Working Remotely or Remotely Working?

Selection, Treatment, and the Market for Remote Work

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Abstract: How does remote work affect productivity and how productive are workers who choose remote jobs? We decompose these effects using data from the call-centers of a US Fortune 500 retailer. The retailer employed both remote and on-site workers prior to Covid-19 and randomly routed calls between them. Prior to Covid-19, remote workers answered 12 percent fewer calls per hour than on-site workers. Once everyone was remote due to Covid-19, this gap declined by 4 percentage points, but an 8 percentage point gap remained. Our results suggest that both a negative treatment effect and a negative selection effect reduced remote workers' productivity prior to the pandemic. Difference-in-differences designs also indicate that remote work degraded metrics of call quality and reduced investments in workers' skills. We discuss the implications of our findings for the provision of remote work before the pandemic and the future of remote work in a post-pandemic world.

Keywords: Remote work, work-from-home, worker productivity, selection

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Before the pandemic, less than a fifth of Americans worked remotely.[1] Even in seemingly remotable tasks like call-center work, remote work was the exception rather than the rule before the pandemic.[2] This rarity was surprising since most workers were willing to take pay cuts to work at home (Mas and Pallais, 2017),[3] and working remotely seemed to boost productivity (Bloom et al., 2015).[4] Thus, 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 tasks?

We analyze this question in the American call-centers of a Fortune 500 firm, which hired both remote workers (N=344) and on-site workers (N=1,621) before Covid-19. Pre-pandemic, managers expressed reservations about remote workers' productivity. This intuition was borne out in the data: even when handling calls randomly routed from the same queue, remote workers answered 11 percent fewer calls per hour than on-site workers.

The source of the lower productivity, however, remained unclear. It's possible that in our setting remote work has a negative treatment effect, and any worker would be less productive at home. Working at home may become shirking at home with more modest incentive pay than in Bloom et al. (2015). Additionally, workers may struggle with self-control problems when not in the office. [5]

[1] In the 2019 American Community Survey (ACS), 5.6% of workers reported working from home (U.S. Census Bureau, 2021b). In the American Time-Use Survey between 2013 and 2017, 11.4% reported spending the entire day of the survey working remotely and only 20.5% of workers reported spending even some time working from home (Bureau of Labor Statistics, 2020a).

[2] In the 2019 ACS, 6.3% of phone workers worked at home, using Mas and Pallais (2017)'s occupational definition, and 12.4% of computer programmers did so.

[3] In a real-stakes choice experiment, Mas and Pallais (2017) find that American call-center workers were willing to take an eight percent wage cut to work at home. Maestas et al. (2018) find similar evidence of a substantial willingness to pay for remote work in a large-scale survey. He et al. (2021) find that more applicants apply for jobs that are randomized to be remote on a Chinese job board.

[4] In an experiment in a Chinese call-center, Bloom et al. (2015) find that remote work increased productivity by 13 percent.

[5] Indeed, the potential benefits of monitoring for workers' self control was noted by Kaur et al. (2015).

Yet, it's also possible that less productive workers choose remote jobs — and adverse selection causes unraveling in otherwise remotable jobs. Indeed, even in the Chinese call-center studied by Bloom et al. (2015), remote work unraveled. Working at home halved workers' promotion chances so came to be seen as something only unproductive workers would choose. After the study, stigma sufficiently reduced take-up that the firm discontinued the remote-work program. Beyond this particular firm, widespread concerns about remote work's promotion consequences may influence who chooses remote jobs (Barrero et al., 2022).

The pandemic and ensuing office closures help us differentiate between remote work's impacts on worker productivity and worker selection in our American call-center context.

To illustrate what we can learn from the Covid-19 office closures, consider two hypothetical possibilities. Suppose the productivity gap between workers who chose to be remote and those who chose to be on-site evaporated when on-site workers went remote. In this case, we could conclude that the pre-pandemic gap was due to the productivity effect of remote work. Suppose, instead, the productivity gap persisted (or even widened) after the lockdown. In this case, we could conclude that the pre-pandemic gap was due to differences in worker selection.

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 percentage points relative to that of already remote workers (p-value = 0.017).[6] Yet, even when everyone was remote, workers who had originally chosen to be remote continued to be 7 percent less productive than those who had originally chosen to be on-site (p-value = 0.0002). Together, these results indicate forty percent of the initial productivity gap was due to the

[6]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.

treatment effect of remote work, with the remaining sixty percent due to selection.

However, we find considerable heterogeneity in the treatment effects. Workers who were more junior—and handle easier calls—saw a positive treatment effect from working remotely. In contrast, more senior workers who handle more complex calls, and may benefit more from the ability to consult with colleagues, saw a negative treatment effect of working remotely.

These inferences rely on an identifying 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' demographic characteristics, parental responsibilities, and local geographic characteristics. We also consider two placebo checks on our design. First, we find no similar differential changes in productivity around placebo periods, including the previous holiday rush, which also saw fluctuations in consumer demand. Second, we find that the differences in worker selection narrow for jobs advertised as on-site during the pandemic.

Our results suggest that remote work not only reduced the quantity but also the quality of calls for two out of the three quality metrics. Around the office closures of Covid-19, customer hold-times increased by 11 percent longer for formerly on-site workers going remote relative to already-remote workers (p-value = 0.028). A similar difference-in-differences design indicates that remote work increased customer call-back rates by 3 percent, suggesting that workers were less likely to fully answer customers' initial questions when at home (p-value = 0.045). We further find that remote work reduced the frequency of manager one-on-one meetings and upskilling training sessions, suggesting that remote work may have longer-term consequences for workers' career trajectories and capacity to handle difficult calls.

To understand the market implications of our empirical findings, we embed them in a simple model of the market for remote work.[7] The costs of offering remote work will depend on both its effect on worker productivity and worker selection if firms cannot effectively screen new hires. Remote work's costs must be balanced against the savings in office real-estate and the potential to offer lower wages to remote workers. Using the firm's internal estimates of its office real-estate costs and Mas and Pallais (2017)'s estimates of workers' demand for remote work, we find that nearly all call-center workers would be remote using Bloom et al. (2015)'s estimates, eighty percent of workers would be remote using our estimated 4 percent negative treatment effect, and two thirds of call-center workers would be remote given our estimated 12 percent joint treatment and selection effects.

While our estimates can explain some of the reason why remote work was relatively rare, our predicted provision of remote work still substantially exceeds pre-pandemic levels. This gap suggests some scope for firm misperceptions about remote work that the pandemic could correct. Consistent with this possibility, the retailer permanently closed some but not all of its call-centers during the lockdown. Beyond this particular retailer, about twice as many workers expect to work remotely post-pandemic as worked remotely pre-pandemic (12% in Barrero et al. (2022) vs. 6% in U.S. Census Bureau (2021b)).[8] In addition to correcting firm misperceptions, the pandemic may change who chooses remote jobs as stigma changes and preferences intensify.

Our paper contributes to the nascent (but growing) literature on remote work. We provide new evidence on the treatment effect of remote work in the US context, which contrast with the findings in Bloom et al. (2015)'s randomized control trial in a Chinese travel agency.[9] We complement Atkin et al. (2022)'s contemporaneous

[7]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).

[8]Similarly, the average respondent expects to spend 23.6% of their workdays remote compared to 11.4% before the pandemic (Barrero et al., 2022; Bureau of Labor Statistics, 2020a).

[9]Our setting does not allow us to speak to tasks that hinge on coordination (Battiston et al., 2017;

experiment among Indian workers in six-week data-entry roles, which also finds remote work has a negative treatment effect of remote work.

Our evidence on the selection effect of remote work bolsters the suggestive evidence in Linos (2018)'s analysis of the remote-work program at the US Patent Office.[10] We offer a more direct test of adverse selection using the pandemic lockdown. Our emphasis on the selection effect of remote work suggests new levers that affect the feasibility of remote work in a post-pandemic world, contributing to the literature on the lasting effects of the pandemic on remote work (Bartik et al., 2020; Brynjolfsson et al., 2020; Barrero et al., 2022; Morales-Arilla and Daboín, 2021).

The rest of the paper proceeds as follows. Section I describes our empirical setting. Section II details how we use the pandemic lockdown to separately identify remote work's impacts on worker productivity and worker selection. Sections III and IV present our empirical findings and Section V analyzes their market implications and discusses potential determinants of adverse selection into remote work and the implications for the post-pandemic world. Section VI concludes.

I Data & Setting

Our data include information on the daily call logs and daily schedules of call-center workers at a Fortune 500 firm between January 2019 and October 2021.[11] Personnel data identifies whether workers were hired into remote or on-site jobs, their pay rates, and their job titles.

Gibbs et al., 2021) or intense concentration (Künn et al., 2020), which have also found less positive effects of remote work.

[10]Linos (2018) found remote workers in the US Patent Office were only less productive than on-site workers when they had been hired after the introduction of the office's remote-work program (and thus could have chosen the jobs because of their desire to work remotely).

[11]Previous drafts included data from 2018 as well, but information on workers' schedules is only available starting in 2019.

Time-line of the Retailer's Remote Work Policies. The retailer hired both remote and on-site call-center workers prior to Covid-19 and went entirely remote during the pandemic.[12] On March 15, 2020, the retailer allowed on-site hires to work from home, and on April 6, 2020, the retailer closed down its on-site call-centers. At that point, the retailer employed 1,436 call-center workers — 262 of whom were hired to work remotely and 1,174 of whom were hired to work on-site but now had to work at home.

Routing of Calls. The retailer'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.

Call Logs. The retailer's routing system tracks the number of calls that each worker handled every day. We focus on the number of calls that the worker handled herself, excluding calls transferred to another worker.[13] The retailer'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 retailer tracks workers' daily schedule in fifteen-minute increments. This dataset allows us to observe the total number of minutes that workers were scheduled to answer customers' calls each day. We can then construct our primary outcome measure of calls handled per hour as the number of calls that

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

[13] We use she/her/hers pronouns since 76 percent of workers identify as female in our sample.

the worker handled that day divided by the number of hours that she was scheduled to be on the phone. Crucially, in the denominator, we can exclude time that the worker was scheduled to answer customers' emails, attend meetings, go to training sessions, and do other productive tasks for the firm.

Measuring Call Quality. In addition to tallying calls, the retailer tracks three proxies of call quality. First, the retailer asks customers to rate their satisfaction with each call from one to five stars. Second, the retailer records whether or not customers call back within two days, which often indicates that the initial question went unanswered.[14] Third, the retailer records how long customers waited on hold.

These quality metrics are imperfect. Customers rarely review calls (the participation rate is 11%) and, when they do, they tend to be polite (the mean review is 4.9 out of 5).[15] Given the challenges of monitoring quality, the retailer 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.[16] Further, given the challenges in measuring call quality, being on-site can impact managers' information about workers and the likelihood of promotion to higher-stakes' roles.

Describing the Sample. Table 1 provides summary statistics on our sample. 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

[14] We further show our primary analysis with calls hour defined in terms of calls that do not lead to a callback within two days.

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

[16] 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.

before or after the pandemic closure of the on-site locations in April 2020.

Productivity Differences. Before the pandemic, the retailer's remote workers answered 11% fewer calls per hour in which they were scheduled to answer calls than its on-site workers (row one in columns 2−4 of Table 1). This gap is composed of remote workers handling fewer calls (row two) in the same amount of time scheduled for calls (row three). Remote workers spent less of their time on the phone (row five) and answered each call more slowly (row six). The gap in calls per hour increases to 11% when controlling for the date and time-zone of work and the experience of the worker (see Table B.1). During the Covid-19 lockdown, the productivity gap persisted though it shrank to 5%, even though all workers were remote (columns 5−7). The differences in call quantity were not offset by differences in call quality, which were similar for remote workers (rows seven and eight).

Workers' Localities and Demographics. Data on each worker's home address allows us 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 one dollar more per hour (row nine). This gap in workers' alternatives is similar for adjacent occupations to customer-service — such as bookkeeping and clerical tasks (see Table B.2 for the most common transitions) — so persists in a more general measure of workers' outside options in row ten.[17] Remote workers also tend to live in counties with higher wages higher incidences of Covid-19 (row 13). Adjusting for these geographic differences does not appreciably change the estimated productivity gaps before or during the lockdown (see Tables B.1 and 4).

