controls for worker fixed effects, call-queue fixed effects, and time-varying effects of worker demographics (see Section II.C). The error bars are 95% confidence intervals with standard errors clustered by worker.

Figure A.6: Difference-in-Differences around Covid-19 Office Closures in Locations with $14/hour Pay



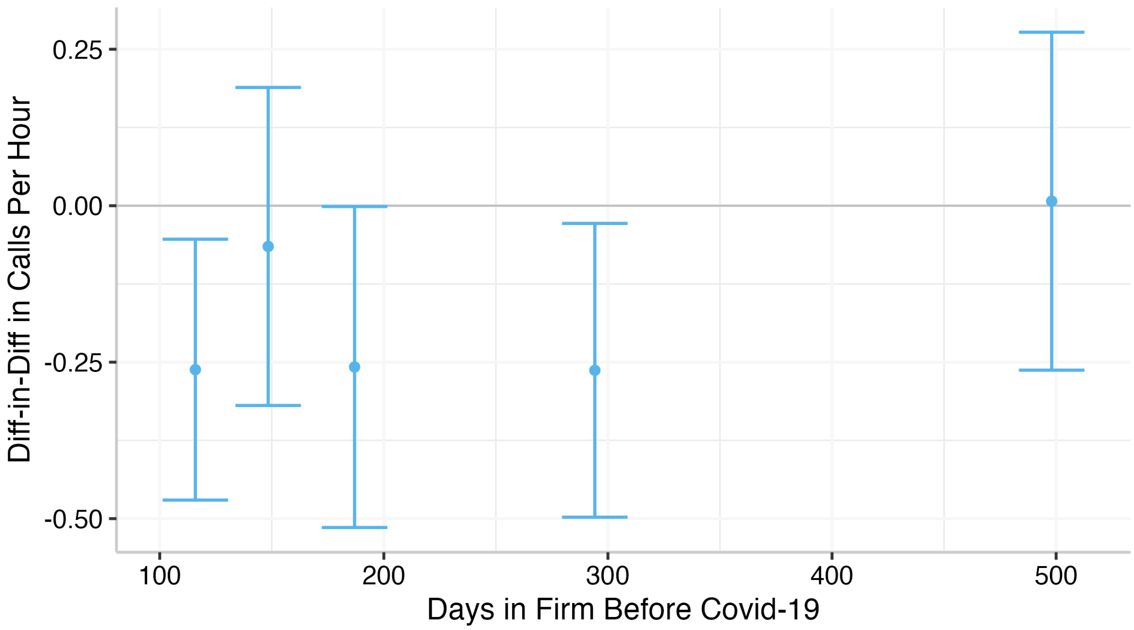
Notes: This figure illustrates a difference-in-differences design that compares the change in calls per hour for on-site and remote hires, limiting to on-site locations with base-pay of $14 per hour. Each point represents a raw three-week average. The shaded region spans from March 15, 2020 — when on-site workers could start working remotely — to April 6, 2020 — when offices fully closed. Calls per hour is computed as the ratio of the number of calls answered over the number of hours that the worker was scheduled to answer calls that day. The sample limits our preferred sample summarized in footnote 22 to on-site locations with base pay of $14 per hour.

Figure A.7: Decomposition of Effects on Calls



Note: This figure decomposes remote work's effect on calls per hour into (a) time spent on the phone and (b) call durations. In Panel (a), the percent of time on the phone is computed as the ratio of a worker's time on the phone to the time that she was scheduled to be taking calls. In Panel (b), the average duration of completed calls is computed as the time that the worker spent on the phone divided by the number of calls that she handled herself (rather than forwarding to another worker). Each panel considers a difference-in-differences design that compares on-site hires who went remote during the Covid-19 office closures (N=1,592) and workers who were already remote (N=344). The shaded region spans from March 15, 2020 — when on-site workers could start working remotely — to April 6, 2020 — when the offices fully closed. The annotated coefficients indicate the difference-in-differences estimate of the effect of going remote from Equation 3, with a six-month bandwidth excluding the shaded region. The controls are our preferred controls for worker fixed effects, call-queue fixed effects, and time-varying effects of worker demographics (see Section II.C). These coefficients are also reported in Table 3. The sample is our primary sample summarised in footnote 22. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

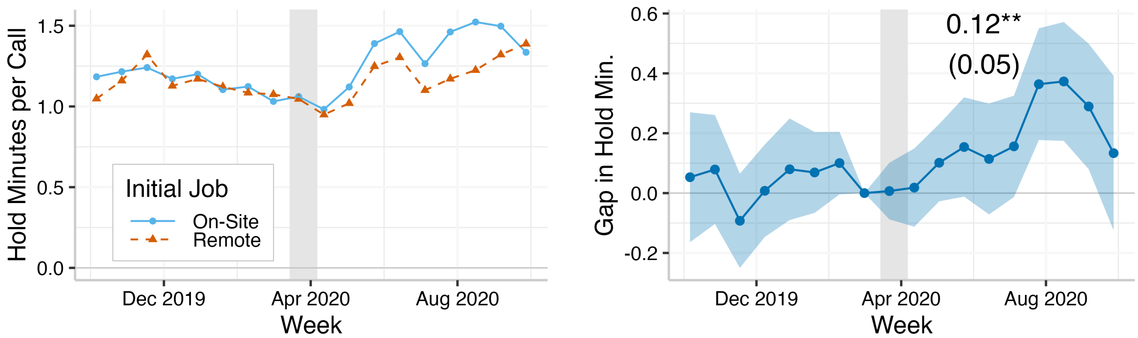
Figure A.8: Consistent Effects on Call per Hour by Tenure



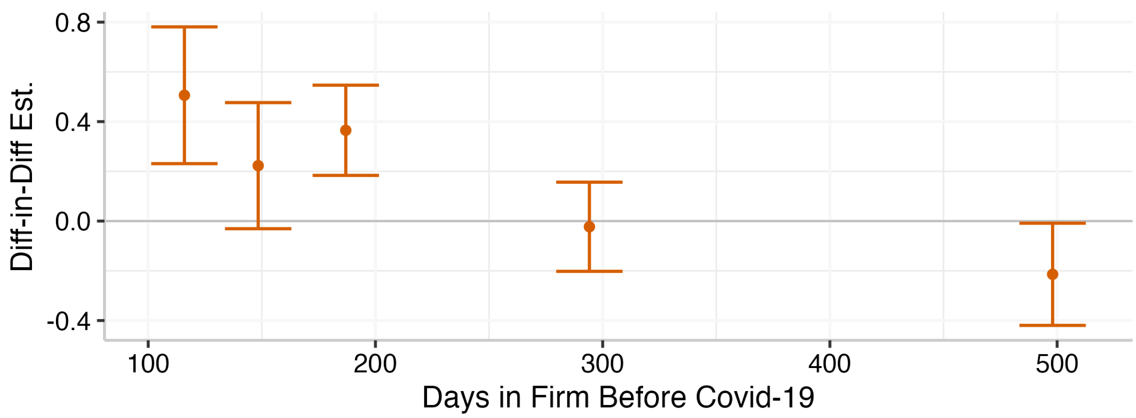
Note: This figure investigates heterogeneous effects of remote work on calls handled per hour by workers' tenure prior to the office closures of Covid-19. Each point captures a different quintile of worker tenure. Each estimate reflects the difference-in-differences design, which compares on-site workers who went remote during the Covid-19 office closures to already-remote workers. All specifications estimate Equation 3, using our preferred set of controls for worker fixed effects, call-queue fixed effects, and time-varying effects of worker demographics (see Section II.C). The sample is our primary sample summarized in footnote 22. The error bars reflect 95 percent confidence intervals with standard errors clustered by worker. There are 426 employees in Q1 (351 who were initially on-site and 75 who were initially remote); 378 employees in Q2 (302 who were initially on-site and 76 who were initially remote); 388 employees in Q3 (333 who were initially on-site and 55 who were initially remote); 388 employees in Q4 (312 who were initially on-site and 76 who were initially remote); and 385 employees in Q5 (323 who were initially on-site and 62 who were initially remote). Table B.11 shows results for a continuous measure of tenure. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure A.9: Challenges in Receiving Coworker Input When Remote

Panel (a): Difference-in-Differences in Hold Times



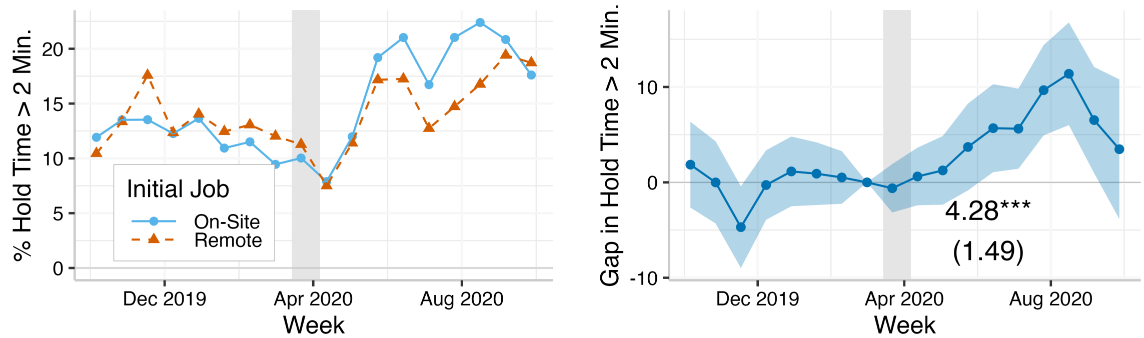
Panel (b): Heterogeneous Difference-in-Differences Effects by Worker Tenure



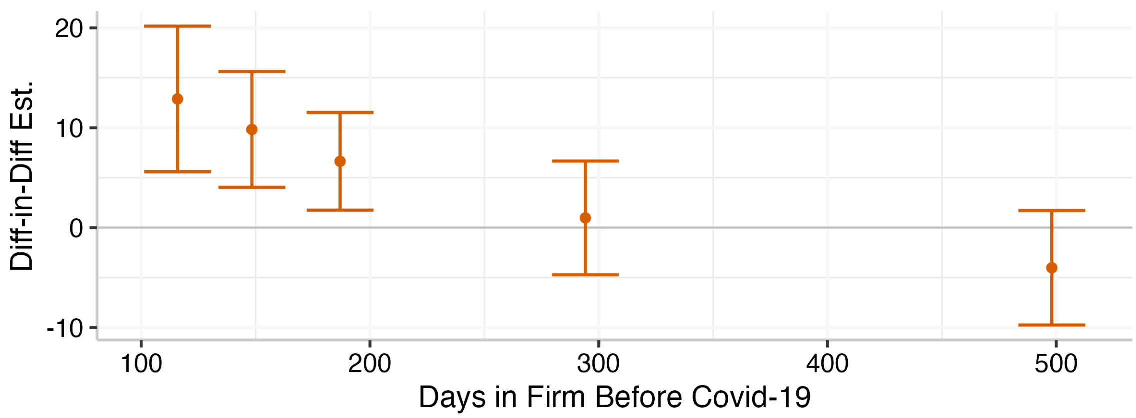
Note: This figure investigates remote work's impacts on customer hold times by worker experience. Panel (a) shows differences in hold times between remote and on-site workers before the office closures. The annotated coefficients represent differences between remote and on-site workers with call-queue fixed effects for the significant differences for junior and senior workers. Panel (b) repeats the analysis in Figure 1 for minutes that customers are kept waiting on hold. Panel (c) presents heterogeneity in these difference-in-difference estimates by workers experience at the time that the offices closed for Covid-19. Ribbons and error bars reflect 95% confidence intervals, with standard errors clustered by worker. Figure A.10 shows these patterns for hold times in excess of two minutes. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure A.10: Difference-in-Differences in Hold Times Over Two Minutes