[17] We characterize adjacent occupations of customer-service workers based on respondents' occupations in the previous year in the Current Population Survey (U.S. Census Bureau, 2021a), which is similar to the methodology in (Schubert et al., 2021). The most common prior occupation is unsurprisingly customer service (86%) followed by receptionists (1.6%) and bookkeepers (1.0%). For each MSA, we then construct an average outside option, where we weight each wage in the Bureau of Labor Statistics (2020b) by these transition probabilities and the share of workers in that alternative occupation in the MSA.

On average, workers had been at the firm about 8 months before the COVID-19 office closures (row 12). The majority of the retailer's call-center workers identify as female (row 13). The average age of these entry-level workers is 35 (row 14). A substantial share of workers report being parents in a June 2020 survey (row 15). Remote workers tend to be a few years older and are more likely to report being female and parents.

II Empirical Framework

The descriptive statistics suggest that remote workers were less productive than on-site workers before the pandemic. This section uses the potential outcomes framework to illustrate how the office closures due to Covid-19 can reveal the sources of this productivity gap and 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, $Y_{i,r} \neq Y_{i,o}$: for example, workers may be more distracted by the family at home (so $Y_{i,r} < Y_{i,o}$) or their 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 in the same job, $\mathbb{E}[Y_{i,j} \,|\, R] \neq \mathbb{E}[Y_{i,j} \,|\, O]$, if, for example, more productive workers are more deterred by remote work's promotion penalties (so $\mathbb{E}[Y_{i,j} \,|\, R] < \mathbb{E}[Y_{i,j} \,|\, O]$).

The productivity difference before the offices closed is given by:

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

The challenge is that we observe different potential outcomes for different sets of workers.[18] Thus, the productivity difference conflates differences in worker selection (R vs. O) with differences in treatment ($Y_{i,r}$ vs. $Y_{i,o}$ for each worker):

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

There are two reasons why remote workers might be less productive than on-site workers when the offices were open. First, the treatment effect of remote work could cause workers to be less productive. If so, on-site workers would be equally unproductive at home ($\mathbb{E}[Y_{i,r} \mid i \in O] - \mathbb{E}[Y_{i,o} \mid i \in O] < 0$). Second, remote work could select for less productive workers. If so, workers who chose remote jobs would be less productive than workers who chose on-site jobs even if all workers were forced to work remotely ($\mathbb{E}[Y_{i,r} \mid i \in R] - \mathbb{E}[Y_{i,r} \mid i \in O] < 0$).

Without a shock to workers' work arrangements, we would never observe the potential outcome of workers who chose to be on-site in remote jobs ($\mathbb{E}[Y_{i,r} \mid i \in O]$). This missing potential outcome would prevent us from disentangling treatment and selection. During the Covid-19 lockdown, this missing potential outcome is revealed.

II.A The Treatment Effect of Remote Work

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

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

the lockdown:

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

When the offices closed, the workers who were already working remotely were not affected by the office closure but were impacted by the pandemic. We can consequently use the already-remote workers as a control group to net out the pandemic's effect in a difference-in-differences design:

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

This design identifies the treatment effect of remote work if workers who originally chose to be on-site face similar pandemic shocks as those who originally chose to be remote. In our call-center context, the control group of already remote workers not only helps us net out pandemic shocks to workers but also to consumers, who may call into the online retailer at different rates (and with different courtesy) during the pandemic. We probe the identifying assumption of similar pandemic shocks to on-site and remote hires in a few ways. First, we probe 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 versus remote hires in placebo periods despite similarly large swings in consumer demand. Third, we do not find any differential trends in productivity between remote and on-site hires leading up to the closures.

II.B Selection Effect of Remote Work

During the lockdown, 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:

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

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 posted wage and local labor-market conditions — must be held as good as constant. We probe these assumptions in a couple of ways. First, we consider robustness to controls described below in Section II.C.

Second, we consider a placebo check that tests whether differences in worker selection persist among workers who are hired when all the offices were closed due to Covid-19. During the pandemic, the retailer continued to advertise on-site jobs that would require workers to return in-person once it was safe to do so, but 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 worker selection dissipate over the course of the pandemic in Section IV.

II.C Estimating Equations

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

$$\begin{aligned}\text{Calls/Hour}_{i,t} = \beta\,\text{Chose to be On-Site}_i \times \text{Post}_t \\ + \psi\,\text{Initially On-Site}_i + \rho\,\text{Post}_t + X'_{i,t}\kappa + \epsilon_{i,t},\end{aligned} \tag{3}$$

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

$$\text{Calls/Hour}_{i,t} = \theta\mathbb{1}[\text{Chose to be Remote}_i] + X'_{i,t}\alpha + u_{i,t} \text{ in the lockdown,} \tag{4}$$

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 $X_{i,t}$ aim to 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 t interacted with the worker's time-zone ℓ and call-type c (routine, standard, or complex). Demographic controls allow workers of different ages and genders to face different pandemic shocks, by interacting gender and age (e.g., a 33-year old woman) with the post-period indicator. When estimating the treatment effect in Equation 3, we also include worker fixed effects in our preferred specification.

We consider robustness to including additional demographic and geographic controls. We include information on 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 reported their parental responsibilities in a June 2020 survey conducted by the retailer. We finally test whether we arrive at similar conclusions in the subsample of workers who all earn $14/hour base wages at the firm.

III 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 volume of calls that worker handle each hour without controls. Initially, there is a sizable gap in productivity between remote and on-site hires, which shrinks 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). We compare the differences between remote and on-site hires to the last period when on-site hires were required to be in the office. The figure shows the same pattern. 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 four of Table 2).

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 retailer'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.

Table 2 shows the results are robust to including a variety of controls, a stability which is marked given the change in the R^2 from 5 percent to 45 percent. The effects of remote work are persistent with similar estimated impacts with a post-period of one to twelve months (Figure A.2). Moreover, in a placebo check, we find no significant effect in a 2-month bandwidth for any month other than the treated ones (Figure A.3), despite similar upticks in customer call volumes during the previous holiday season. The results are robust to including the donut that our main specification excludes because it is unclear where on-site workers are working from (Table **??**). Likewise, estimates are similar if we consider only those call centers that pay their customer service workers $14/hour (Table B.4, Figure A.4). Moreover, the results remain stable and significant if we include controls for the MSA of work (Table B.5) or the particular schedule of the customer service worker (Table B.6) We also do not find evidence of significant differences in pre-trends prior to the office closures.

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 with customers. 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.5(a)). In addition to spending less time on the phone, on-site hires took 1.1 minutes longer to answer customers' questions once they were remote, an increase of 8.2 percent relative to their pre-period mean (p-value = 0.0021, Column 2 of Table 3 and Figure

A.5(b)).[19]

In addition to reducing the quantity of calls, remote work seems to have reduced their quality. Once on-site hires started to work at home, they kept customers waiting on hold for longer as illustrated in Figure 2(a). Our preferred specification indicates that remote work increased customers' hold time by 0.12 minutes per call or 10.6 percent (p-value = 0.028, Column 3 of Table 3). Similarly, when on-site hires transitioned to remote work, they were more likely to have customers call back into the service line within two days, indicating that their initial question likely went unanswered (Figure 2(b)). Our preferred specification indicates that remote work increased customers' call-back rates by 0.4 percentage points or 2.5 percent (p-value = 0.045, Column 4 of Table 3). We do not see significantly differential changes in customer satisfaction scores: while the onset of the pandemic led to poorer reviews in the aggregate (Figure 2(c)), the difference-in-difference design suggests that this was due to the strains of the pandemic (on workers and customers), rather than the effects of remote work. Finally, we consider a composite measure of quality, that captures the calls that do not result in a call back that a worker answers per hour. In this measure, we also see a significant decrease in quality.

We plumb which customer service workers drive the decreases in quality (Table B.7 and quantity of calls (Table B.8). We find no difference along gender dimensions.[20] However, we find a substantive gap based on tenure at the firm (Figure 3). We see an improvement for the most junior workers along all quantity and quality dimensions, indicative of employee learning (Figure A.7). However, the overall decrease

[19] In Bloom et al. (2015)'s experiment in a Chinese travel agency, the productivity advantages of remote work primarily came from workers spending more time on the phone rather than answering calls more quickly although call speeds also became marginally faster.

[20] Using a survey conducted by the firm to establish whether the individual is a caregiver, we cannot discern differences based on caregiver status. However, the results are noisy because of a low response rate to the survey.

in calls per hour and quality is driven exclusively by workers who have been at the firm more than six months at the time of office closures. This is consistent both with more senior workers work on a queue with more difficult questions experiencing an uptick in more challenging calls and differentially responding.[21] In a survey conducted in June 2020 that asked about their challenges in working from home, responses underscored that remote work is more difficult with colleagues "not answering you in chat and managers not being readily available" and "not having neighbors to turn to for assistance."

These results suggest that remote work reduced the quantity and quality of calls in the short- to medium-term. We can further consider suggestive evidence on the longer-term impacts of remote work, by investigating investments in workers' skills and career trajectories.

When the offices were open, on-site hires spent more time in training sessions devoted to developing new skills. When the offices closed, this advantage disappeared. Figure 4(a) illustrates this in the raw data and with our preferred set of controls. Our difference-in-differences estimate suggest that remote work reduced training time by 19.1 minutes per month (p-value = 0.089).

When the offices were open, on-site hires also spent more time each month in one-on-one, "30-60-90" meetings with their managers, charting out the plan for their career trajectory over the next 30, 60, and 90 days. Once the offices closed due to Covid-19, this advantage was lost, but these meetings also became much less common for both on-site and remote hires (Figure 4(b)).

Consistent with remote work undermining investments in workers' skills and trajectories, we see stark promotion differences prior to the pandemic. Figure 4(c)

[21] We compare those who have been at the firm more than six months because this is the point at which they often begin taking calls from more challenging queues. Indeed, we see similar results when we consider how job level may impact these quality measures (Figure A.6.

illustrates the promotion gap, plotting the share of workers who are promoted against the time since they were hired. Focusing on a year since workers were hired, 44.0 percent of on-site hires had been promoted into handling complex or specialized calls compared to just 20.9 percent of remote hires.[22] These cross-sectional promotion differences are similar to those in Bloom et al. (2015)'s randomized control trial, which found that remote work halved workers' promotion chances in a Chinese travel agency.

IV THE SELECTION EFFECT OF REMOTE WORK

During Covid-19's office closures, workers who had originally chosen on-site jobs worked remotely alongside those who had originally chosen remote jobs. Thus, during the office closures, productivity differences no longer capture the treatment effect of remote work but still capture differential selection into remote work.

Empirically, the productivity gap between workers who had originally chosen remote jobs and those who had originally chosen on-site ones narrows — but does not close — once everyone is at home due to Covid-19. Figure 1 illustrates this, showing that workers who had originally chosen on-site jobs are still systematically more productive than workers who had originally chosen remote jobs even once all workers are working under the same conditions.

Workers who had originally chosen to be remote averaged 0.3 fewer calls per hour (7.8 percent) than those who had originally chosen to be on-site (p-value $= 0.00004$ in Column 3 of Table 4), even when comparing observationally similar workers, who are of the same age, gender, and on the same call queue. The differences

[22]This figure plots unconditional promotion rates. If we instead condition on persisting in the firm, promotion rates start to approach one hundred percent for those who persist in the firm. As a result, the remote workers who persist in the company catch up to the promotion rates of their on-site counterparts by about 15 months at the firm. Figure A.8 illustrates this, showing differences in promotion in the first year followed by convergence.

between workers who chose remote and on-site jobs shows throughout the productivity distribution: during Covid-19's lockdown, workers who had originally chosen remote jobs are over-represented throughout the lower tail of the productivity distribution in Figure 5. Table 4 shows the robustness of the results to the inclusion of controls: the estimates consistent suggest productivity differences on the order of $5-8$ percent despite an increase in the R^2 from 0.002 to 0.17. These results are also robust to limiting only to locations that pay $14/hour (Table B.10), to including controls for workers' schedules (Table B.11), and to including controls for the local geographic context (Table B.12).

Workers who initially chose to be remote answered fewer calls per hour primarily because they took longer to answer each call (Column 2 of Table 5). However, workers who originally chose to be remote kept customers on hold for similar durations and had similar customer ratings as workers who were originally on-site (Column 3 and 5 of Table 5).

Workers who originally chose to be remote are less likely to have customers call back to the service line within two days — potentially because they attempt fewer challenging calls. During Covid-19's lockdown, workers who originally chose to be remote are 0.62 percentage points (or 3.9 percent) less likely to have customers call back to the service line within two days (p-value = in Column 4 of Table 5). Yet, initially remote workers also transfer fully 4.0 percentage points (or 15.3 percent) more calls to other workers (p-value < 0.00001 in Column 3 of Table B.15). These patterns are consistent with workers who initially chose to be remote attaining lower call-back rates because they do not stretch their capacities. If we combine the two measures of worker productivity and consider 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 5.[23]

[23]We show robustness tables for the outcomes included in Table B.13 -B.15

Finally, we explore whether selection differs based on demographic characteristics. We find no meaningful difference based on gender, caregiver status, or tenure (Table B.16).

Location Expectations and Selection. As the pandemic persisted, the productivity gap narrowed between workers who were hired into remote jobs — which would allow them to work at home permanently — and workers who were hired into on-site jobs — which would require them to return to the office once it was sufficiently safe to do so. Figure 6 illustrates this, showing the productivity gap between remote and on-site hires in different cohorts. For all the cohorts hired before the offices closed, workers hired into remote jobs were less productive than workers hired into on-site jobs during Covid-19's lockdown. 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 pandemic persisted and, in fact, intensified in the winter of 2021, the differences in productivity narrowed as the return to the office may have seemed like a distant possibility. Indeed, in the winter of 2021, 61 percent of Americans believed that a return to normal pre-Covid life was at least 6 months away (Ipsos, 2021). These patterns are consistent with differences in worker productivity stemming from differential selection into remote and on-site jobs and are less consistent with productivity differences stemming from geographic differences or differences in compensation that did not change over the pandemic.

V MARKET IMPLICATIONS

We consider the implications of our findings for the market provision of remote work in call-center jobs. In a simple demand and supply framework, the equilibrium provision of remote work is determined by the intersection of workers' demand for remote work and firms' supply of remote work, which in turn is determined by the costs to firms of providing remote jobs.

Workers' demand for remote work depends on their willingness to accept lower wages to work at home: we use estimates of the distribution of willingness to pay from Mas and Pallais (2017)'s real-stakes choice experiment to characterize this demand curve. In a sample of American call-center workers, Mas and Pallais (2017) find that workers were willing to take an eight percent wage cut on average to work at home. This high willingness to pay is corroborated in large-scale surveys in the US (Maestas et al., 2018; **?**) and Poland (Lewandowski et al., 2022) and differential application rates to remote and on-site job posts on a Chinese job board (He et al., 2021).

The supply of remote work depends on the costs of offering remote work to firms. Firms must weigh remote work's productivity effects against the potential savings in office real-estate costs. A back-of-the-envelope calculation suggests that firms like this retailer spend nearly one dollar per hour on office overhead for their on-site workers.[24] Thus, the savings in office real-estate from remote work could easily overwhelm remote work's negative treatment effect — indeed, at an average hourly wage of $15.69/hour, it would cost the firm $0.61/hour to hire enough additional workers to offset the 3.9 percent decline in calls per hour. Based on the treatment effect alone, we would consequently expect workers to be paid a slight 2.2 percent wage premium to work at home, which would lead to fully 84 percent of call-center workers to work at home based on Mas and Pallais (2017)'s estimates.

Yet, remote work's treatment effect is not the only productivity consideration for firms. We make two simplifying assumptions to pin down the remaining elements of firms' costs: first, firms cannot screen for more productive workers for entry-

[24]This amounts to $20/square foot per year in rent and utilities for office space. Typical office space needs run about 100 square feet per worker. For a full time worker, this would be $0.96/hour in office costs.

level jobs like the one that we study, and, second, workers fully internalize the promotion penalty of remote work. Under these two assumptions, the costs of remote work include both the treatment and selection effect of remote work but not the differences in workers' career trajectories in remote and on-site jobs.

Together with the negative treatment effect, negative selection into remote work more than offsets the savings in office real estate. Our estimates suggest that workers would have to take a 6.2 percent wage penalty to work at home. Given Mas and Pallais (2017)'s estimates, 59 percent of workers would be willing to make this sacrifice.

Differences in worker selection can make it costly for individual firms to offer remote work but does not affect remote work's costs to society. From the perspective of an individual firm, attracting latently less productive workers with an offer of remote work is costly. Yet, from the perspective of society, attracting latently less productive workers into remote jobs at a given firm does not impact output, since these workers would also be less productive in on-site jobs at another firm. Thus, the social cost (or benefit) of remote work does not include the selection effect of offering remote work. Our estimates suggest that the selection effect of remote work deters 25 percent of workers from working remotely because these workers do not want to pool with less productive types. The distortion in these workers' decisions leads to a deadweight loss of $343 per year averaged over all workers.[25]

Our model's predictions about the market provision of remote work is far lower than what would be expected given pre-existing evidence about remote work's impacts in call-center jobs. Using Bloom et al. (2015)'s estimated treatment effect

[25]Adverse selection into remote work also depresses the wages of inframarginal remote workers, who choose remote work (1) because of latently low-ability and (2) because of strong tastes, such as due to caregiving responsibilities. Transferring resources to remote workers tagged as low-ability would be more efficient than transferring resources to them through perturbations of the tax system.

and ignoring differential selection would suggest that fully 98 percent of call-center workers would work remotely.

Yet, our model's prediction of 59 percent of the market working at home still substantially exceeds the 6 percent of call-center workers who worked at home prior to the pandemic. Thus, our estimates suggest that call-center firms may have been making mistakes about remote work's costs.

V.A Implications for a "Post"-pandemic World

There are multiple reasons to believe that Covid-19's lockdowns will have a lasting impact on the market provision of remote work.

First, the pandemic could correct firms' misperceptions about the costs of remote work that depressed the supply of remote work.

Second, firms may have invested in management practices and informational technologies that mitigate remote work's negative productivity effects (**?**).

Third, the determinants of who sorts into remote and on-site jobs may have changed over the course of the pandemic. Indeed, workers' idiosyncratic preferences over remote work seem to have intensified, with the variation in workers' stated willingness to pay for remote work steadily increasing in Barrero et al. (2022)'s surveys (Figure A.9). At the same time, individuals report that the stigma associated with remote work has fallen (Barrero et al., 2022), potentially reducing workers' incentives to choose on-site jobs to improve their career opportunities. Both of these forces may cause workers to increasingly sort into remote jobs on the basis of their preferences for working at home rather than their desire to shield or showcase their productivity.

Consistent with these factors, the retailer has chosen to close some but not all of its on-site call-centers over the course of the pandemic.

VI Conclusion

We consider why so few Americans worked remotely prior to Covid-19 even in seemingly 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 (Bloom et al., 2015).

We return to the question of remote work's productivity effects in the call-center context. We use data from an American Fortune 500 firm, which hired both remote and on-site workers prior to Covid-19. Around Covid-19's lockdown, the hourly calls of on-site workers going remote fell by 4 percent relative to that of already remote workers and the quality of their calls also seemed to deteriorate. These findings stand in contrast to the positive productivity effects in Bloom et al. (2015)'s experiment. This divergence suggests that the impact of remote work is context dependent, and, for some firms, remote work reduces productivity even in seemingly remotable tasks.

We further argue that adverse selection offers an additional missing piece to the puzzle of remote work's rarity. In our context, remote workers were half as likely to be promoted as on-site workers, consistent with Bloom et al. (2015)'s RCT evidence. If lower promotion rates deter more productive workers from working remotely, this could lead to adverse selection into remote work. The theoretical prediction of adverse selection is borne out empirically. During the pandemic lockdown, workers who initially chose remote jobs were 7 percent less productive than those who initially chose on-site jobs, even though all workers were working at home.

A back-of-the-envelope calculation suggests that adverse selection distorts the decisions of a quarter of call-center workers, who do not choose to be remote because they do not want to pool with less productive types. There is some promise that the pandemic could nudge us into a more efficient equilbrium. Yet, distortions will likely persist unless career opportunities cannot be equalized.

Our paper has a few important limitations. We identify a negative but small treatment effect of remote work for autonomous tasks but cannot speak to collaborative tasks, where more negative effects have been found (Battiston et al., 2017; Gibbs et al., 2021). Further, 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. 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.