Panel (a): Difference-in-Differences in Hold Times Over Two Minutes

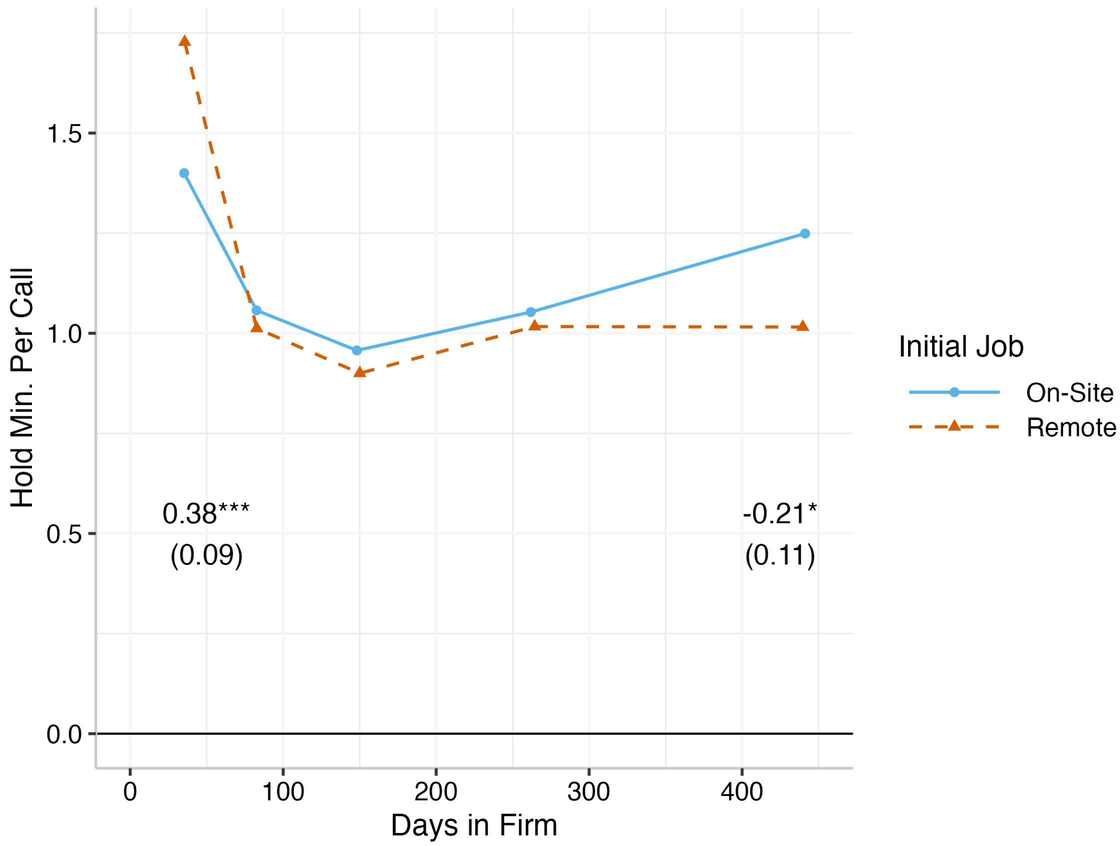


Panel (b): Heterogeneous Effects by Worker Tenure



Note: This figure investigates remote work's impacts on customer hold times by worker experience, focusing on hold times exceeding two minutes. Panel (a) repeats the analysis in Figure 1 for the share of workers who keep customers waiting on hold for more than two minutes on average. Panel (b) presents heterogeneity in these difference-in-difference estimates by quintiles of workers' experience at the time that the offices closed for Covid-19. Ribbons and error bars reflect 95% confidence intervals, with standard errors clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

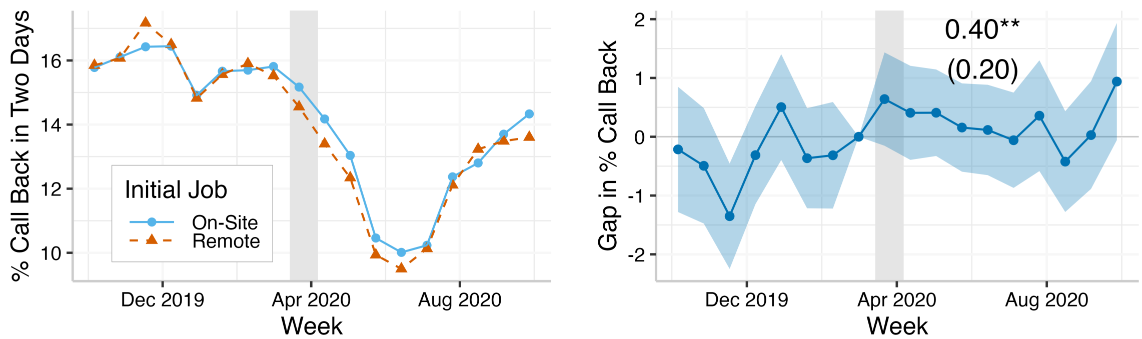
Figure A.11: Differences in Hold Times Before the Office Closures



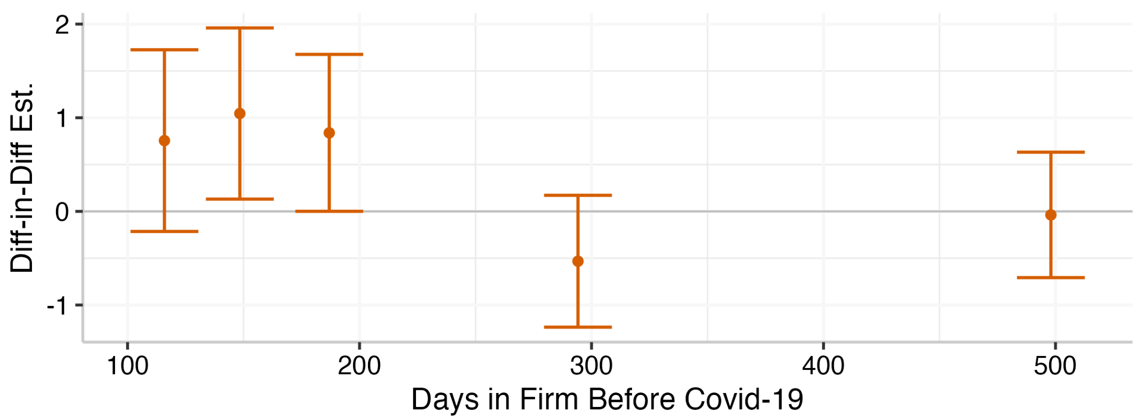
Note: This figure shows differences in hold times between remote and on-site workers before the office closures. The annotated coefficients represent differences between remote and on-site workers with call-queue fixed effects for the significant differences for junior and senior workers. Ribbons and error bars reflect 95% confidence intervals, with standard errors clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure A.12: Difference-in-Differences in Callback Rates

Panel (a): Difference-in-Differences



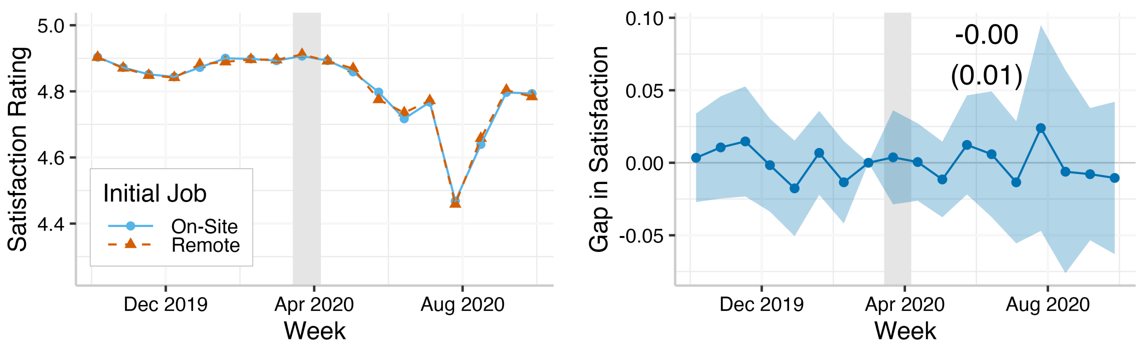
Panel (b): Heterogeneous Difference-in-Differences Effects by Tenure



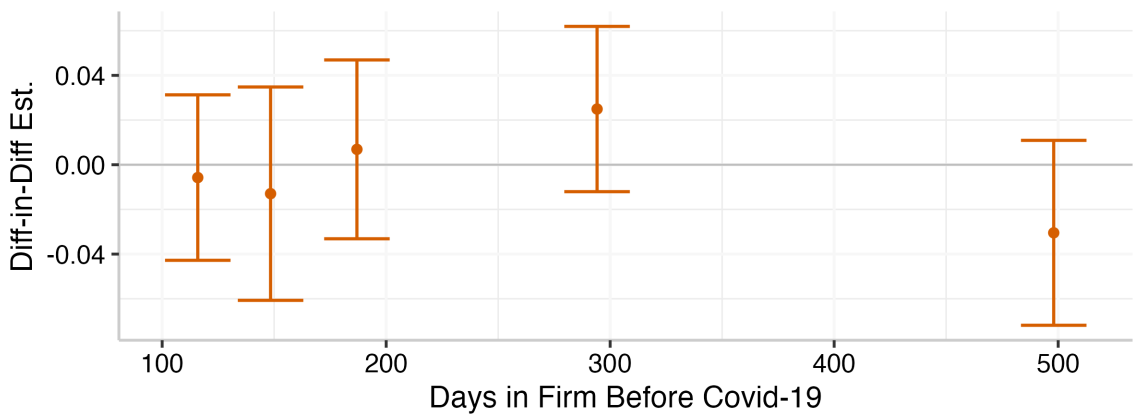
Note: This figure investigates remote work's impacts on the rate at which customers call back to the service line within two days, likely with initially unanswered questions. Panel (a) repeats Figure 1 for this quality measure. The difference-in-differences coefficient is also reported in column 4 of Table 3. Panel (b) presents the difference-in-difference estimates separately by quintile of workers' tenure at the firm when the offices closed. Ribbons and error bars reflect 95% confidence intervals, with standard errors clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure A.13: Difference-in-Differences in Satisfaction

Panel (a): Average Customer Satisfaction Ratings

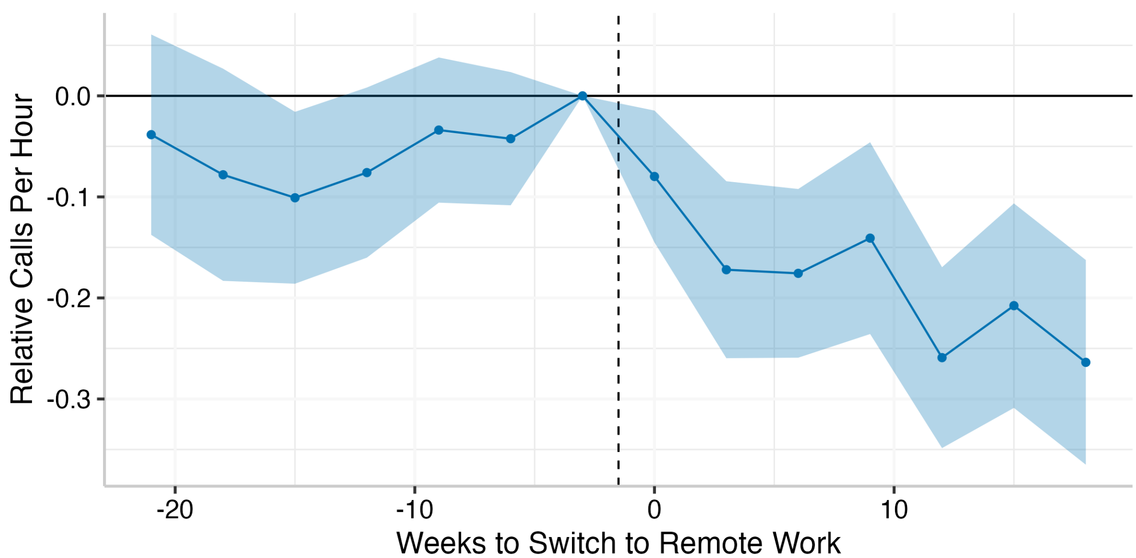


Panel (b): Heterogeneous Difference-in-Differences Effects by Tenure



Note: This figure investigates remote work's impacts on average customer satisfaction scores on a five-point scale. Panel (a) repeats Figure 1 for this quality measure. The difference-in-differences coefficient is also reported in column 5 of Table 3. Panel (b) presents the difference-in-difference estimates separately by quintile of workers' tenure at the firm when the offices closed. Ribbons and error bars reflect 95% confidence intervals, with standard errors clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

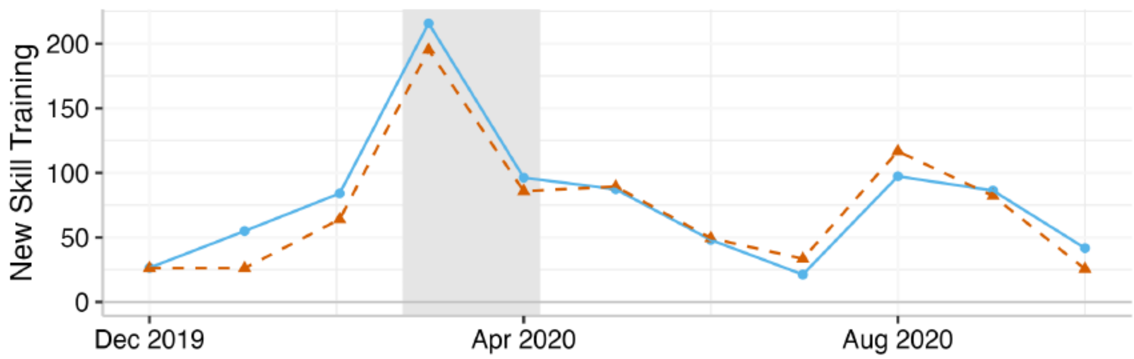
Figure A.14: Switches to Remote Work Before Covid-19