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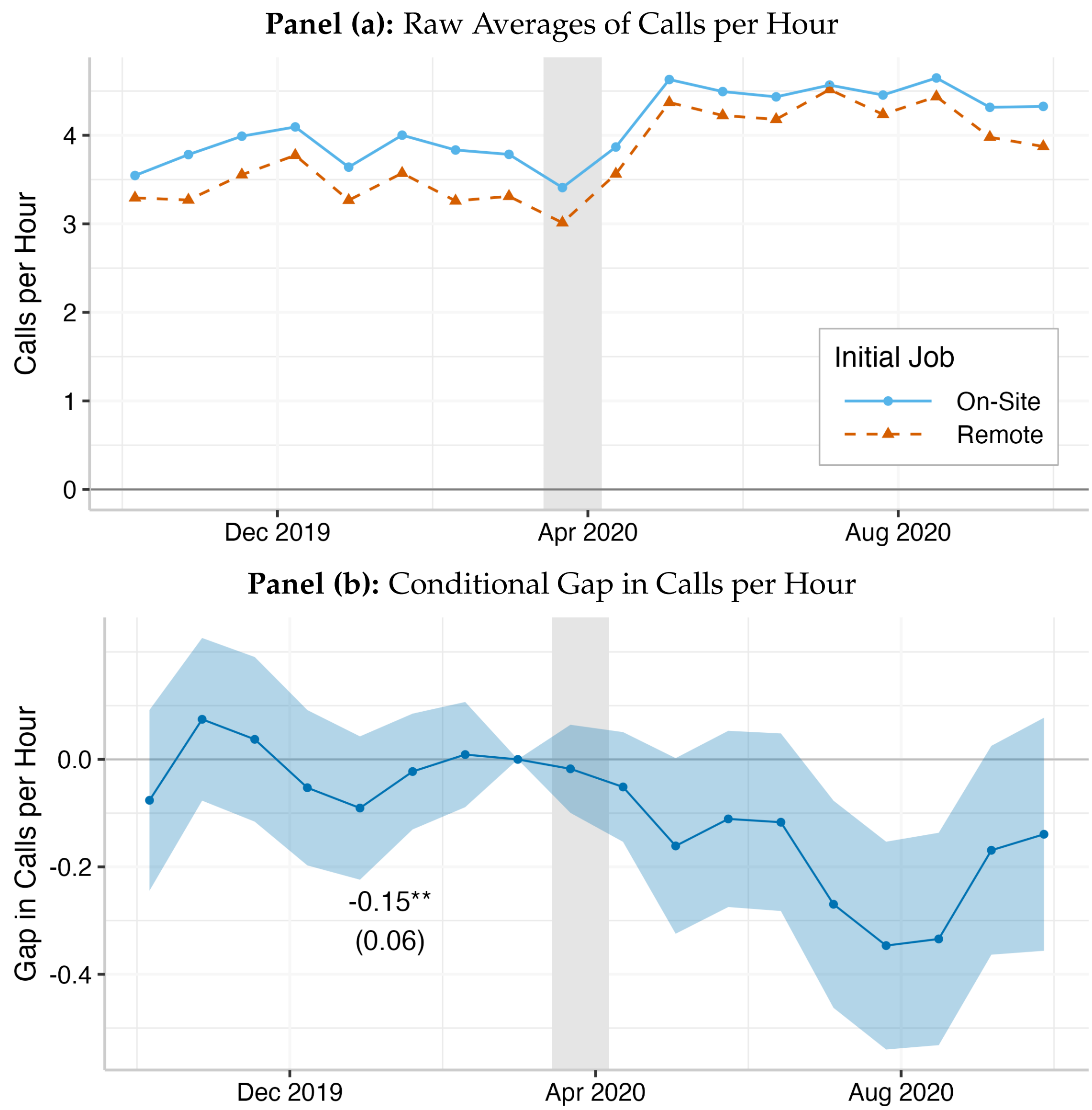
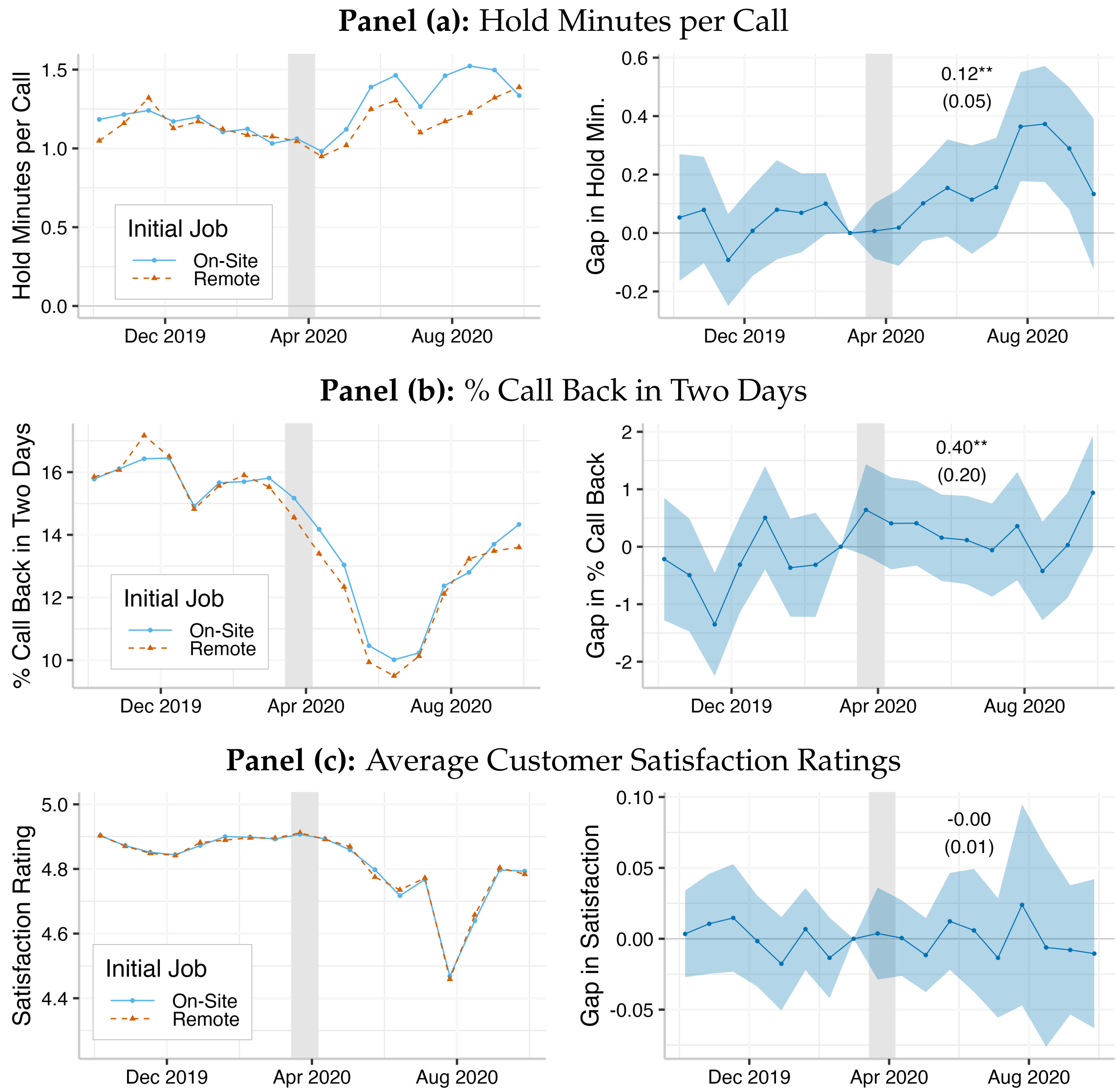
Figure 1: Difference-in-Differences Around Covid-19 Office Closures



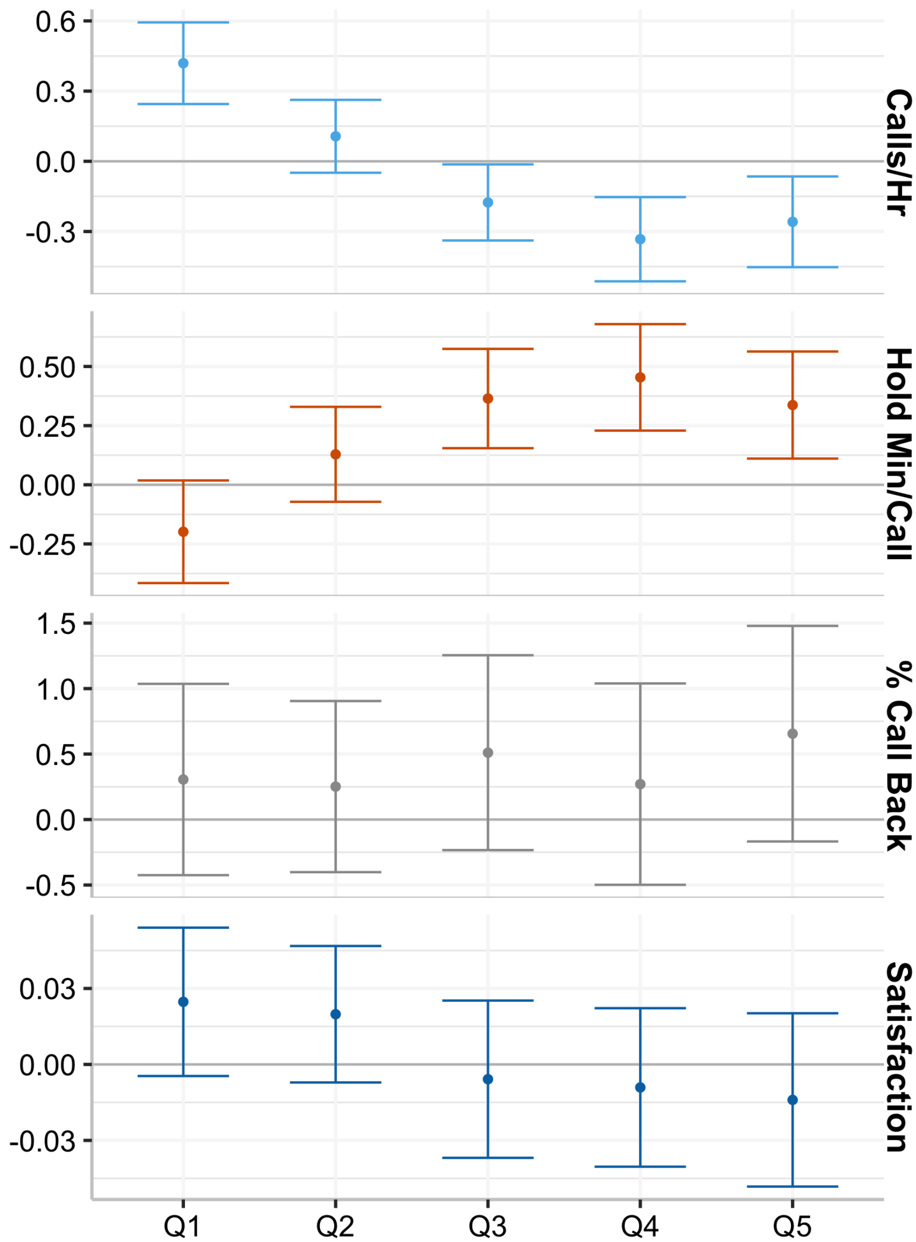
Note: This figure illustrates the difference-in-differences in calls taken per hour between on-site workers who went remote during the Covid-19 office closures (N=1,623) and remote workers who were already working from home (N=344). Panel (a) plots raw three-week averages. Panel (b) plots conditional gaps, using our preferred set of controls for call queues and worker fixed effects (see Section II.C). 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 coefficient indicates the difference-in-differences estimate of the effect of going remote from Equation 3, with a six month bandwidth excluding the shaded period. 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). The sample is limited to workers hired between July 2018 — when the retailer started to hire remote workers — 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.

Figure 2: Difference-in-Differences in Call Quality



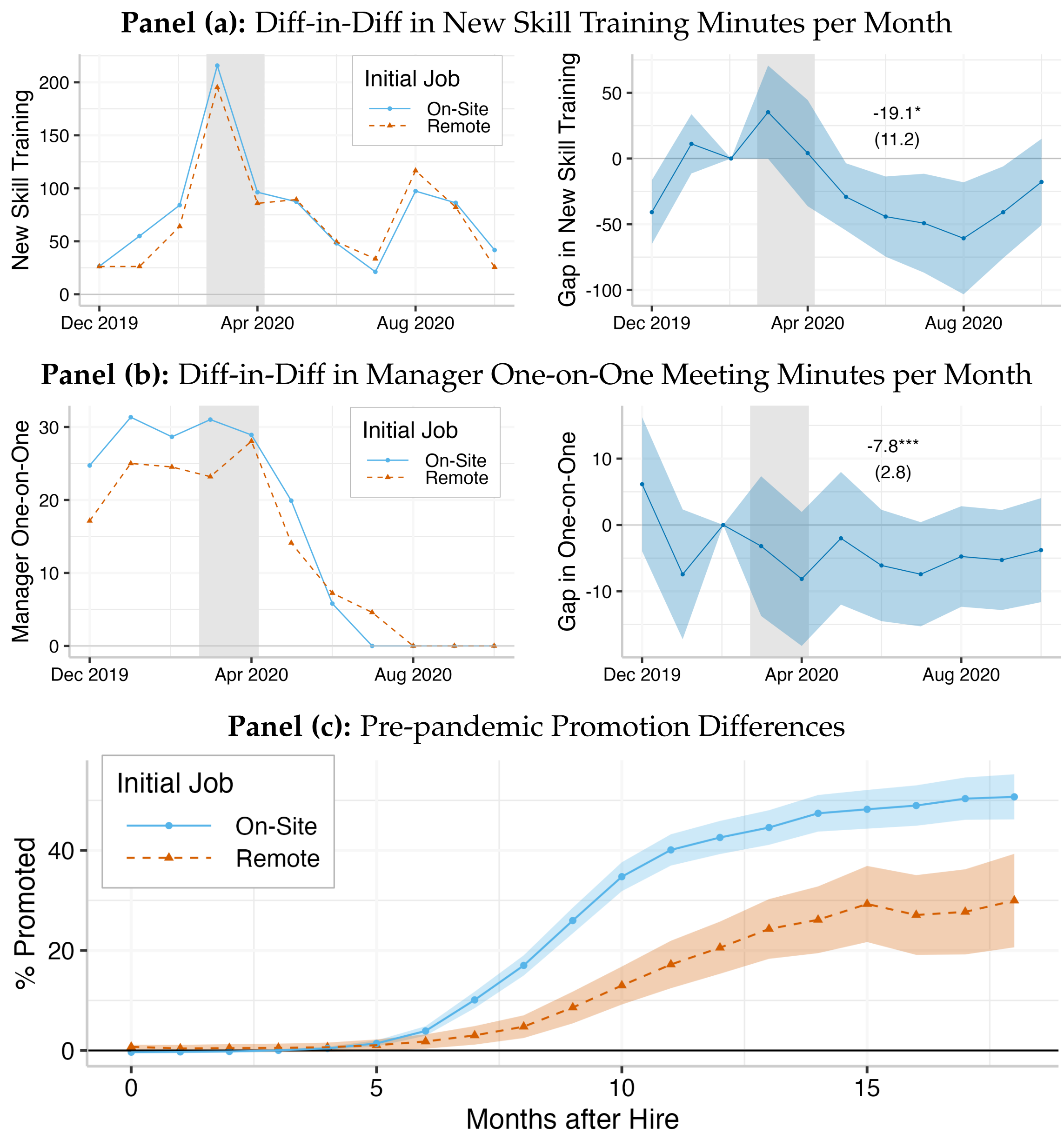
Note: This figure illustrates the difference-in-differences design in call quality between on-site workers who went remote during the Covid-19 office closures (N=1,623) and workers who were already remote (N=344). Panel (a) considers minutes that customers are kept waiting on hold. Panel (b) reports the rate at which customers call back to the service line within two days, likely with initially unanswered questions. Panel (c) presents average customer satisfaction scores on a five-point scale. 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 left plots show raw averages. The right plots include our preferred set of controls for demographics, call-queue fixed effects, and worker fixed effects. The annotated coefficients report the corresponding difference-in-differences estimate of the effect of going remote from Equation 3. These coefficients are also reported in Table 3. The sample is limited to workers hired between July 2018 — when the retailer started to hire remote workers — 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.

Figure 3: Difference-in-Differences Around Closures by Tenure



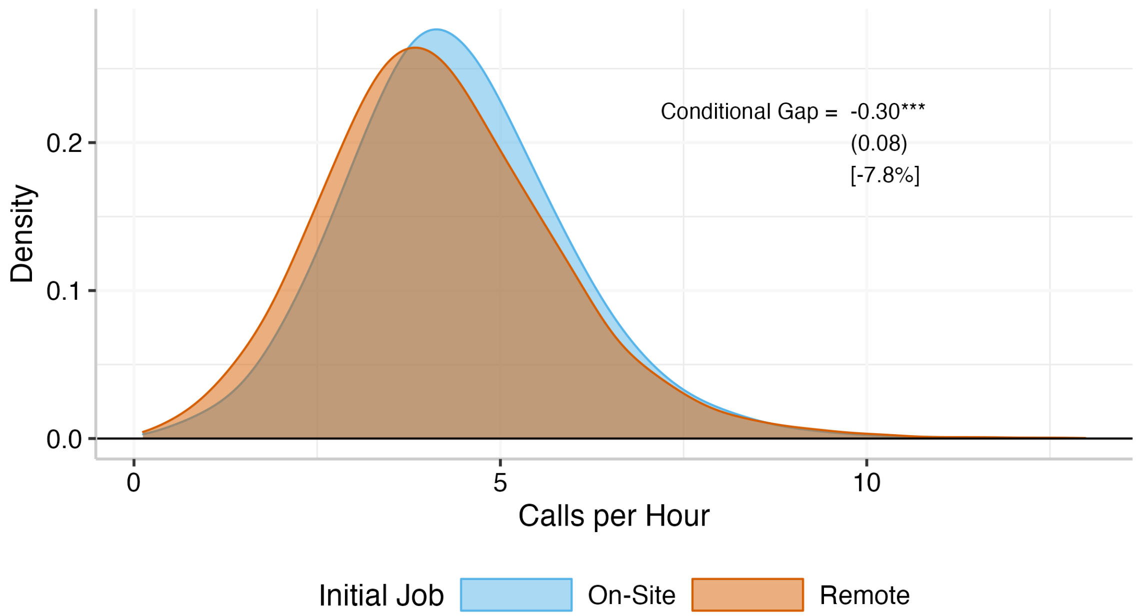
Note: This figure illustrates the difference-in-differences between on-site workers who went remote during the Covid-19 office closures and already-remote workers, based on the quantile of the worker's tenure when the offices closed. All specifications use our preferred set of controls for call queues and worker fixed effects (see Section II.C). There are 426 employees in Q1, with an average of 1 month at the firm just before the office closures; 378 employees in Q2 with an average of 3 months; 388 employees in Q3 with an average of 5 months; 388 employees in Q4 with an average of 9 months; and 385 employees in Q5 with an average of 16 months. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure 4: Effect of Remote Work on Workers' Careers



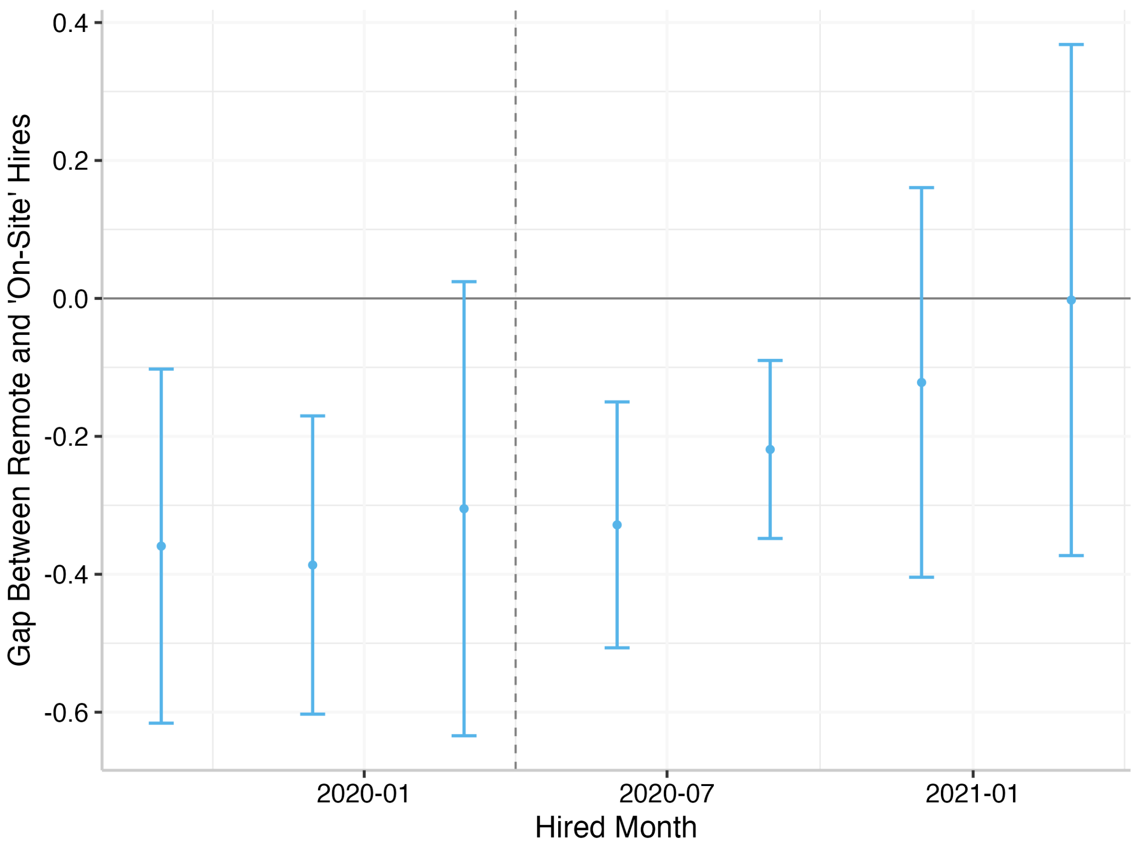
Note: This figure investigates remote work's impact on workers' careers. The top two panels illustrate difference-in-differences designs in time spent per month on (a) training for new skills and (b) attending one-on-one meetings with managers. The left plots show raw monthly averages. The right plots show conditional differences relative to February 2020. The shaded region highlights March and April which were both affected by the office closures. The annotated coefficients indicate the difference-in-differences estimate of remote's impact from Equation 3. Panel (c) 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.8 conditions on persisting in the firm. Samples include workers hired between July 2018 and March 15, 2020. Ribbons 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 5: Productivity Differences When All Workers are Remote Due to Covid-19



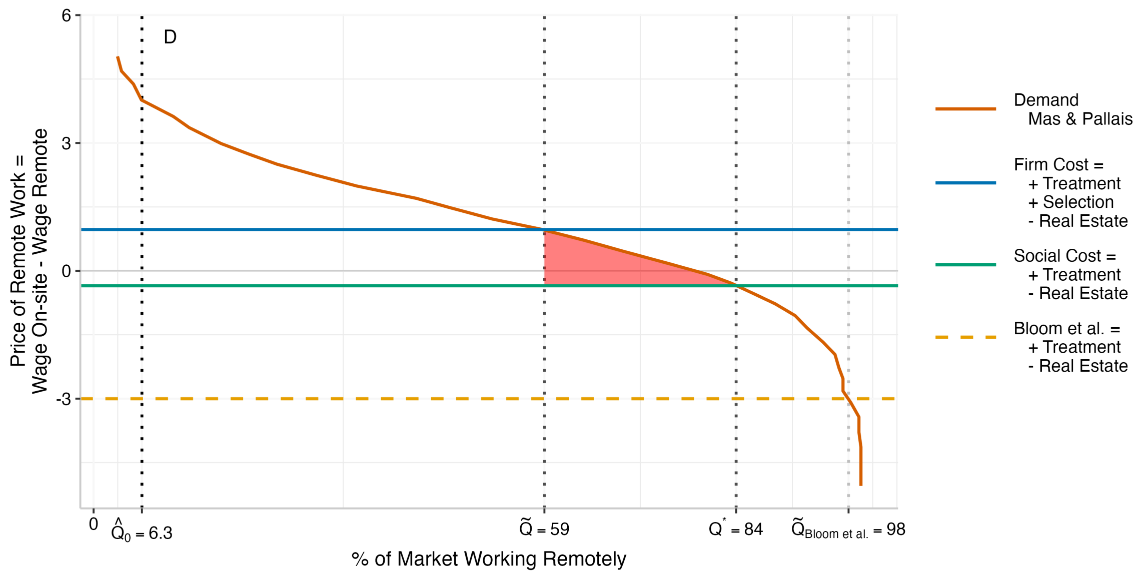
Note: This figure illustrates the differences in calls taken per hour between workers who initially chose on-site jobs (N=1,391) and those who initially chose remote jobs (N=242) in the six months after the offices closed (April 2020 to October 2020). The densities show the distribution of calls taken 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., attending meetings or answering customer emails). 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.

Figure 6: Productivity Differences Between Remote and On-Site Hires Based on 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 to workers who answer calls from English-speaking, American customers. Workers hired into on-site jobs after the offices closed in the vertical line (N = 336) were told that they would eventually need to return to the office but would initially work remotely. On-site workers hired before the offices closed (N = 741) expected to work on-site. 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., attending meetings or answering customer emails). Fixed effects are included for the queue of the call, which depends on the date, time-zone, and complexity of the call (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.

Figure 7: Market for Remote Work



Note: This figure illustrates the implications of our findings for the provision of remote work in call-center jobs in a simple 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 comes from Mas and Pallais (2017)'s real-stakes choice experiment with 608 workers (in red). The estimated private cost of remote work to firms like the retailer (in dark blue) come from our estimates of the treatment and selection effects of remote work net of the savings in office real-estate costs (at \$20/square foot per year × 100 square feet per worker ÷ 2,080 hours per worker-year = \$0.96/worker-hour in rent). The estimated social cost (or, in fact, benefit) of remote work (in green) comes from our estimate of the treatment effect of remote work net of the savings in office real-estate costs. The intersection of demand with the firm cost of remote work determines the market quantity of remote work but the intersection with the social cost determines the efficient quantity. 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.