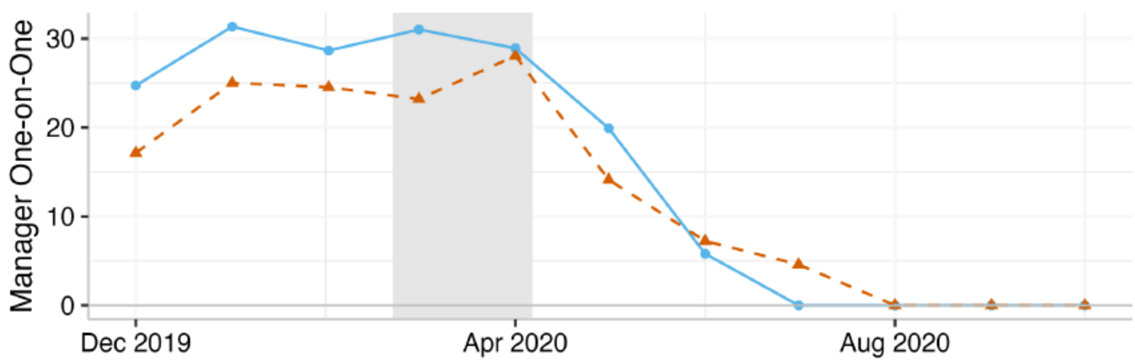
Note: This figure illustrates the changes in calls per hour for workers who transitioned from on-site to remote work prior to the pandemic. The figures shows a difference-in-differences design that compares the change in calls handled of on-site workers who were permitted to go remote to that of workers who stayed on-site until the offices closed for Covid-19. Calls per hour is defined as calls answered per hour that the worker is scheduled to answer customers' calls. The figure plots conditional differences relative to the three weeks before the transition to remote work. The controls include worker fixed effects and call-queue fixed effects that specify the date, time-zone, and type of call. We follow the approach of (Dube et al., 2023) to limit the control group to workers who took calls from the same queue but stayed on-site until the pandemic as summarized in footnote 32. The sample excludes workers who handle specialized calls. Ribbons reflect 95% confidence intervals with standard errors clustered by worker.

Figure A.15: Effect of Remote Work on Workers' Careers: Time Series

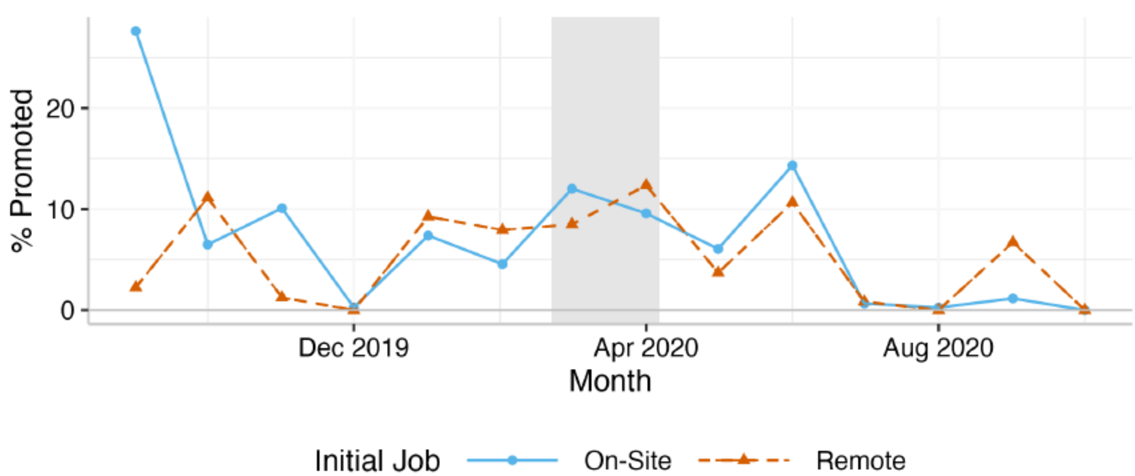
Panel (a): Diff-in-Diff in New Skill Training Minutes per Month



Panel (b): Diff-in-Diff in Manager One-on-One Meeting Minutes per Month

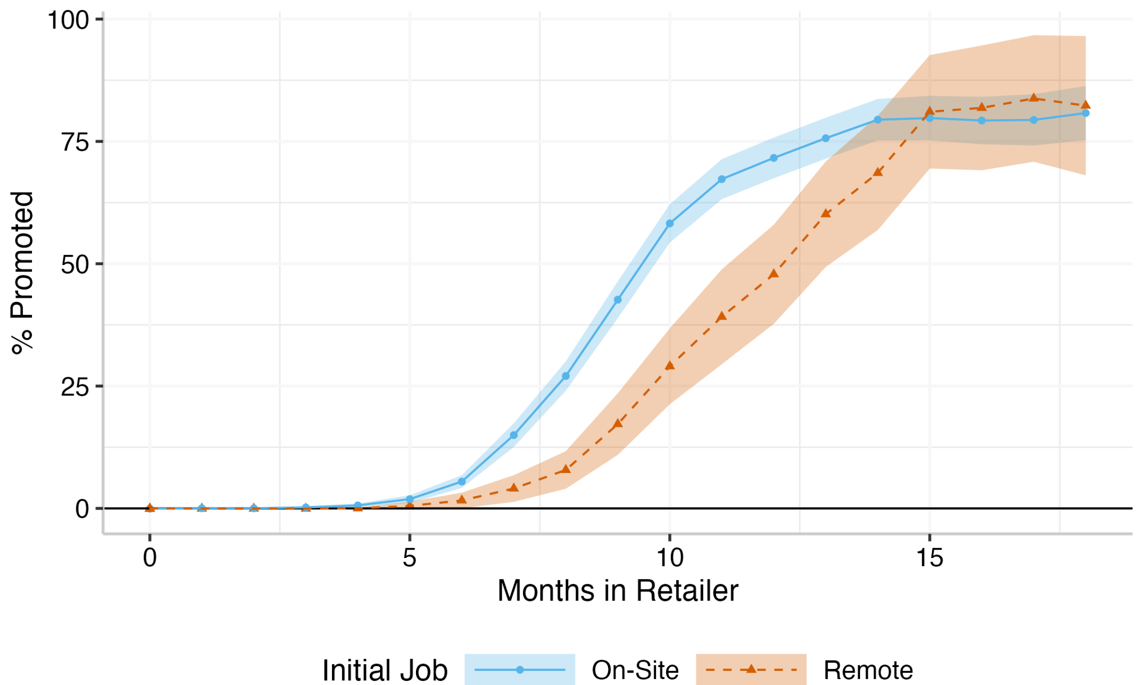


Panel (c): Diff-in-Diff in Promotions



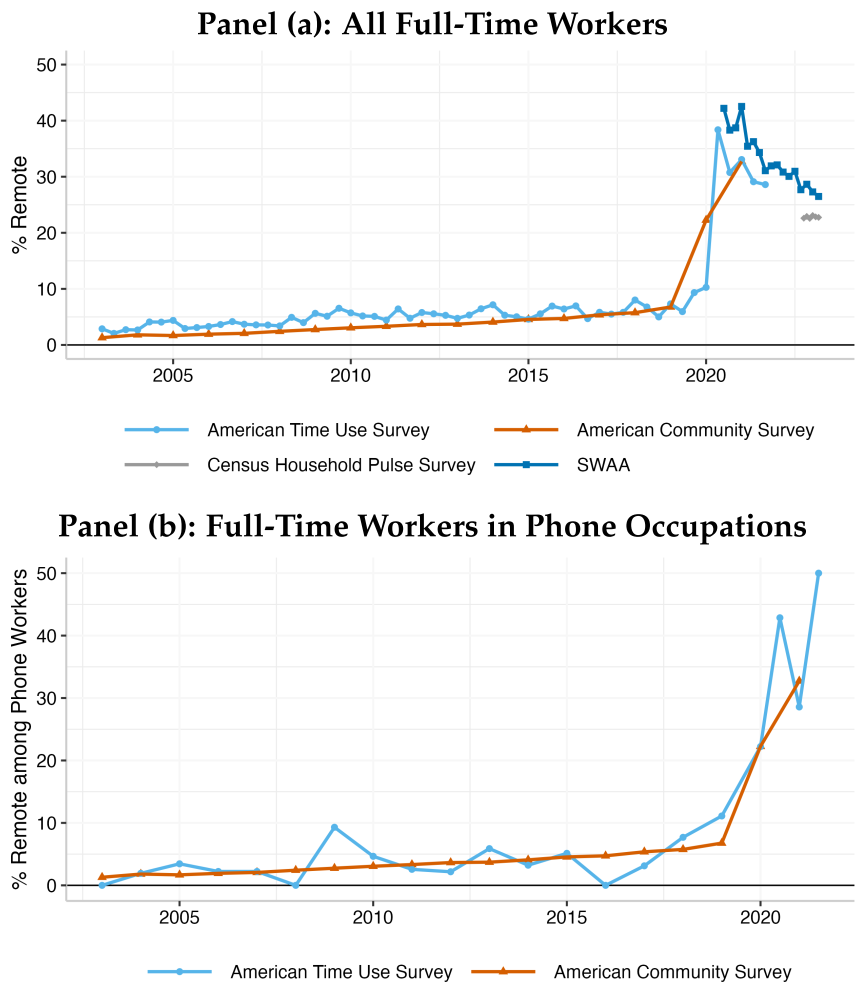
Note: This figure investigates remote work's impact on workers' careers. Each panel repeats Figure 1. Panel (a) captures time spent per month on training for new skills, and Panel (b) captures time spent attending one-on-one meetings with managers. Panel (c) presents the share of workers who are promoted to higher stakes, customer-service roles each month: these promotions feature pay raises of $2 per hour or 13 percent.

Figure A.16: Pre-pandemic Promotion Differences Conditional on Persisting in the Firm



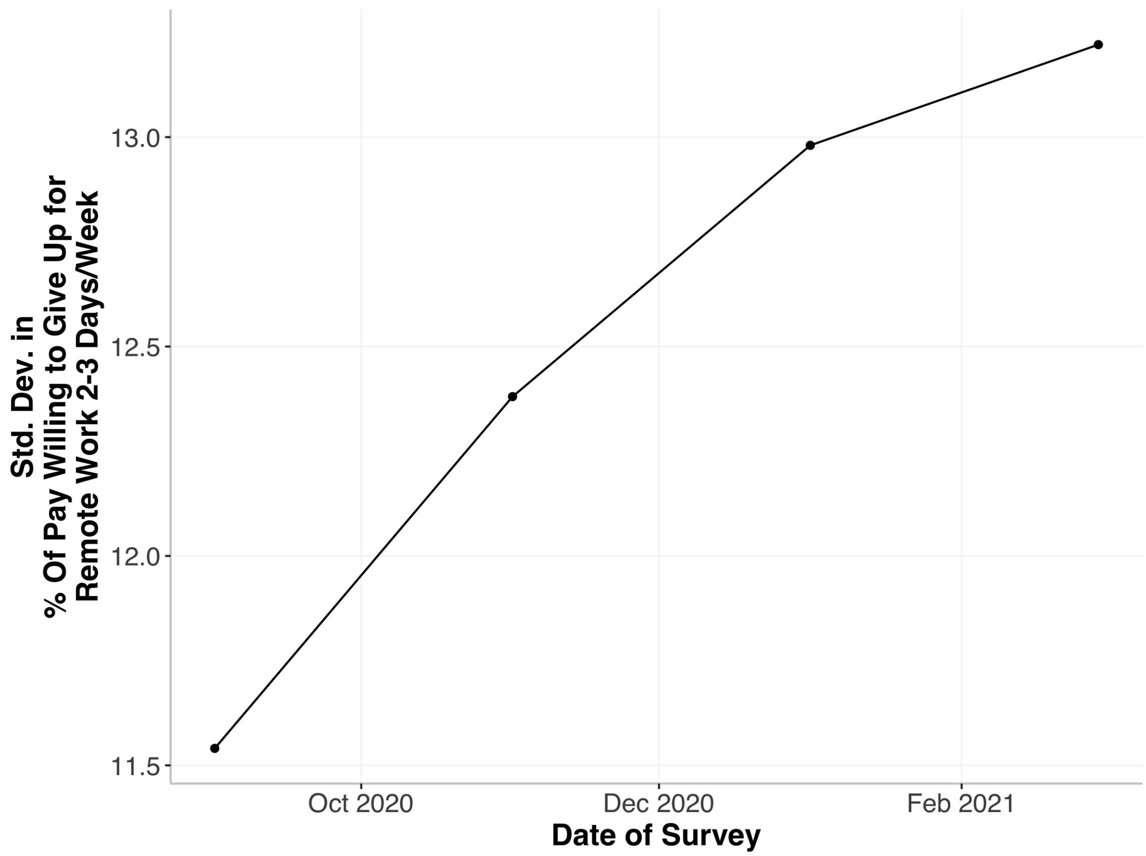
Note: This figure illustrates the differences in promotion rates for on-site and remote workers conditional on persisting in the firm. Each point represents the share of workers who have been promoted as a function of the months since their hire date. The sample is limited to workers hired between July 1, 2018 and March 15, 2020. Standard errors are clustered by worker.

Figure A.17: Trends in Remote Works' Prevalence in the US



Note: This figure illustrates trends in the prevalence of remote work in the US. All samples are limited to employed workers, ages 18−64 who worked at least 35 hours per week. Panel (a) includes all workers. Panel (b) limits to the subset of phone workers, using Mas and Pallais (2017)'s definition of telemarketers (Census code 4940), bill and account collectors (5100), customer service representatives (5240), and interviewers (except eligibility and loan) (5310) in the surveys in which this is possible. In the American Time Use Survey, remote work is defined as doing all of one's work at home, excluding time-diaries taken on weekends and those with less than 7 hours of work (Bureau of Labor Statistics, 2022). In the American Community Survey, remote work is defined as responding to questions about transportation to work with the possible response of working at home (U.S. Census Bureau, 2022). In the Census Household Pulse Survey (U.S. Census Bureau, 2023) and in the Survey of Workplace Arrangements and Attitudes (SWAA) (Barrero et al., 2022), remote work is defined as the respondent spending all of their paid workdays at home.

Figure A.18: The Time-Series of the Variation in Workers' Willingness to Pay for Remote Work Over the Course of the Pandemic



Notes: This figure illustrates the time-series change in the variation in workers' stated willingness to pay for remote work over the course of the pandemic, using surveys of Barrero et al. (2022). The x-axis plots the date of the survey. The y-axis plots the standard deviation in the percent of workers' pay that they report being willing to give up to have the option to work at home two to three days per week. Specifically, the question asks respondents: "how much of a pay raise/cut would you value WFH 2 to 3 days per week?" In total, 19,166 individuals were asked this question over the survey waves. Weights are used so that the surveyed individuals match the Current Population Survey. For details on the survey design and reweighting, see Barrero et al. (2022).

B Appendix Tables

Table B.1: Pre-pandemic Productivity Differences

	Calls per Hour						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Chose Remote Job	-0.394^{***}	-0.415^{***}	-0.454^{***}	-0.620^{***}	-0.646^{***}	-0.560^{***}	-0.552^{***}
	(0.063)	(0.082)	(0.081)	(0.127)	(0.164)	(0.103)	(0.139)
Base Pay				-0.010	-0.008	-0.029	-0.048
				(0.037)	(0.051)	(0.038)	(0.055)
Local Outside Option Pay in MSA				0.042	0.033		
				(0.027)	(0.033)		
Unemployment Rate in MSA				0.047^{***}	0.093^{***}		
				(0.018)	(0.024)		
Mother					-0.011		-0.047
					(0.087)		(0.106)
Father					-0.023		0.110
					(0.155)		(0.135)
Pre-Mean On-Site	3.80	3.80	3.80	3.80	3.76	3.80	3.89
Chose Remote in %	-10.38%	-10.91%	-11.95%	-16.31%	-17.18%	-14.72%	-14.18%
	(1.66)	(2.15)	(2.14)	(3.34)	(4.35)	(2.70)	(3.57)
Age x Gender FE		✓	✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓	✓
Propensity Weights						✓	✓
# Workers	1936	1936	1936	1936	697	1936	697
# Remote Workers	344	344	344	344	146	344	146
# On-site Workers	1592	1592	1592	1592	551	1592	551
# Days	116273	116273	116273	116273	56150	116273	56150

Note: This table presents the differences in calls taken per hour between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months *before* the offices closed. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day. Each specification estimates Equation 4 in the six months before the offices closed. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Pay for customer service representatives in the worker's metropolitan statistical area (MSA) comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Unemployment information comes from Bureau of Labor Statistics (2021a). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020 and that we supplemented with a survey run in April of 2021. The last two columns reweight observations based on the inverse likelihood that on-site workers would be on-site and remote workers would be remote based on the local pay in customer service in the MSA and the local unemployment rate. The sample is our preferred sample summarised in footnote 22. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.2: Pre-pandemic Productivity Differences Limited to $14 per hour Locations

	Calls per Hour						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Chose Remote Job	-0.424^{***}	-0.469^{***}	-0.542^{***}	-0.711^{***}	-0.744^{***}	-0.646^{***}	-0.658^{***}
	(0.072)	(0.119)	(0.115)	(0.145)	(0.194)	(0.123)	(0.175)
Base Pay				0.038	0.030		
				(0.029)	(0.037)		
Local Outside Option Pay in MSA				0.019	0.053		
				(0.025)	(0.034)		
Unemployment Rate in MSA					-0.076		-0.087
					(0.115)		(0.122)
Mother					0.173		0.345^{**}
					(0.222)		(0.173)
Pre-Mean On-Site	3.83	3.83	3.83	3.83	3.71	3.84	3.93
Chose Remote in %	-11.07%	-12.25%	-14.15%	-18.57%	-20.06%	-16.80%	-16.75%
	(1.88)	(3.12)	(2.99)	(3.79)	(5.22)	(3.20)	(4.45)
Age x Gender FE		✓	✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓	✓
Propensity Weights						✓	✓
# Workers	977	977	977	977	363	977	363
# Remote Workers	344	344	344	344	146	344	146
# On-site Workers	633	633	633	633	217	633	217
# Days	62163	62163	62163	62163	30678	62163	30678

Note: This table replicates Table B.1 for the subsample of on-site locations with base pay of $14 per hour that matches the base pay of remote workers at the firm. Thus, everyone in this sample makes the same wages at entry into the firm. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.3: Adjacent Occupations to Customer Service

Prior Occupation (Code)	% of Customer Service Workers
Customer Service Representatives (5240)	86.42
Receptionists And Information Clerks (5400)	1.59
Bookkeeping, Accounting, And Auditing Clerks (5120)	0.95
Tellers (5160)	0.57
Couriers And Messengers (5510)	0.49
Billing And Posting Clerks And Machine Operators (5110)	0.45
Waiters And Waitresses (4110)	0.43
Retail Salespersons (4760)	0.43
Cashiers (4720)	0.41
Dispatchers (5520)	0.34

Note: This table shows the adjacent occupations to customer service. Data comes from the Current Population Survey for 2018 to 2020 (U.S. Census Bureau, 2021). The table reports the percent of customer service workers who had been in various occupations in the prior year. These percentages are computed using survey weights. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.4: Turnover Around the Office Closures

	Turnover		Fired		All Quits		For Personal Reasons		Other Quits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Initially On-site x Post	0.19	0.03	0.03	−0.01	0.17	0.05	0.16	0.13	0.02	−0.08
	(0.19)	(0.21)	(0.08)	(0.09)	(0.17)	(0.20)	(0.13)	(0.15)	(0.11)	(0.13)
Initially On-site	0.27**	0.35**	−0.001	0.04	0.26**	0.31**	0.06	0.05	0.18**	0.26***
	(0.13)	(0.15)	(0.05)	(0.06)	(0.12)	(0.14)	(0.09)	(0.10)	(0.08)	(0.10)
Post	0.03		0.05		−0.01		0.02		−0.04	
	(0.16)		(0.07)		(0.14)		(0.12)		(0.09)	
Dependent Mean	1.22	1.22	0.19	0.19	1.02	1.02	0.59	0.59	0.43	0.43
Week x Time-Zone x Call Level		✓		✓		✓		✓		✓
# Workers	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055
# Initially On-site	1,692	1,692	1,692	1,692	1,692	1,692	1,692	1,692	1,692	1,692
# Already Remote	421	421	421	421	421	421	421	421	421	421
# Worker Weeks	72,470	72,470	72,470	72,470	72,470	72,470	72,470	72,470	72,470	72,470
R^2	0.0003	0.02	0.0001	0.02	0.0002	0.01	0.0002	0.01	0.0001	0.01

Note: This table presents a difference-in-differences design that compares the change in turnover of on-site workers who went remote during the Covid-19 office closures to that of remote workers who were already working from home. The dependent variable is weekly turnover: the columns 1−2 include all departures, columns 3−4 include involuntary firings for performance or behavior, columns 5−6 include quits, columns 7−8 include quits for personal reasons (e.g., family move or sickness), and columns 9−10 include quits for other reasons.Each specification estimates Equation 3 in a six month bandwidth, excluding the period from March 15, 2020 when on-site workers were allowed to work from home and April 6, 2020 when the offices closed. The sample is our preferred sample summarised in footnote 22 but includes individuals who never took calls in six months before and after hte office closures. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.5: Difference-in-Differences Around Covid-19 Office Closures without Donut around Closure Period

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	-0.177^{***}	-0.123^{*}	-0.142^{*}	-0.139^{**}	-0.139^{**}	-0.203^{***}
	(0.063)	(0.064)	(0.076)	(0.055)	(0.054)	(0.068)
Initially On-Site	0.394^{***}	0.450^{***}	0.454^{***}			
	(0.063)	(0.065)	(0.081)			
Post	0.624^{***}					
	(0.057)					
County Covid Cases/10K					0.018	0.013
					(0.015)	(0.018)
County Covid Deaths/100K					-0.020	-0.052
					(0.053)	(0.065)
Mother x Post						-0.036
						(0.057)
Father x Post						-0.119
						(0.120)
Pre Dependent Mean On-Site	3.80	3.80	3.80	3.80	3.80	3.80
Initially On-Site x Post in %	-4.65%	-3.24%	-3.74%	-3.67%	-3.65%	-5.22%
	(1.65)	(1.69)	(1.99)	(1.44)	(1.43)	(1.74)
Age x Gender x Post FE		✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓
Worker FE				✓	✓	✓
# Workers	1,965	1,965	1,965	1,965	1,965	712
# Initially On-site	1,621	1,621	1,621	1,621	1,621	566
# Already Remote	344	344	344	344	146	
# Worker Days	242,365	242,365	242,365	242,365	242,365	136,493

Note: This table presents a difference-in-differences design that compares the change in productivity of on-site workers who went remote during the Covid-19 office closures to that of remote workers who were already working from home. The dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. Each specification estimates Equation 3 in a six month bandwidth. The post period is defined as starting on March 15, 2020 when on-site workers were allowed to work from home. The queue fixed effects specify the date, time-zone, and call-type (see Section II.C). Covid-19 cases and deaths in columns 4 and 5 come from NYT (2021). Parenting characteristics in column 5 come from a caregiving survey that the firm fielded in June of 2020 and that we supplemented in April of 2021. The sample is our preferred sample summarised in footnote 22. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.6: Difference-in-Difference Around Covid-19 Office Closures in Locations with $14/hour Pay

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	-0.16^{**}	-0.11	-0.19	-0.23^{***}	-0.23^{***}	-0.19
	(0.08)	(0.08)	(0.12)	(0.08)	(0.08)	(0.12)
Initially On-Site	0.42^{***}	0.46^{***}	0.54^{***}			
	(0.07)	(0.08)	(0.11)			
Post	0.79^{***}					
	(0.06)					
County Covid Cases/10K					0.01	0.01
					(0.02)	(0.02)
Mother x Post						-0.04
						(0.10)
Father x Post						0.07
						(0.21)
Pre Dependent Mean On-Site	3.83	3.83	3.83	3.83	3.83	3.83
Initially On-Site x Post in %	-4.3%	-2.9%	-4.8%	-5.9%	-6%	-4.7%
	(2.00)	(2.10)	(3.00)	(2.20)	(2.20)	(2.90)
Age x Gender x Post FE		✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓
Worker FE				✓	✓	✓
# Workers	994	994	994	994	994	373
# Initially On-site	650	650	650	650	650	227
# Already Remote	344	344	344	344	344	146
# Worker Days	113,864	113,864	113,864	113,864	113,864	55,263
R^2	0.06	0.11	0.21	0.48	0.48	0.50

Note: This table replicates Table 2 but limits to on-site locations with $14 per hour base pay: in this sample, all workers have the same base pay upon entry into the firm. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.7: Difference-in-Difference Around Covid-19 Office Closures with Schedule Controls

	Calls per Hour				
	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-0.16^{**}	-0.15^{**}	-0.14^{**}	-0.15^{**}	-0.14^{**}
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Initially On-Site x Post in %	-4.1%	-4.0%	-3.7%	-3.8%	-3.6%
	(1.8)	(1.8)	(1.7)	(1.7)	(1.7)
Preferred	✓	✓	✓	✓	✓
Call Min. FE		✓	✓	✓	✓
Email Min. FE			✓	✓	✓
Meeting Min. FE				✓	✓
Other Min. FE					✓
# Workers	1,646	1,646	1,646	1,646	1,646
# Worker Days	172,352	172,352	172,352	172,352	172,352
R^2	0.44	0.45	0.46	0.46	0.46