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)
2	Calls	25.9	26.4	24.9	1.55***	25.9	24.8	1.09**	-0.46
					(0.54)			(0.54)	(0.52)
3	Hours Scheduled for Calls	5.0	5.0	5.1	-0.06	5.1	4.8	0.30***	0.36***
					(0.06)			(0.07)	(0.07)
4	Hours Scheduled for Emails	1.7	1.3	1.2	0.12***	1.9	2.2	-0.31***	-0.43***
					(0.03)			(0.06)	(0.06)
Call Rate Components									
5	% Scheduled Time on Phone	76.8	74.3	71.8	2.53***	79.7	79.4	0.27	-2.26***
					(0.61)			(0.47)	(0.57)
6	Min. Per Call	9.7	9.6	10.0	-0.48**	9.6	9.8	-0.11	0.37**
					(0.20)			(0.16)	(0.15)
Call Quality									
7	Customer Satisfaction	4.8	4.9	4.9	-0.00	4.8	4.8	-0.00	-0.00
					(0.01)			(0.01)	(0.01)
8	% No Call Back	85.9	84.1	84.2	-0.01	87.5	87.9	-0.41**	-0.40**
					(0.19)			(0.17)	(0.19)
Local Traits									
9	Wage	15.0	15.1	14.0	1.14***	15.3	14.0	1.26***	0.12***
					(0.03)			(0.03)	(0.02)
10	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)
11	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									
12	Firm Tenure	248.1	194.1	190.3	3.82	297.5	303.1	-5.62	-9.44
					(9.61)			(12.29)	(7.52)
13	% Female	72.8	70.3	88.2	-17.87***	68.4	88.8	-20.37***	-2.50
					(2.42)			(2.56)	(1.70)
14	Age	34.6	33.5	37.9	-4.48***	34.1	38.3	-4.17***	0.31
					(0.71)			(0.81)	(0.46)
15	% Parent	44.9	42.5	56.9	-14.41***	42.6	53.2	-10.59**	3.83
					(5.24)			(5.06)	(2.91)
16	% Mother	38.0	35.1	54.5	-19.48***	34.1	50.8	-16.64***	2.84
					(5.22)			(5.02)	(2.90)
17	# Workers	1965	1592	344		1218	282		
18	# Caregiving Respondents	712	551	146		530	142		

Notes: This table characterizes the firm's on-site and remote entry-level workers. The sample is limited to remote and on-site workers recruited before March 15, 2020 in locations with uniform pay of $14 per hour. The sample includes workers who took calls from the same queue of English-speaking, American customers with generic questions (e.g., when will my order be delivered?). 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, 2020b). Adjacent occupations are defined as those that are common prior occupations of customer-service workers in the Current Population Survey (see Table B.2 for common transitions). The wages in these occupations are incorporated into workers' outside option wage in each MSA, which weights the wage in each occupation by the transition probabilities and the share of local workers in that occupation in the OES. Data on the unemployment rate in the worker's county comes from the Local Area Unemployment Statistics (Bureau of Labor Statistics, 2021). 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. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 2: Difference-in-Differences Around Covid-19 Lockdown

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	-0.19^{***}	-0.14^{**}	-0.16^{*}	-0.15^{**}	-0.15^{**}	-0.16^{*}
	(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	0.02
					(0.01)	(0.02)
Mother x Post						-0.06
						(0.07)
Father x Post						0.01
						(0.17)
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%	-4%
	(1.80)	(1.80)	(2.20)	(1.60)	(1.60)	(2.10)
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	106,727
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 lockdown 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.3 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 retailer fielded in June of 2020. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 3: Difference-in-Differences: 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 On-Site x Post	-1.99^{***}	1.14^{***}	0.12^{**}	0.40^{**}	-0.002	-0.13^{**}
	(0.54)	(0.37)	(0.05)	(0.20)	(0.01)	(0.05)
Pre Mean On-Site	74.3	14.0	1.1	15.8	4.9	15.9
Initially On-Site x Post in %	-2.7%	8.2%	10.6%	2.5%	-0.03%	-0.8%
	(0.7)	(2.7)	(4.8)	(1.3)	(0.20)	(0.30)
# 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	216,671	216,671	216,671	224,447	189,285	224,447
R^2	0.63	0.21	0.18	0.13	0.09	0.42

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 lockdown to that of already-remote workers. The first two columns decompose the change 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. The final three columns 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; (5) 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 toa call back that the worker answers each hour. Each specification estimates Equation 3 in a six-month bandwidth excluding the period from March 15, 2020 — when remote work became optional for on-site hires — to April 6, 2020 — when remote work became required. We include our preferred set of controls for demographics, call-queue fixed effects, and worker fixed effects. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table 4: 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^{**}	-0.27^{*}
	(0.07)	(0.07)	(0.08)	(0.08)	(0.09)	(0.11)	(0.14)
County Covid Cases/10K				0.01	0.02	0.02	0.01
				(0.02)	(0.02)	(0.02)	(0.03)
Base Pay					0.06	0.04	0.05
					(0.04)	(0.04)	(0.06)
Local Outside Option Pay in MSA						0.03	0.05
						(0.03)	(0.04)
Unemployment Rate in MSA						−0.01	−0.003
						(0.02)	(0.02)
Mother							0.08
							(0.09)
Father							0.03
							(0.18)
Pre 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%	-7%
	(1.9)	(1.9)	(2.1)	(2.1)	(2.4)	(2.9)	(3.6)
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	0.03	0.13	0.13	0.13	0.13	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. 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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 5: Productivity Differences When All Workers are Remote Due to Covid-19: Auxiliary Measures

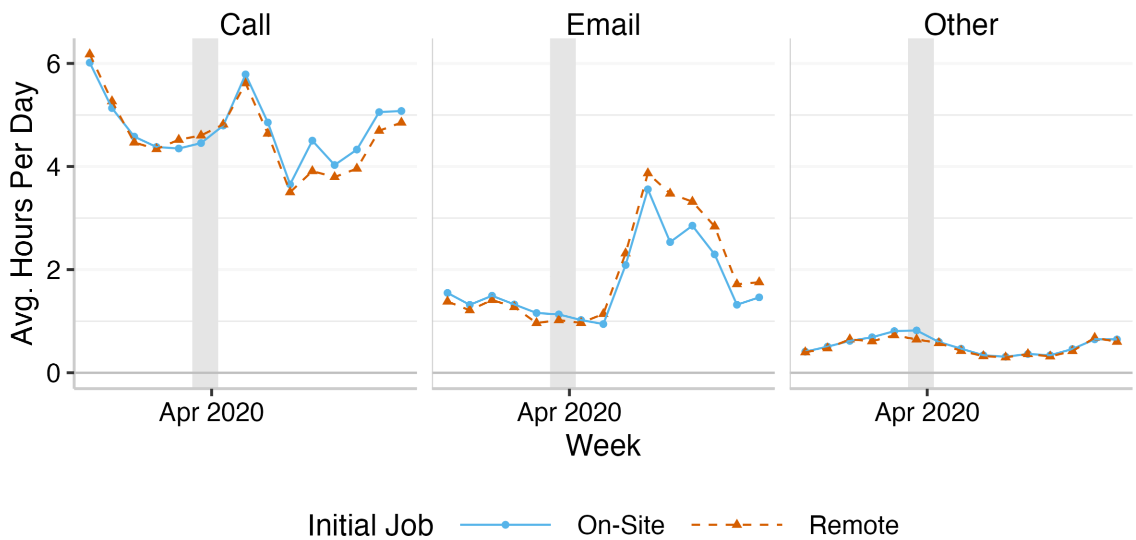
	Decomposition		Call Quality			
	% On Phone	Min. Call	Hold Min. Call	% Call Back (2 Day)	Satisfaction Rating	Call Without Call Back Hour
	(1)	(2)	(3)	(4)	(5)	(6)
Initially Remote	-0.54	1.23^{***}	-0.02	-0.62^{***}	0.01	-0.24^{***}
	(0.50)	(0.31)	(0.06)	(0.20)	(0.01)	(0.07)
Pre Mean On-Site	74.3	14.0	1.1	15.9	4.9	3.2
Initially Remote in %	-0.7%	8.8%	-2.2%	-3.9%	0.25%	-7.4%
	(0.7)	(2.2)	(5.2)	(1.3)	(0.23)	(2.20)
# 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	108,174	100,414	108,174	89,143	108,174
R^2	0.46	0.07	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. The first two columns 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. The next three columns 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. 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. Each specification estimates Equation 4. We include our preferred set of controls for demographics and call-queue fixed effects. 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

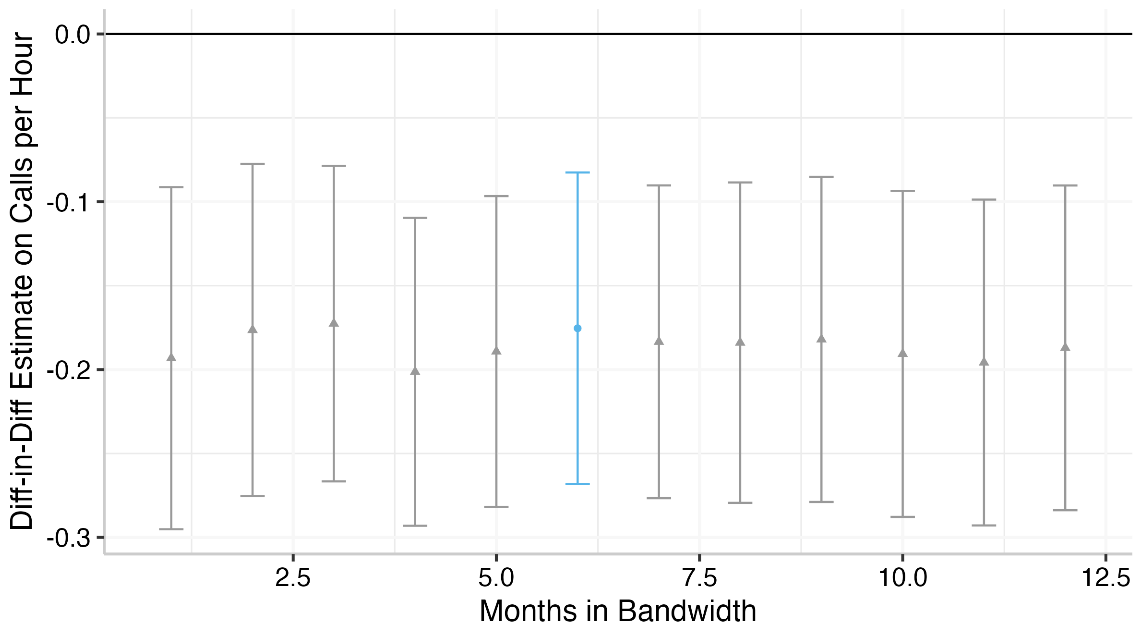
A Appendix Figures

Figure A.1: Scheduled Time Per Day for Initially Remote and On-Site Workers Around the Covid-19 Office Closures



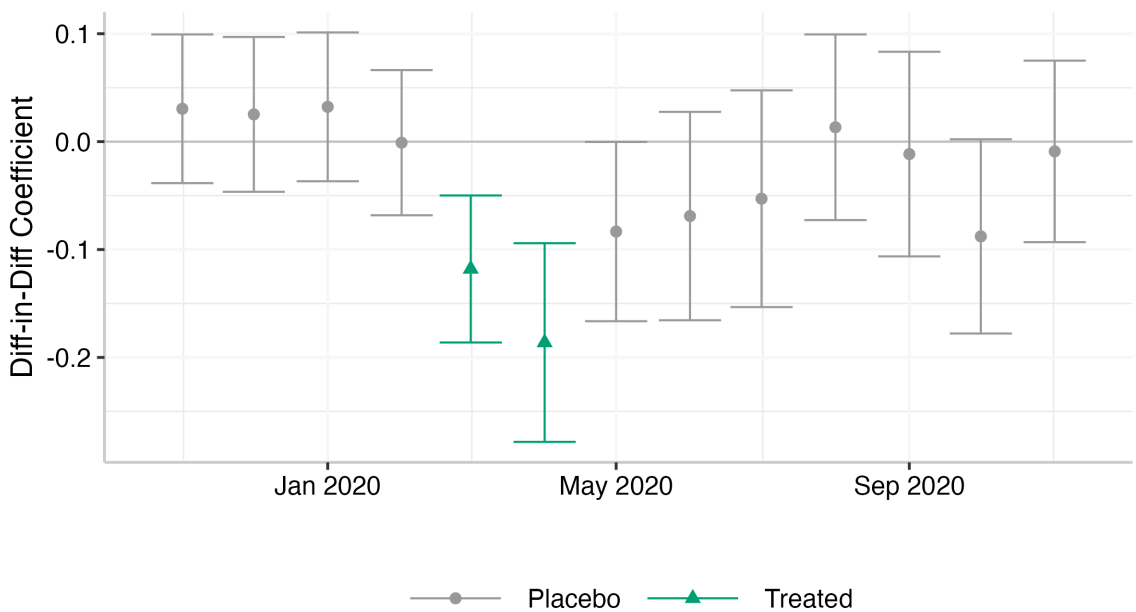
Note: This figure illustrates the changes in the scheduled time of on-site workers who went remote during the Covid-19 office closures (N=1,623) and workers who were already remote (N=344). The left plot shows hours scheduled for answering customer calls. The middle plot shows hours scheduled for answering 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 limited to workers hired between July 2018 — when the retailer started to hire remote workers — and March 15, 2020.