Note: This table presents a difference-in-difference design that compares the change in productivity of on-site workers who went remote during the Covid-19 office closures to that of remote workers who were already working from home. The dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. Each specification estimates Equation 3 in a six month bandwidth excluding the period from March 15, 2020, when on-site workers could work from home, to April 6, 2020, when remote work was required. The preferred set of controls include worked fixed effects, call-queue fixed effects, and time-varying demographic effects (see Section II.C). Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.8: Difference-in-Differences Around Covid-19 Office Closures with Geographic Controls

	Calls per Hour			
	(1)	(2)	(3)	(4)
Initially On-Site x Post	-0.15^{**}	-0.15^{**}	-0.13^{**}	-0.15^{**}
	(0.06)	(0.06)	(0.06)	(0.06)
Covid-19 Cases/10K		0.02	0.02	0.02
		(0.01)	(0.01)	(0.01)
Covid-19 Deaths/100K		-0.03	-0.03	-0.03
		(0.05)	(0.05)	(0.05)
% In Customer Service			0.37^{***}	0.44^{***}
			(0.13)	(0.17)
% Unemployed				-0.03^{**}
				(0.01)
Initially On-Site x Post in %	-3.9%	-3.9%	-3.9%	-3.4%
	(1.60)	(1.60)	(1.60)	(1.60)
Preferred	✓	✓	✓	✓
# Workers	1,965	1,965	1,965	1,965
# Initially On-site	1,621	1,621	1,621	1,621
# Already Remote	344	344	344	344
# Worker Days	224,447	224,447	224,447	224,447
R^2	0.44	0.44	0.44	0.44

Note: This table presents a difference-in-difference design that compares the change in productivity of on-site workers who went remote during the Covid-19 office closures to that of remote workers who were already working from home. The dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. Each specification estimates Equation 3 in a six month bandwidth in a six month bandwidth excluding the period from March 15, 2020 when on-site workers could work from home to April 6, 2020, when remote work was required. The preferred set of controls include worked fixed effect, age-by-gender-by-post fixed effects to allow for different pandemic shocks for different demographic groups, and call-queue fixed effects that specify the date, time-zone, and call-type (see Section II.C). Covid-19 cases and deaths come from NYT (2021). Controls for the share of employment in customer service in the metropolital statistical area (MSA) comes from Bureau of Labor Statistics (2021b). The unemployment rate in the MSA comes from Bureau of Labor Statistics (2021a). Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.9: Difference-in-differences with Pre-Covid Switchers Control Group

	Calls per Hour			
	(1)	(2)	(3)	(4)
Initially On-Site x Post	-0.22^{***}	-0.21^{***}	-0.13^{*}	-0.16
	(0.07)	(0.07)	(0.07)	(0.10)
Initially On-Site		-0.01	-0.01	-0.0000
		(0.02)	(0.02)	(0.02)
Post				-0.05
				(0.07)
County Covid Cases/10K				0.06
				(0.16)
Pre Dependent Mean On-Site	3.8	3.8	3.8	3.9
Initially On-Site x Post in %	-5.7%	-5.7%	-3.5%	-4.2%
	(1.90)	(1.90)	(2.00)	(2.60)
Age x Gender x Post FE	✓	✓	✓	✓
Call Queue FE	✓	✓	✓	✓
Worker FE	✓	✓	✓	✓
Hired Location x Post FE			✓	✓
# Workers	2,084	2,084	2,084	740
# Initially On-site	1,855	1,855	1,855	645
# Already Remote	229	229	229	95
# Worker Days	237,840	237,840	237,840	110,888
R^2	0.46	0.46	0.46	0.47

Note: This table presents difference-in-differences designs that compare the change in productivity metrics of initially on-site workers who went remote because of the pandemic office closures to workers who voluntarily chose to go remote before the pandemic. We include time varying controls for age and gender, call-queue fixed effects, and worker fixed effects. Column 3 adds in the area the worker was hired and Column 4 the Covid-19 case rate. Standard errors are clustered at the worker level. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.10: Difference-in-Difference Around Covid-19 Office Closures with Subsamples with Complete Metrics

	Calls/Scheduled Hour		% Call Back in 2 Days		Call Without Call Back/Hour	
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	-0.15^{**}	-0.17^{***}	0.35^{*}	0.35^{*}	-0.13^{**}	-0.15^{***}
	(0.06)	(0.06)	(0.20)	(0.19)	(0.05)	(0.05)
Sample: Time-Use	✓		✓		✓	
Sample: Satisfaction		✓		✓		✓
Pre Mean On-Site	3.8	3.9	15.8	15.8	3.2	3.3
Initially On-Site x Post in %	-4%	-4.4%	2.2%	2.2%	-4.1%	-4.5%
	(1.6)	(1.5)	(1.3)	(1.2)	(1.70)	(1.6)
Preferred Controls	✓	✓	✓	✓	✓	✓
# Workers	1,965	1,954	1,965	1,954	1,965	1,954
# Initially On-site	1,621	1,610	1,621	1,610	1,621	1,610
# Already Remote	344	344	344	344	344	344
# Worker Days	216,671	189,285	216,671	189,285	216,671	189,285
R^2	0.45	0.48	0.13	0.17	0.42	0.45

Note: This table presents a difference-in-differences design that compares the change in productivity of on-site workers who went remote during the Covid-19 office closures to that of already-remote workers. The table considers the robustness of the results to using subsamples with complete data on worker time-use in call-time and hold-time in the odd columns and subsamples with complete data on customer satisfaction in the even columns. The first two columns consider calls per hour, the second two consider two-day call-back rates (that indicate initial questions went unanswered), and the last two columns consider calls without call-backs per hour. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.11: Difference-in-Differences By a Continuous Measure of Worker Experience

		Decomposition		Call Quality			
	Calls / Hour	% On Phone	Min. / Call	Hold Min. / Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back / Hour
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Initially On-Site x Post	-0.17^{***}	-2.01^{***}	0.44^{**}	0.14^{***}	0.46^{**}	-0.002	-0.15^{***}
	(0.06)	(0.52)	(0.21)	(0.05)	(0.20)	(0.01)	(0.05)
Tenure (Z-Score) x Initially On-Site x Post	0.09	0.46	-0.49^{**}	-0.18^{***}	-0.40^{**}	-0.003	0.09
	(0.06)	(0.68)	(0.23)	(0.05)	(0.18)	(0.01)	(0.05)
Pre Mean On-Site	3.8	74.3	13.2	1.1	15.9	4.9	3.2
Percentage Effects							
Initially On-Site x Post	-4.4%	-2.7%	3.4%	12.4%	2.9%	-0.05%	-4.54%
	(1.6)	(0.7)	(1.6)	(4.7)	(1.3)	(0.20)	(1.60)
Tenure (Z) x Initially On-Site x Post	2.4%	0.6%	-3.7%	-16%	-2.6%	-0.05%	2.68%
	(1.6)	(0.9)	(1.7)	(4.3)	(1.1)	(0.20)	(1.70)
Preferred Controls	✓	✓	✓	✓	✓	✓	
# Workers	1,936	1,936	1,936	1,965	1,965	1,926	1,965
# Worker Days	222,782	215,101	215,101	215,101	222,782	187,877	222,782
R^2	0.44	0.63	0.38	0.18	0.13	0.09	0.42

Note: This table analyzes the heterogeneous effects of remote work by workers' tenure at the firm. Each specification estimates the difference-in-differences design in Equation 3, fully interacted with tenure. In column 1, the dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. The next three columns consider three metrics of call quality: (2) minutes that customers are kept waiting on hold; (3) the rate at which customers call back to the service line within two days, likely with unanswered questions; (4) average customer satisfaction scores on a five-point scale. The final column considers an alternative measure of productivity that considers the number of customer calls that do not lead to a call back that the worker answers each hour. Call-queue fixed effects account for the date, time-zone, and call-level to compare workers handling calls randomly routed from the same queue. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.12: Heterogeneous Effects by Parenting

	Calls Hour	Hold Min. Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back Hour
	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-0.23^{**}	0.13	0.50	0.01	-0.21^{**}
	(0.12)	(0.09)	(0.35)	(0.01)	(0.10)
Parent x Initially On-Site x Post	0.03	0.03	-0.22	0.005	0.04
	(0.15)	(0.12)	(0.43)	(0.01)	(0.13)
Pre Mean On-Site, Parent	3.9	1.0	15.5	4.9	3.3
Pre Mean On-Site, Non-Parent	3.8	1.0	15.7	4.9	3.2
Percentage Effects					
Parent: Initially On-Site x Post	-6%	12.9%	3.2%	0.12%	-6.23%
	(2.9)	(8.4)	(2.2)	(0.30)	(2.90)
Non-Parent: Initially On-Site x Post	-5.2%	16%	1.8%	0.27%	-5.18%
	(2.6)	(9.1)	(1.9)	(0.30)	(2.70)
Parent x Post FE	✓	✓	✓	✓	✓
Worker FE	✓	✓	✓	✓	✓
Age x Gender x Post FE	✓	✓	✓	✓	✓
Call Queue FE	✓	✓	✓	✓	✓
# Workers	840	840	840	838	840
# Initially On-site	678	678	678	676	678
# Already Remote	162	162	162	162	162
# Worker Days	126,603	121,167	126,603	107,687	126,603
R^2	0.45	0.16	0.15	0.11	0.43

Notes: This table presents difference-in-differences designs that compare the change in calls answered of on-site workers who went remote during the Covid-19 office closures to that of already remote workers, interacted with whether the individual is a parent. Parental responsibilities come from a June 2020 survey that we supplemented in April 2021. Each specification estimates Equation 4, with our preferred set of controls for worker fixed effects, demographics (age by gender by post period fixed effects), and call-queue fixed effects (date by time-zone by call-level). Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.13: Heterogeneous Effects by Private Workspace

	Calls / Hour	Hold Min. / Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back / Hour
	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-0.41^{**}	0.44^{***}	1.10^{*}	-0.02	-0.38^{***}
	(0.17)	(0.15)	(0.62)	(0.02)	(0.14)
No Private Workspace x Initially On-Site x Post	0.26	0.33	-0.41	-0.04	0.26
	(0.23)	(0.26)	(1.49)	(0.04)	(0.18)
Pre Mean On-Site	3.8	1.0	16.0	4.9	3.2
Worker FE	✓	✓	✓	✓	✓
Age x Gender x Post FE	✓	✓	✓	✓	✓
Call Queue FE	✓	✓	✓	✓	✓
# Workers	235	235	235	234	235
# Initially On-site	195	195	195	194	195
# Already Remote	40	40	40	40	40
# Worker Days	37,833	36,061	37,833	32,439	37,833
R^2	0.50	0.30	0.22	0.19	0.48

Notes: This table presents difference-in-differences designs that compare the change in calls answered of on-site workers who went remote during the Covid-19 office closures to that of already remote workers, interacted with whether the individual had a private workspace. Information on workspaces come from a survey that we conducted of workers in April 2021. Respondents were asked where they had typically worked in the previous week. We define a private workspace as an office or bedroom as opposed to a living room or kitchen. Each specification estimates Equation 4, with our preferred set of controls for worker fixed effects, demographics (age by gender by post period fixed effects), and call-queue fixed effects (date by time-zone by call-level). Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.14: Difference-in-Differences By Gender

	Calls / Hour	Hold Min. / Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back / Hour
	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-0.14^{**}	0.13^{**}	0.37^{*}	-0.001	-0.12^{**}
	(0.06)	(0.06)	(0.21)	(0.01)	(0.06)
Male x Initially On-Site x Post	-0.08	-0.10	0.24	-0.01	-0.08
	(0.17)	(0.19)	(0.62)	(0.02)	(0.15)
Pre Mean On-Site, Female	3.8	1.1	15.9	4.9	3.2
Pre Mean On-Site, Male	3.7	1.2	15.7	4.9	3.1
Percentage Effects					
Female: Initially On-Site x Post	-3.5%	12.1%	2.3%	-0.02%	-3.62%
	(1.7)	(5.1)	(1.3)	(0.20)	(1.70)
Male: Initially On-Site x Post	-5.9%	3%	3.9%	-0.15%	-6.2%
	(4.4)	(15.1)	(3.8)	(0.40)	(4.60)
Worker FE	✓	✓	✓	✓	✓
Age x Gender x Post FE	✓	✓	✓	✓	✓
Call Queue FE	✓	✓	✓	✓	✓
# Workers	1,965	1,965	1,965	1,954	1,965
# Worker Days	224,447	216,671	224,447	189,285	224,447
R^2	0.44	0.18	0.13	0.09	0.42