Figure A.2: Robustness of Diff-in-Diff Estimate to Alternative Bandwidths



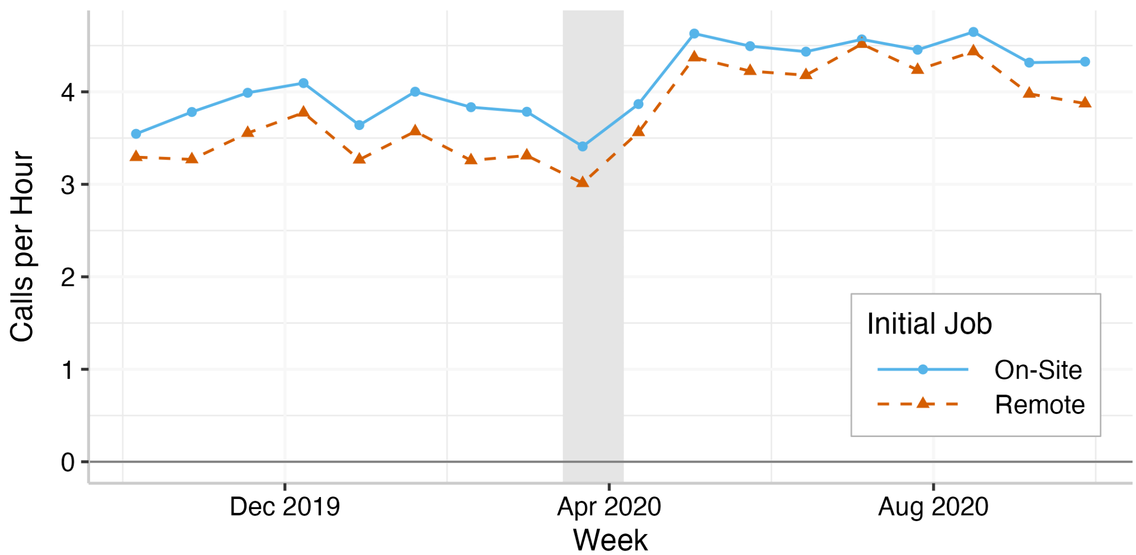
Notes: This figure illustrates difference-in-differences estimates that compare the change in calls per hour for on-site and remote hires around the office closures within various bandwidths. The blue circle shows the estimate with our preferred six-month bandwidth. The grey triangles show estimates 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. The error bars are 95% confidence intervals with standard errors clustered by worker.

Figure A.3: Placebo Treatment Dates' Effects of Proximity on On-the-Job Training from Coworkers



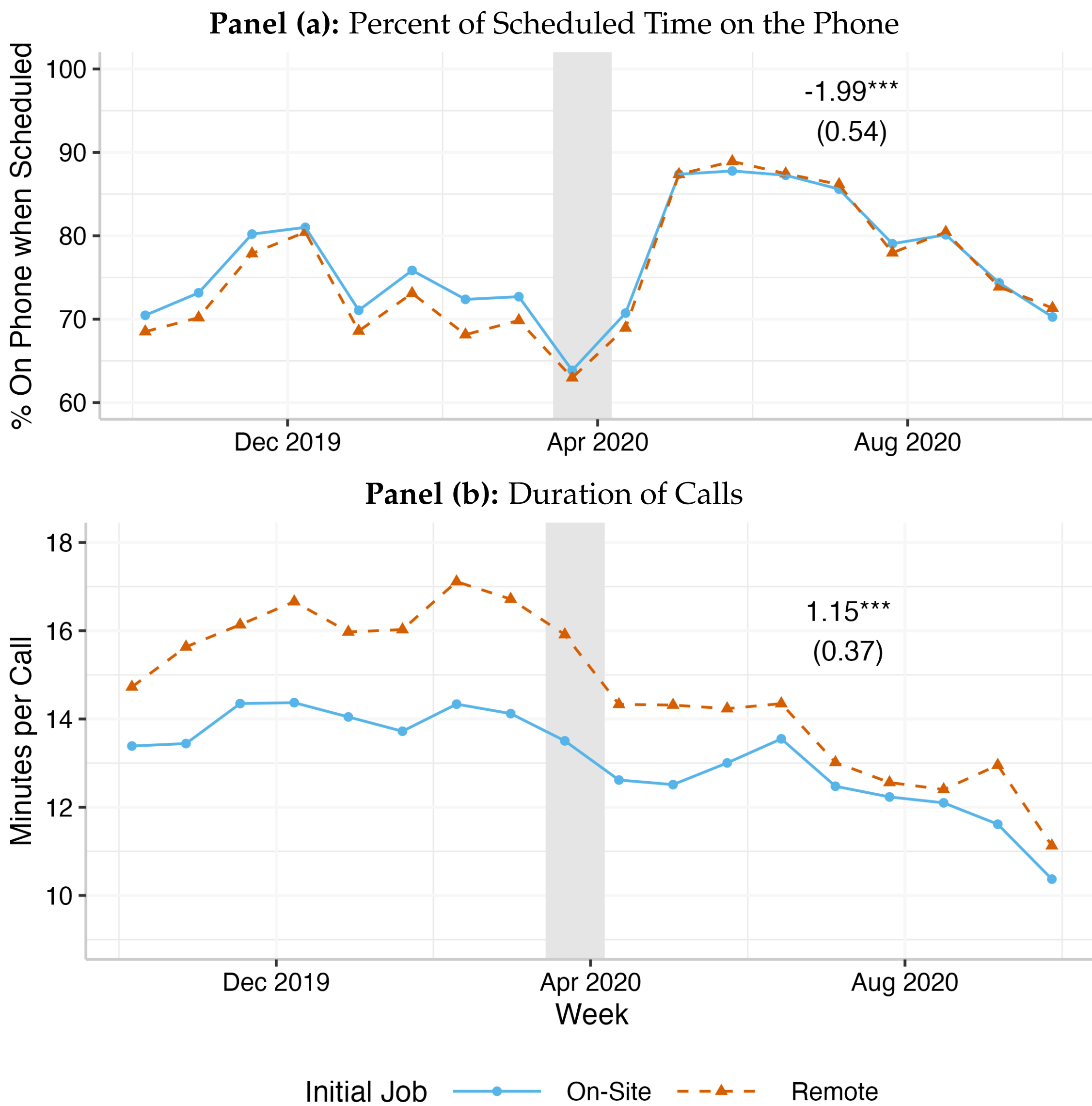
Notes: This figure illustrates difference-in-differences estimates that compare the change in calls per hour for on-site and remote hires within two-month bandwidths. The grey circles show periods that do not include the treated window; the green triangles include the treated window. All regressions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

Figure A.4: Difference-in-Differences around COVID-19 Office Closures in Locations with $14/hour Pay



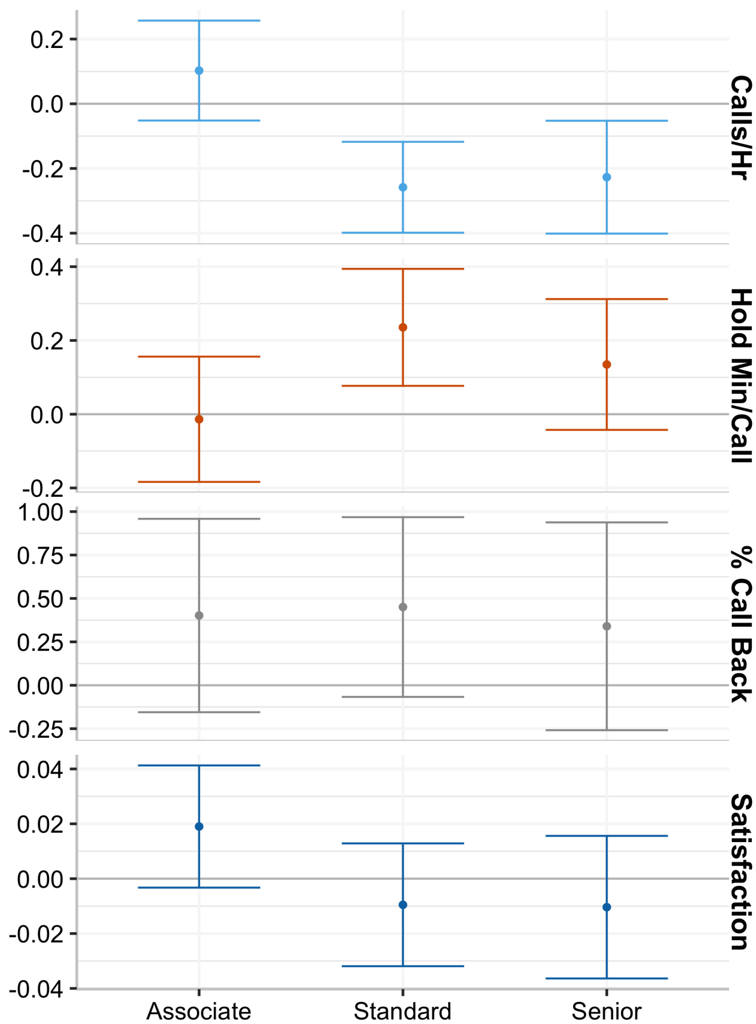
Notes: This figure illustrates difference-in-differences estimates that compare the change in calls per hour for on-site and remote hires, limiting to those locations with hourly pay of $14. We plot three-week averages and the shaded region spans from March 15, 2020 — when on-site workers could start working remotely — to April 6, 2020 — when offices fully closed. Calls per hour is computed as the ration of the number of calls answered over the number of hours that the worker was scheduled to answer calls that day. The sample of workers is limited to workers hired between July 2018 — when the retailer started to hire remote workers — and March 15, 2020.