Note: This table analyzes the heterogeneous effects of remote work by workers' self-reported gender. Each specification estimates the difference-in-differences design in Equation 3, fully interacted with gender. In Column 1, the dependent variable is calls answered per hour that the worker is scheduled to answer customers' calls. The next three columns consider three metrics of call quality: (2) minutes that customers are kept waiting on hold; (3) the rate at which customers call back to the service line within two days, likely with unanswered questions; (4) average customer satisfaction scores on a five-point scale. The final column considers an alternative measure of productivity that considers the number of customer calls that do not lead to a call back that the worker answers each hour. Call-queue fixed effects account for the date, time-zone, and call-level to compare workers handling calls randomly routed from the same queue. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.15: Remote Work and Investments in Workers

Panel (a): New Skill Training Min. Per Month

	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-16.88^{**}	-22.73^{***}	-22.80^{***}	-19.12^{**}	-23.46^{**}
	(8.39)	(8.17)	(8.30)	(8.37)	(9.56)
Initially On-Site	14.35^{***}	23.00^{***}	21.30^{***}	17.84^{***}	19.48^{***}
	(2.64)	(5.85)	(5.90)	(4.86)	(4.84)
Post	27.22^{***}				
	(7.66)				
R2	0.001	0.091	0.09	0.15	0.15
Pre Mean On-Site	72.7	72.7	72.7	72.7	72.7
Percentage Effect					
Initially On-Site x Post	-23.2%	-31.3%	-31.3%	-26.3%	-32.3%
Initially On-Site	19.7%	31.6%	29.3%	24.5%	26.8%

Panel (b): Manager One-on-One Min. Per Month

	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-8.29^{***}	-9.54^{***}	-10.98^{***}	-10.23^{***}	-10.52^{***}
	(2.69)	(2.03)	(2.52)	(1.93)	(2.00)
Initially On-Site	8.63^{***}	7.86^{***}	9.92^{***}	9.20^{***}	9.47^{***}
	(2.20)	(1.52)	(2.09)	(1.76)	(1.82)
Post	-11.73^{***}	4.08^{**}			
	(2.40)	(1.85)			
R2	0.003	0.016	0.13	0.23	0.23
Pre Mean On-Site	30.0	30.0	30.0	30.0	30.0
Percentage Effect					
Initially On-Site x Post	-27.6%	-31.8%	-36.6%	-34.1%	-35.1%
Initially On-Site	28.8%	26.2%	33.1%	30.7%	31.6%
Quartic in Worker Tenure		✓	✓	✓	
Date x Hire Month FE				✓	✓
Call-Queue FE			✓	✓	✓
Age by Gender by Post FE					✓
# Workers	1,965	1,965	1,965	1,965	1,965

Note: This table investigates remote work's impact on workers' careers. Each specification estimates the difference-in-differences design in Equation 3, excluding the period when on-site workers could start working from home on March 15, 2020 and when the offices closed entirely on April 6, 2020. Panel (a) captures time spent per month on training for new skills, and Panel (b) captures time spent attending one-on-one meetings with managers. The sample is the primary sample summarised in footnote 22. Call-queue fixed effects account for the date, time-zone, and call-level to compare workers who handle calls randomly routed from the same queue. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.16: Effect of Remote Work on Promotions

	% Promoted Each Month				
	(1)	(2)	(3)	(4)	(5)
Initially On-Site x Post	-0.63	-1.71	-3.06^{*}	-3.60^{**}	-3.42^{**}
	(1.44)	(1.45)	(1.60)	(1.49)	(1.62)
Initially On-Site	0.38	1.94^{**}	2.00^{**}	2.96^{***}	2.94^{***}
	(0.78)	(0.78)	(0.93)	(0.92)	(0.98)
Post	0.77	-5.53^{***}			
	(1.28)	(1.34)			
Pre Mean On-Site	6.1	6.1	6.1	6.1	6.1
Percentage Effect					
Initially On-Site x Post	-10.2%	-27.8%	-50%	-58.7%	-55.7%
	(23.5)	(23.7)	(26.0)	(24.3)	(26.4)
Initially On-Site	6.2%	31.7%	32.6%	48.3%	48%
	(12.7)	(12.8)	(15.2)	(14.9)	(15.9)
Quartic in Worker Tenure		✓	✓	✓	
Date x Hire Month FE				✓	✓
Call-Queue FE			✓	✓	✓
Age by Gender by Post FE					✓
# Workers	1,746	1,746	1,746	1,746	1,746
# Initially On-Site	1,425	1,425	1,425	1,425	1,425
# Initially Remote	325	325	325	325	325
# Worker Days	278,321	278,321	278,321	278,321	278,321
R^2	0.0000	0.002	0.16	0.28	0.29

Note: This table investigates remote work's impact on workers' promotion rates. Each specification estimates the difference-in-differences design in Equation 3, excluding the period when on-site workers could start working from home on March 15, 2020 and when the offices closed entirely on April 6, 2020. The sample is the primary sample summarised in footnote 22, which is further limited to workers who have either not yet been promoted or just been promoted. Promotions to higher-stakes customer-service roles involve a pay raise of $2 per hour or 13 percent of base pay. Call-queue fixed effects account for the date, time-zone, and call-level. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.17: Productivity Differences When All Workers are Remote Due to Covid-19 in Locations with $14/hour Pay

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Remote Hire	-0.26^{***}	-0.35^{***}	-0.36^{***}	-0.36^{***}	-0.37^{***}	-0.47^{***}
	(0.08)	(0.09)	(0.11)	(0.11)	(0.12)	(0.16)
County Covid Cases/10K				0.005	0.01	-0.01
				(0.03)	(0.03)	(0.03)
Local Outside Option Pay in MSA					0.01	0.004
					(0.03)	(0.04)
Mother						-0.02
						(0.13)
Father						0.36
						(0.26)
Dependent Mean On-Site Hire	4.45	4.45	4.45	4.45	4.45	4.45
Remote Hire in %	-5.9%	-7.8%	-8%	-8%	-8.4%	-10.3%
	(1.8)	(2.0)	(2.4)	(2.4)	(2.7)	(3.5)
Age x Gender FE		✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓
# Workers	714	714	714	714	714	344
# Initially On-site	452	452	452	452	452	207
# Already Remote	262	262	262	262	262	137
# Worker Days	51,701	51,701	51,701	51,701	51,701	29,028
R^2	0.01	0.07	0.16	0.16	0.16	0.22

Notes: This table presents the differences in calls taken per hour between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months after the offices closed in locations with hourly pay of $14/hour. Each specification estimates Equation 4. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., attending meetings or answering customer emails). Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is limited to workers hired between July 2018 and March 15, 2020. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.18: Productivity Differences When All Workers are Remote Due to Covid-19 with Schedule Controls

	Calls per Hour				
	(1)	(2)	(3)	(4)	(5)
Initially Remote	-0.30^{***}	-0.29^{***}	-0.29^{***}	-0.29^{***}	-0.28^{***}
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Initially Remote in %	-7.81%	-7.57%	-7.51%	-7.51%	-7.45%
	(2.10)	(2.20)	(2.20)	(2.20)	(2.19)
Preferred	✓	✓	✓	✓	✓
Call Min. FE		✓	✓	✓	✓
Email Min. FE			✓	✓	✓
Meeting Min. FE					✓
Other Min. FE					
# Workers	1,436	1,436	1,436	1,436	1,436
# Worker Days	108,174	101,019	101,019	101,019	101,019
R^2	0.13	0.15	0.16	0.16	0.17

Notes: This table presents the differences in calls taken per hour between workers who initially chose on-site jobs and those who initially chose remote jobs in the six months after the offices closed. Each specification estimates Equation 4. Controls for minutes scheduled for calls, emails, meetings, and other tasks account for fatigue effects. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., attending meetings or answering customer emails). Our preferred controls include call queue fixed effects that interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. The sample is limited to workers hired between July 2018 and March 15, 2020. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.19: Productivity Differences When All Workers are Remote Due to Covid-19 with Geographic Controls

	Calls per Hour			
	(1)	(2)	(3)	(4)
Remote Hire	-0.30^{***}	-0.31^{***}	-0.26^{***}	-0.18^{*}
	(0.08)	(0.08)	(0.09)	(0.10)
Covid-19 Cases/10K		0.01	0.02	0.02
		(0.02)	(0.02)	(0.02)
Covid-19 Deaths/100K		0.07	0.05	0.10
		(0.09)	(0.09)	(0.09)
% In Customer Service			-0.07	-0.10^{*}
			(0.06)	(0.06)
% Unemployed				-0.03
				(0.02)
Remote Hire in %	-7.81%	-8.1%	-6.83%	-4.86%
	(2.10)	(2.09)	(2.32)	(2.72)
Preferred	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436
# Worker Days	108,174	101,019	101,019	101,019
R^2	0.13	0.13	0.13	0.13

Notes: This table presents the differences in calls taken per hour between workers who initially chose on-site jobs and those who initially chose remote jobs once everyone was remote due to the offices closures of Covid-19. Each specification estimates Equation 4 in the six months after the office closures. Calls per hour is computed as the ratio of the number of completed calls over the number of hours that the worker was scheduled to answer calls that day (as opposed to, e.g., answering customer emails). Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases and deaths come from NYT (2021). The share of employment in customer service representatives in the worker's metropolitan statistical area (MSA) comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Unemployment rates in MSAs come from the Bureau of Labor Statistics (2021a). The sample is our primary sample summarized in footnote 32. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.20: Customer Satisfaction Score Differences When All Workers are Remote Due to Covid-19

	Satisfaction Rating (out of 5)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Remote Hire	0.003	0.007	0.012	0.013	0.012	0.013	0.004
	(0.009)	(0.010)	(0.011)	(0.011)	(0.013)	(0.013)	(0.019)
County Covid Cases/10K				−0.002	−0.002	−0.002	−0.002
				(0.004)	(0.004)	(0.004)	(0.006)
Base Pay					−0.001	−0.0001	−0.003
					(0.005)	(0.005)	(0.007)
Local Outside Option Pay in MSA						−0.001	−0.003
						(0.003)	(0.004)
Mother							0.008
							(0.012)
Father							−0.023
							(0.023)
Dependent Mean Initially On-Site	4.77	4.77	4.77	4.77	4.77	4.77	4.77
Remote Hire in %	0.06%	0.15%	0.26%	0.27%	0.26%	0.28%	0.09%
	(0.20)	(0.21)	(0.24)	(0.24)	(0.28)	(0.28)	(0.40)
Age x Gender x Post FE		✓	✓	✓	✓	✓	
Call Queue FE			✓	✓	✓	✓	✓
# Workers	1,429	1,429	1,429	1,429	1,429	1,429	666
# Initially On-site	1,168	1,168	1,168	1,168	1,168	1,168	529
# Already Remote	261	261	261	261	261	261	137
# Worker Days	89,143	89,143	89,143	89,143	89,143	89,143	49,597
R^2	0.00000	0.003	0.076	0.076	0.076	0.076	0.097

Notes: This table presents the differences in customer satisfaction scores between workers who initially chose on-site jobs and those who initially chose remote jobs once everyone was remote due to the offices closures of Covid-19. Each specification estimates Equation 4 in the six months after the office closures. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is our primary sample summarized in footnote 32. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.21: Hold Time Differences When All Workers are Remote Due to Covid-19

	Hold Min./Call						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Remote Hire	-0.160^{***}	-0.017	-0.025	-0.022	-0.050	-0.041	-0.090
	(0.055)	(0.054)	(0.060)	(0.059)	(0.067)	(0.079)	(0.098)
County Covid Cases/10K				-0.009	-0.009	-0.010	0.005
				(0.018)	(0.018)	(0.019)	(0.023)
Base Pay					-0.027	-0.024	-0.053
					(0.032)	(0.035)	(0.041)
Local Outside Option Pay in MSA						-0.004	0.003
						(0.020)	(0.025)
Mother							0.021
							(0.069)
Father							-0.183
							(0.170)
Dependent Mean Initially On-Site	1.32	1.32	1.32	1.32	1.32	1.32	1.32
Remote Hire in %	-12.1%	-1.26%	-1.89%	-1.66%	-3.75%	-3.1%	-7.67%
	(4.16)	(4.11)	(4.54)	(4.46)	(5.09)	(5.95)	(8.41)
Age x Gender x Post FE		✓	✓	✓	✓	✓	
Call Queue FE			✓	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436	1,436	1,436	666
# Initially On-site	1,174	1,174	1,174	1,174	1,174	1,174	529
# Already Remote	262	262	262	262	262	262	137
# Worker Days	100,414	100,414	100,414	100,414	100,414	100,414	54,959
R^2	0.001	0.041	0.116	0.116	0.116	0.116	0.126

Notes: This table presents the differences in minutes that customers spent on hold between workers who initially chose on-site jobs and those who initially chose remote jobs once everyone was remote due to the offices closures of Covid-19. Each specification estimates Equation 4 in the six months after the office closures. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is our primary sample summarized in footnote 32. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.22: Differences in Call Back Rates When All Workers are Remote Due to Covid-19

	Percent who Call Back in Two Days						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Remote Hire	-0.364^{**}	-0.473^{**}	-0.617^{***}	-0.645^{***}	-0.720^{***}	-0.598^{**}	-0.241
	(0.178)	(0.190)	(0.205)	(0.203)	(0.226)	(0.272)	(0.331)
County Covid Cases/10K				0.101	0.099	0.087	0.013
				(0.063)	(0.063)	(0.063)	(0.077)
Base Pay					-0.074	-0.028	0.027
					(0.089)	(0.099)	(0.124)
Local Outside Option Pay in MSA						-0.065	-0.053
						(0.071)	(0.084)
Mother							0.337
							(0.209)
Father							-0.023
							(0.421)
Dependent Mean Initially On-Site	12.19	12.19	12.19	12.19	12.19	12.19	12.19
Remote Hire in %	-2.99%	-3.88%	-5.06%	-5.29%	-5.91%	-4.9%	-2%
	(1.46)	(1.56)	(1.68)	(1.67)	(1.85)	(2.23)	(2.74)
Age x Gender x Post FE		✓	✓	✓	✓	✓	
Call Queue FE			✓	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436	1,436	1,436	666
# Initially On-site	1,174	1,174	1,174	1,174	1,174	1,174	529
# Already Remote	262	262	262	262	262	262	137
# Worker Days	108,174	108,174	108,174	108,174	108,174	108,174	59,488
R^2	0.0002	0.006	0.083	0.083	0.083	0.083	0.104

Notes: This table considers the percent of calls that result in a callback within two days, which often indicates the initial question went unanswered. The table compares the callback rate of workers who initially chose on-site jobs and those who initially chose remote jobs once everyone was remote due to the offices closures of Covid-19. Each specification estimates Equation 4 in the six months after the office closures. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is our primary sample summarized in footnote 32. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.23: Differences in Call Transfer Rates When All Workers are Remote Due to Covid-19

	Call Transfer Rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Initially Remote	3.68***	4.54***	3.98***	3.91***	2.92***	2.25**	2.32**
	(0.75)	(0.75)	(0.83)	(0.83)	(0.87)	(0.89)	(1.15)
County Covid Cases/10K				0.27	0.24	0.11	0.15
				(0.22)	(0.22)	(0.20)	(0.24)
Base Pay					−0.97***	−0.68*	−1.19**
					(0.35)	(0.39)	(0.47)
Local Outside Option Pay in MSA						−0.24	−0.54**
						(0.22)	(0.25)
Unemployment Rate in MSA						0.54***	0.44**
						(0.16)	(0.21)
Mother							0.78
							(0.79)
Father							1.86
							(1.72)
Pre Dependent Mean On-Site	26.1	26.1	26.1	26.1	26.1	26.1	26.1
Initially Remote in %	14.1%	17.4%	15.3%	15%	11.2%	8.6%	9.1%
	(2.9)	(2.9)	(3.2)	(3.2)	(3.3)	(3.4)	(4.5)
Age x Gender x Post FE		✓	✓	✓	✓	✓	
Call Queue FE			✓	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436	1,436	1,436	666
# Initially On-site	1,174	1,174	1,174	1,174	1,174	1,174	529
# Already Remote	262	262	262	262	262	262	137
# Worker Days	108,174	108,174	108,174	108,174	108,174	108,174	59,488
R^2	0.01	0.04	0.14	0.14	0.14	0.14	0.18

Notes: This table considers the percent of incoming calls that workers transfer to other workers. The table compares the transfer rate of workers who initially chose on-site jobs and those who initially chose remote jobs once everyone was remote due to the offices closures of Covid-19. Each specification estimates Equation 4 in the six months after the office closures. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is our primary sample summarized in footnote 32. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.24: Differences in Calls without Call Backs per Hour When All Workers are Remote Due to Covid-19

	Calls with No Call Back per Hour						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Remote Hire	-0.161^{***}	-0.256^{***}	-0.236^{***}	-0.239^{***}	-0.185^{**}	-0.238^{***}	-0.232^{**}
	(0.062)	(0.063)	(0.069)	(0.069)	(0.078)	(0.089)	(0.114)
County Covid Cases/10K				0.010	0.012	0.017	0.010
				(0.019)	(0.019)	(0.018)	(0.023)
Base Pay					0.052	0.033	0.044
					(0.034)	(0.040)	(0.051)
Local Outside Option Pay in MSA						0.028	0.047
						(0.024)	(0.031)
Mother							0.060
							(0.076)
Father							0.024
							(0.162)
Dependent Mean Initially On-Site	3.86	3.86	3.86	3.86	3.86	3.86	3.86
Remote Hire in %	-4.17%	-6.62%	-6.12%	-6.19%	-4.8%	-6.16%	-5.83%
	(1.60)	(1.63)	(1.80)	(1.79)	(2.02)	(2.31)	(2.87)
Age x Gender x Post FE		✓	✓	✓	✓	✓	
Call Queue FE			✓	✓	✓	✓	✓
# Workers	1,436	1,436	1,436	1,436	1,436	1,436	666
# Initially On-site	1,174	1,174	1,174	1,174	1,174	1,174	529
# Already Remote	262	262	262	262	262	262	137
# Worker Days	108,174	108,174	108,174	108,174	108,174	108,174	59,488
R^2	0.002	0.030	0.127	0.127	0.128	0.128	0.163

Notes: This table considers the number of calls that workers handle per hour, limiting to calls that do not result in a callback within two days. The table compares the number of these calls of workers who initially chose on-site jobs and those who initially chose remote jobs once everyone was remote due to the offices closures of Covid-19. Each specification estimates Equation 4 in the six months after the office closures. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is our primary sample summarized in footnote 32. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.25: Heterogeneity in Differences in Call Rates When All Workers are Remote Due to Covid-19

	Calls/Working Hour			Calls Without Call Back/Working Hour		
	(1)	(2)	(3)	(4)	(5)	(6)
Initially Remote	-0.43^{**}	-0.29^{***}	-0.31^{***}	-0.34^{**}	-0.22^{***}	-0.25^{***}
	(0.18)	(0.09)	(0.08)	(0.16)	(0.08)	(0.07)
Initially Remote x Female	0.14			0.11		
	(0.19)			(0.17)		
Initially Remote x Parent		-0.05			-0.07	
		(0.14)			(0.12)	
Initially Remote x Tenure (Z-Score)			0.04			0.04
			(0.07)			(0.06)
Preferred Controls	✓	✓	✓	✓	✓	✓
Pre Mean On-Site, Control	3.7	3.8	3.8	3.1	3.2	3.2
Pre Mean On-Site, Focal Group	3.8	4.0	3.8	3.2	3.3	3.2
# Control Workers	377	215	511	377	215	511
# Focal Workers	841	325	707	841	325	707
R^2	0.18	0.18	0.18	0.18	0.18	0.18

Notes: This table considers heterogeneity in the differences in productivity between remote and on-site hires by gender identity, parenthood status, tenure before the offices closed for the pandemic. The first three columns consider calls handled per work hour; the next three consider calls per hour that do not result in a call back within two days. Each specification estimates Equation 4 with interactions for the focal characteristic in the six months after the office closures. Call queue fixed effects interact the date of the call with the worker's time-zone and call-level (routine, intermediate, or complex) to limit comparisons to workers handling calls randomly routed from the same queue. Covid-19 cases come from NYT (2021). Pay for customer service representatives in the worker's metropolitan statistical area comes from occupational employment statistics (Bureau of Labor Statistics, 2021b). Parenting characteristics come from a caregiving survey that the firm fielded in June of 2020. The sample is our primary sample summarized in footnote 32. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

C MODEL: MICROFOUNDATIONS

This section microfounds the model of the market for remote work in Section V. We show how fewer career opportunities in remote jobs can lead to adverse selection into remote jobs and result in an under-provision of remote jobs.[42]

In our two-period model, workers choose between remote and on-site jobs. Each job features two possible tasks — one low-skill and one high-skill. Workers vary in their tastes for remote work and their productivities. Firms post menus of jobs. All firms have the same, additive production function and operate in competitive markets.[43]

In period zero, each firm posts a menu of one-period contracts.[44] Each worker chooses a contract after privately learning the probability that she will be a high-performer. During the first period of work, firms learn some workers are high-performers and some are poor-performers, while remaining uncertain about others. Those revealed to be high-performers are promoted, while those revealed to be poor-performers are demoted. Firms are more likely to learn about — or act upon — the productivity of on-site workers than remote workers.

C.I THE FIRM'S PROBLEM

Each firm's production function is as follows. In the low-skill task ($T = L$), a poor-performer ($\Theta_i = L$) produces y, while a high-performer ($\Theta_i = H$) produces

[42]Remote workers could have fewer career opportunities for various reasons. In order to advance, productive workers might need to be noticed, well-connected, or fully tooled. If working on-site makes it easier for productive workers to be noticed, build connections, or pick up new skills, then more productive workers will gravitate on-site. Thus, any of these mechanisms would create adverse selection into remote work.

[43]Our stylized model features two-periods and two rungs of the career ladder. This is a good approximation of our empirical context. Further, the insights are qualitatively similar for an infinite period problem with a continuous choice of what share of time to spend working remotely.

[44]We assume that firms cannot sort workers by varying the bonus for high productivity. This constraint could reflect workers' fairness concerns or risk aversion.

$y + a$ where $a > 0$. When assigned the high-skill task, a high-performer's output increases by A and a poor-performer's output decreases by C. Working remotely changes output by τ, the treatment effect of remote work. The per-period output Y of worker i in job $j \in \{\text{r} \equiv \text{remote}, \text{o} \equiv \text{on-site}\}$ and task T is:

$$Y_{ijT} = y + a \cdot \mathbb{1}[\Theta_i = \text{H}] + \begin{cases} -C & \Theta_i = L,\ T = H \\ A & \Theta_i = H,\ T = H \end{cases} + \tau \cdot \mathbb{1}[j = \text{remote}], \quad (6)$$

where C is assumed to be sufficiently high that the firm only assigns workers the high-skill task when they are known to be high-performers.

Initially, firms do not know individual workers' productivities and can only infer likely productivity from workers' choices to be remote or on-site. Once workers' productivity is revealed, workers are paid their marginal product since we assume that the signals are public and markets are competitive. The average cost of hiring a remote worker instead of an on-site one equals the difference in average products in the first period:

$$\text{AC} = \mathbb{E}_{\text{o}}[Y_{ioL}] - \mathbb{E}_{\text{r}}[Y_{irL}] = -\tau + a(\Pr(\Theta_i = H \mid o) - \Pr(\Theta_i = H \mid r)) \quad (7)$$

The first term reflects the treatment effect of remote work; the second term reflects the self-selection of high-performers into on-site jobs.

C.II The Worker's Problem

Workers vary in their productivities and tastes. Worker i's productivity is either high or low, $\Theta_i \in \{H, L\}$. When choosing her first job, she privately knows her probability, $\theta_i \sim \text{Uniform}[0, 1]$, of being a high-performer. Each worker has an idiosyncratic taste for remote work, $\nu_i = \bar{\nu} + \jmath\epsilon_i$ where $\epsilon_i \sim \mathscr{L}(0, 1)$ is logistic and orthogonal to productivity.[45]

We assume that workers make fixed cost investments in their work arrangement that make switching prohibitively costly in the second period.[46]

Workers choose their job to maximize:

$$U(\theta_i, \nu_i) = \max_{j \in \{\mathrm{r,o}\}} \begin{cases} w_{\mathrm{r}} + (1+\delta)\nu_i + \delta\mathbb{E}[w \mid \theta_i, r] & \text{if remote} \\ w_{\mathrm{o}} + \delta\mathbb{E}[w \mid \theta_i, o] & \text{if on-site} \end{cases}, \tag{8}$$

yielding a threshold rule for choosing remote work of:

$$w_o - w_r \leq \nu_i(1+\delta) + \delta(\mathbb{E}[w \mid \theta_i, r] - \mathbb{E}[w \mid \theta_i, o]). \tag{9}$$

The worker weighs the first-period change in income against her tastes and her likely second-period income, which is discounted according to δ.[47]

When predicting her future income, the worker considers two possibilities. One, with probability, p_j, her productivity is revealed and she earns her marginal product. This is more likely in on-site jobs than remote ones ($p_o > p_r$). Two, with probability, $1 - p_j$, her type remains unknown and her wage remains constant, so:[48]

$$\mathbb{E}[w \mid \theta_i, j] = w_j + p_j(\mathbb{E}[\mathrm{MP}_j \mid \theta_i] - w_j). \tag{10}$$

A worker who privately knows she is likely to be a high-performer (high θ_i) expects her marginal product to exceed the pooled wage ($\mathbb{E}[\mathrm{MP}_j \mid \theta_i] > w_j$). Thus, for her, working remotely is costly because it obscures her productivity. By contrast,

[45] This might reflect, for example, the length of the worker's potential commute or her childcare responsibilities.

[46] Workers might buy a car to commute or build a home office for working remotely.

[47] In reality, the gains from promotion may also include social validation.

[48] The probability p_j is a feature of the job and not of the worker. Thus, nothing can be inferred about productivity if it is not fully revealed.

a worker who privately knows he is likely to be a poor-performer (low θ_i) expects his marginal product to fall short of the pooled wage ($\mathbb{E}[\mathrm{MP}_j \mid \theta_i] < w_j$). Thus, for him, working remotely hides his low-productivity and allows him to pool with more productive types.

Remote work's career consequences reduce the demand for remote work among workers who know they are likely high-performers. This downward shift is the source of the selection problem: at any given wage penalty — or price of remote work — a lower share of workers who are likely high-performers choose remote work.

Workers' idiosyncratic tastes mean their choice to be remote is not fully revealing of their private information about their productivity. Particularly, some workers choose to be remote despite positive signals about their likely productivity because of strong tastes for remote work, while others choose on-site jobs despite negative signals about their likely productivity because of strong tastes for the office. The more variable tastes are (higher $\mathscr{s}$), the more likely these outliers will be and the noisier workers' choices will be as signals of latent productivity.

By contrast, the more career concerns weigh in workers' choices, the rarer these outliers will be and the more informative choices will be about likely productivity. The weight on career concerns depends on the answer to two questions. The first is "how much does choosing a remote job affect the probability of being identified as high- or low-productivity?" The answer is $p_o - p_r$. The second is "how much does it matter to be revealed as high- versus low-productivity?" The answer is $\frac{\delta}{1+\delta}(A + a)$, which reflects (a) the returns to productivity in the low-skill task (a), (b) the productivity increase from assigning a high-productivity worker, a high-skill task (A), and (c) worker's discounting of second period income (δ).

The link between a worker's latent productivity and her demand for remote work is the source of the selection problem. Selection is more acute when career concerns loom large relative to variation in tastes.

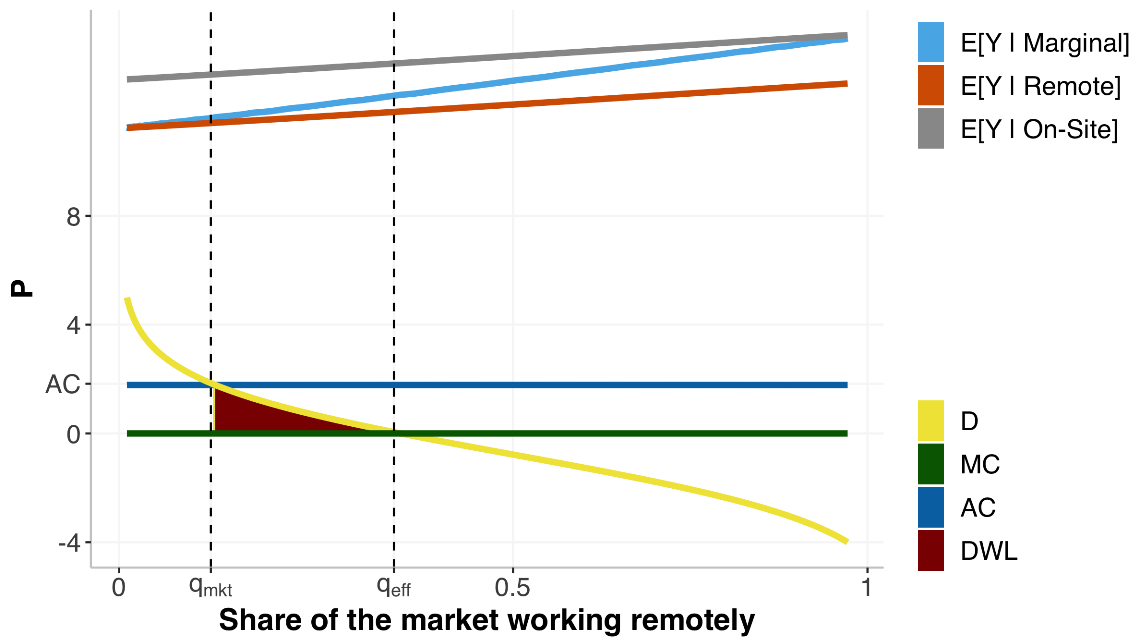
C.III The Market Equilibrium

Figure 5 illustrates the market for remote work. The x-axis plots the share of workers who are working remotely and the y-axis plots the wage penalty — or price of remote work. In equilibrium, the price of remote work equals the average cost of hiring a remote worker instead of an on-site one in the navy line. Even when the marginal cost of switching a given worker from on-site to remote work is zero as pictured in the green line, it can still be costly for a firm to hire a remote worker instead of an on-site one.

Deriving the Average Product in Remote and On-Site Work. The sorting of workers into remote and on-site jobs depends on workers' demand for remote work that reflect both tastes and likely productivity. Consider the pool of workers who choose a remote job even at a high price (e.g., $4/hour in Figure C.1). A worker who is likely to perform well (high θ_i) knows that she is likely to miss a potential promotion and unlikely to avoid a demotion by taking a remote job. Thus, she will only choose a remote job is if she has an extreme taste for remote work. By contrast, a worker who is less likely to perform well knows that he is less likely to miss out on a promotion and more likely to avoid a demotion by taking a remote job. Thus, he requires a less extreme taste to opt into remote work. Since tastes in the tails are less likely, a worker who is likely to do well will be less likely to opt into the remote job than a worker who expects to do poorly. As the price of remote work falls, workers who know they are likely to be high-productivity need less extreme tastes to choose remote work: hence, the share of high-productivity workers on the margins of remote work rises. This causes the marginal product curve — illustrated by the light blue line of Figure C.1 — to have a positive slope.

While the marginal remote work becomes more productive, the gap in the average productivity of remote workers and on-site workers does not change because two margins of selection are changing simultaneously.

Figure C.1: Selection Market for Remote Work



Note: This figure plots the market for remote work under selection into on-site and remote jobs assuming there is no treatment effect of remote work on productivity. The x-axis represents the share of the market working remotely. The y-axis represents the price or wage penalty of remote work. The yellow curve plots the demand curve for remote work or the share of the market that would work remotely at any given price. Since the expected ability of workers on the margin of remote work, $\mathbb{E}[Y \mid \text{Marginal}]$, rises with the share of the market working remotely, the marginal product in remote work, drawn in light blue, is increasing. The average product in the remote job, $\mathbb{E}[Y \mid \text{Remote}]$, drawn in orange, integrates the light blue line from left to right to average over the output of marginal and inframarginal remote workers. The average product in the on-site job, $\mathbb{E}[Y \mid \text{On-Site}]$, drawn in grey, integrates the light blue line from right to left to average over marginal and inframarginal on-site workers. The differences in average product between the on-site workers (in grey) and the remote workers (in orange) produces the average cost, AC, of remote work to the firm in navy blue. This will be the equilibrium price of remote work in the market. The intersection with the demand curve in yellow will determine the equilibrium share of the market working remotely. By contrast, the efficient price of remote work would be zero, which would induce a higher share of the market to work remotely.

The average output of remote workers increases as more workers work remotely. At each point, the pool of remote workers include both marginal workers and inframarginal remote workers, who choose remote work even when the wage penalty is higher. Thus, the average output of remote workers (in orange) integrates the light blue line from left to right (or 0 to q) in Figure C.1. If we approximate the marginal product as $\text{MP}_\text{j}(q) \approx m_0 + m_1 q + \tau \mathbb{1}[j = \text{remote}]$, then:

$$\text{AP}_\text{r}(q) = \mathbb{E}[Y \mid \text{Remote}, q] = \frac{1}{q} \int_0^q m_0 + m_1 q + \tau dq = m_0 + \frac{1}{2} m_1 q + \tau. \tag{11}$$

Since the marginal product is rising, workers on the margin of remote work (in light blue) are always more productive than the average remote worker (in orange). In equations, $\text{AP}_\text{r}(q) - \text{MP}_\text{r}(q) = -\frac{1}{2} m_1 q < 0$. Thus, marginal workers pool with *less* productive workers when they opt into remote work.

At the same time, the average output of on-site workers increases as more workers work remotely and a more selected set of workers work on-site. At each point, the pool of on-site workers includes both marginal workers and inframarginal on-site workers, who only choose remote work when the wage penalty is lower. Thus, the average output of on-site workers (in grey) integrates the light blue line from right to left (or 1 to q) in Figure C.1:

$$\text{AP}_\text{o}(q) = \mathbb{E}[Y \mid \text{On-Site}, q] = \frac{1}{1-q} \int_q^1 m_0 + m_1 q dq = m_0 + \frac{1}{2} m_1 (1 + q). \tag{12}$$

Since the marginal product is rising, those on the margin of on-site work (in light blue) are always *less* productive than the average on-site worker (in grey). In equations, $\text{AP}_\text{o}(q) - \text{MP}_\text{o}(q) = \frac{1}{2} m_1 (1 - q) > 0$. Thus, choosing on-site work means marginal workers pool with *more* productive workers.

In sum, as the wage penalty — or price — of remote work falls, remote jobs become less adversely selected in keeping with classic selection models. At the same time, those who remain on-site become more advantageously selected. Thus, the average product in both remote and on-site jobs rise as the price of remote work falls. As a result, the difference in average products — or the average cost of hiring a remote worker in navy — remains constant at:

$$\mathrm{AC}(q) = \mathrm{AP}_{\mathrm{o}}(q) - \mathrm{AP}_{\mathrm{r}}(q) = \frac{1}{2}m_1 - \tau. \tag{13}$$

where m_1 summarises the link between workers' willingness to work remotely and their productivity: the tighter this link is, the greater the average cost of hiring a remote work instead of an on-site one. Starting from equation 7, this cost can be shown to be:

$$\mathrm{AC} \approx -\tau + a\frac{(p_{\mathrm{o}} - p_{\mathrm{r}})\frac{\delta}{1+\delta}(A+a)}{\varsigma}\mathrm{Var}(\theta_i). \tag{14}$$

Workers' self-selection into jobs based on their private information about their productivity drives a wedge between the marginal and average costs of remote work. The wedge is larger when there are greater returns to high-productivity in the low-skill task (a) and when more workers self-select into jobs based on their latent productivity. Workers self-select more on productivity when they have more private information about productivity, $\mathrm{Var}(\theta_i)$, and when remote work is more determinative of their second-period income. Remote work affects second period income more when (i) there is a greater gap in the probability that productivity is revealed in the two jobs, $p_{\mathrm{o}} - p_{\mathrm{r}}$, and (ii) there is a greater discounted return to being observably high- rather than low-productivity, $\frac{\delta}{1+\delta}(A+a)$. Workers self-select less on productivity when there is more taste variation, ς, which can cause latently high-performers to choose remote jobs and latently poor-performers to choose on-site jobs.

Since the average cost determines the equilibrium price of remote work, the market quantity, q_{mkt}, is found at its intersection with the demand curve in Figure 5.

The market does not arrive at the efficient equilibrium because firms price at the average rather than the marginal cost of remote work, leading to deadweight losses in the red Harberger triangle in Figure 5.[49]

This inefficient equilibrium, however, is not set in stone. Instead, it is a function of the technologies for evaluating remote workers, which determine $p_o - p_r$, and the distribution of tastes for remote work, which determines $\mathcal{s}$.

If firms become better able to evaluate remote workers, then the average cost of remote work will fall towards the marginal cost $\left(\frac{\partial AC}{\partial (p_r - p_o)} < 0\right)$. If firms have learned how to better assess the productivity of remote workers during the pandemic, Covid-19 could lead to a more efficient equilibrium.

If tastes become more variable, the average cost of remote work falls towards the marginal cost $\left(\frac{\partial AC}{\partial \mathcal{s}} < 0\right)$. During Covid-19, tastes may have become more variable as many workers experienced full-time remote work for the first time. By forcing all workers to learn about their tastes, Covid-19 may have pushed the market into a new equilibrium where workers are more certain of their tastes, tastes are more heterogeneous, and choices to be remote are less indicative of low-productivity.[50]

In the model, greater informational frictions in remote work make remote work (i) unattractive for latently high-productivity workers who want their productivity revealed and (ii) attractive for latently low-productivity workers who want their productivity hidden. Thus, the model's central empirical prediction is that remote workers will be adversely selected. Adverse selection leads to the model's central welfare implication that remote work will be under-provided.

[49] In addition, workers' demand for remote work also deviates from the marginal social benefit because the revelation of productivity changes the attribution of credit as well as the assignment of tasks. These private gains lead to excessive sorting by productivity and depress the demand for remote work around the equilibrium quantity. Thus, the Harberger triangle is a conservative estimate of the deadweight losses from asymmetric information.

[50] Covid-19 may have also made remote work more attractive if workers bore fixed costs of setting up home offices or learning new technologies. These changes would increase both the efficient and market quantity of remote work so would not eliminate the market failure.