Figure A.5: Decomposition of Difference-in-Differences Effect on Calls



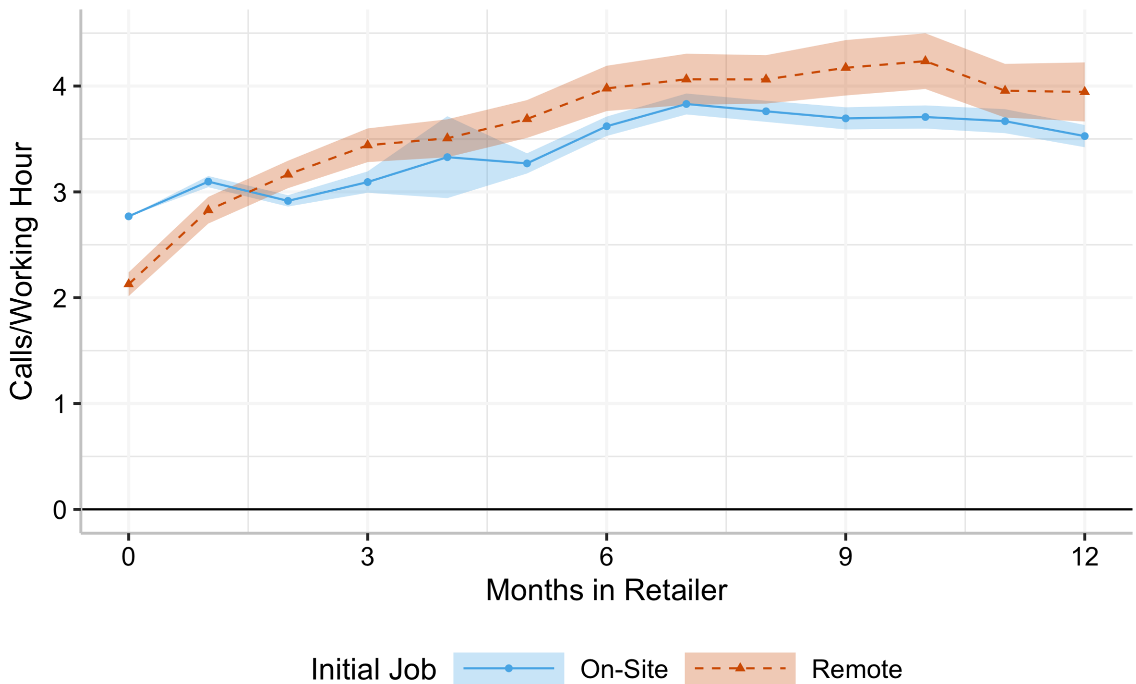
Note: This figure decomposes remote work's effect on calls per hour into (a) time spent on the phone and (b) call durations. In Panel (a), the percent of time on the phone is computed as the ratio of a worker's time on the phone to the time that she was scheduled to be taking calls. In Panel (b), the 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=1,623) 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 period from March 15 to April 6, 2020 and including our preferred set of controls for demographics, call-queue fixed effects, and worker fixed effects. These coefficients are also reported in Table 3. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Figure A.6: Difference-in-Differences Around Covid-19 Office Closures by Job Level



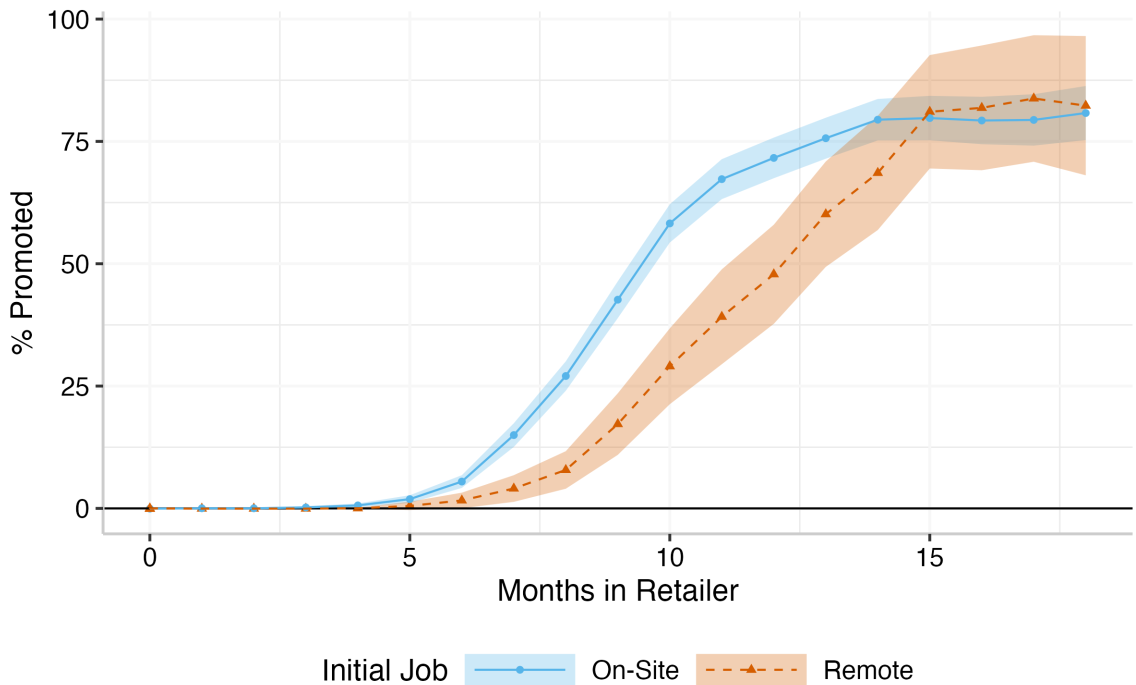
Note: This figure illustrates the differences-in-differences in calls taken per working hour between on-site worker who went remote during the Covid-19 office closures and workers who were already remote, based on their job level at the time of office closures. All estimates include our preferred set of controls for call queues and worker fixed effects (see Section II.C). Standard errors are clustered by worker.

Figure A.7: Pre-pandemic Calls per Working Hour Differences Conditional on Persisting in the Firm



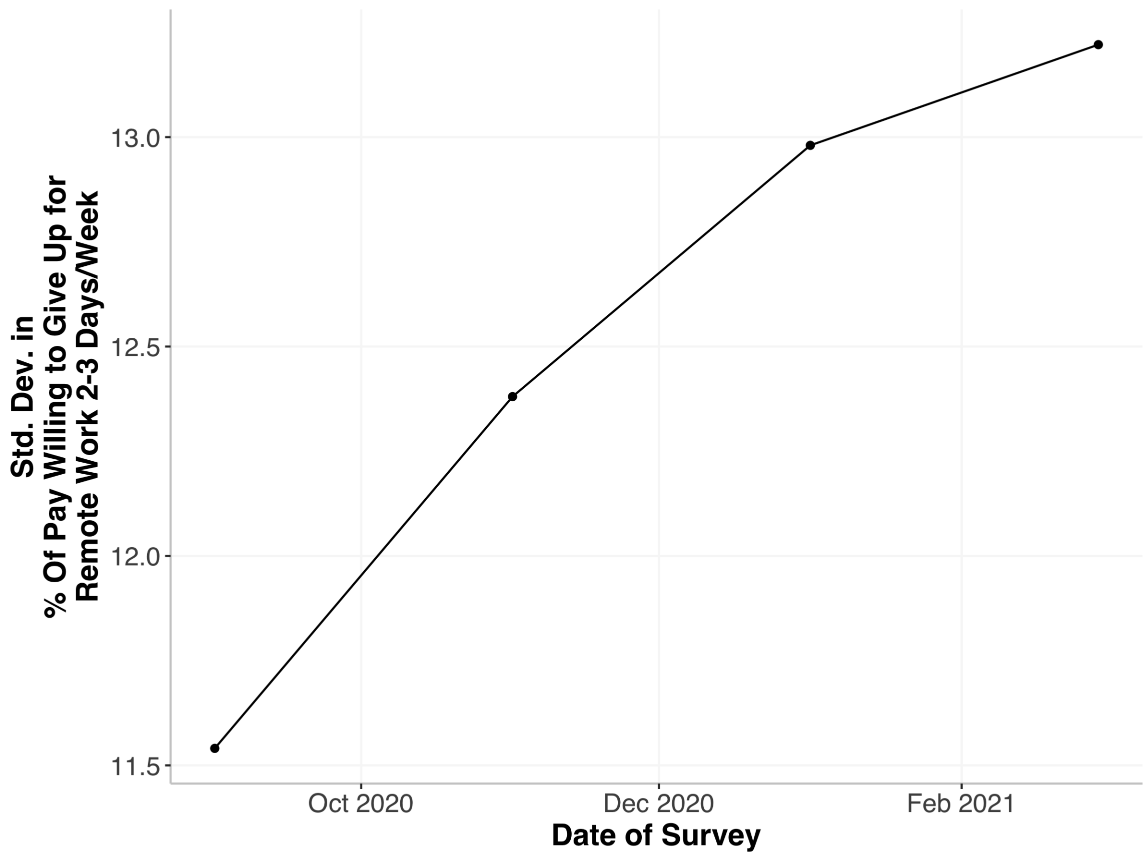
Note: This figure illustrates the differences in calls handled per working hour by on-site and remote workers conditional on persisting in the firm. The sample is limited to workers hired between July 2018 and March 15, 2019. Standard errors are clustered by worker.

Figure A.8: 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 2018 and March 15, 2020. Standard errors are clustered by worker.

Figure A.9: 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 CPS. For details on the survey design and reweighting, see Barrero et al. (2022).

B Appendix Tables

Table B.1: Productivity Differences When All Workers are Remote Due to Covid-19 controlling for date, time-zone, and experience

	Calls/Hour				
	(1)	(2)	(3)	(4)	(5)
Chose Remote Job	-0.311^{***}	-0.345^{***}	-0.316^{***}	-0.401^{***}	-0.381^{***}
	(0.060)	(0.082)	(0.119)	(0.063)	(0.088)
MSA CSR Wage		-0.007	-0.009		
		(0.017)	(0.025)		
Unemployment Rate		0.011	0.002		
		(0.017)	(0.026)		
Mother			0.046		0.036
			(0.054)		(0.077)
Father			-0.0001		0.059
			(0.098)		(0.135)
% Difference	-10.08%	-11.20%	-9.93%	NA%	-11.81%
	(1.96)	(2.67)	(3.75)	(NA)	(2.73)
Dependent Mean	3.08	3.08	3.18	NA	3.23
Dependent Std. Dev.	1.54	1.54	1.55	NA	NA
Propensity Weights				✓	✓
# Workers	3873	3873	1001	3873	1001
# Remote Workers	478	478	146	478	146
# On-site Workers	3395	3395	855	3395	855
# Days	256309	256309	74868	256309	74868

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 after the offices closed. Each specification estimates Equation 4, with additional controls for the date, the time-zone in which they work, and the individual worker's experience. 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. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.2: Adjacent Occupations for Customer Service Workers

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 transitions between occupations for customer services workers based on Current Population Survey respondents' occupations in the prior year. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.3: Difference-in-Difference Around Covid-19 Lockdown without Donut around Closure Period

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x After Office Closure	-0.177^{***}	-0.123^{*}	-0.142^{*}	-0.139^{**}	-0.116^{**}	-0.150^{**}
	(0.063)	(0.064)	(0.076)	(0.055)	(0.054)	(0.072)
Initially On-Site	0.394^{***}	0.450^{***}	0.454^{***}			
	(0.063)	(0.065)	(0.081)			
After Office Closure	0.624^{***}					
	(0.057)	(0.000)				
County Covid Cases/10K					-0.020	-0.007
					(0.019)	(0.025)
County Covid Deaths/100K					-0.067	-0.040
					(0.060)	(0.075)
Mother x After Office Closure						-0.053
						(0.063)
Father x After Office Closure						0.006
						(0.149)
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%	-3.05%	-3.85%
	(1.65)	(1.69)	(1.99)	(1.44)	(1.43)	(1.86)
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	115,053

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 lockdown 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. 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 four and five come from NYT (2021). Parenting characteristics in the fifth column come from a caregiving survey that the retailer fielded in June of 2020. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.4: 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 presents a difference-in-difference design that compares the change in productivity of on-site workers who went remote during the Covid-19 lockdown 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. The post period is defined as starting on April 6, 2020. The shaded region in Figure A.4 where it is unclear where on-site workers may be is excluded. The queue fixed effects specify the date, time-zone, and call-type (see Section II.C). Covid-19 cases and deaths in columns four and five come from NYT (2021). Parenting characteristics in the fifth column come from a caregiving survey that the retailer fielded in June of 2020. 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-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 lockdown 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, using our preferred set of controls (see Section II.C). The post period is defined as starting on April 6, 2020. The shaded region in Figure A.4 where it is unclear where on-site workers may be is excluded. 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 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 lockdown 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, using our preferred set of controls (see Section II.C). The post period is defined as starting on April 6, 2020. The shaded region in Figure A.4 where it is unclear where on-site workers may be is excluded. Covid-19 cases and deaths in columns four and five come from NYT (2021). Controls for MSA-level customer service and share of the workforce in customer service are from the American Community Survey. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.7: Heterogeneity in Difference-in-Differences in Call Volume around COVID-19 Office Closures

	Calls per Hour			
	(1)	(2)	(3)	(4)
Initially On-Site x Post	-0.15^{**}	-0.12^{*}	-0.14	-0.13
	(0.06)	(0.07)	(0.10)	(0.09)
Initially On-Site x Post x Female		-0.03		
		(0.05)		
Initially On-Site x Post x Caregiver			-0.03	
			(0.07)	
Initially On-Site x Post x Mother				-0.06
				(0.07)
Initially On-Site x Post x Father				-0.01
				(0.15)
Preferred Controls	✓	✓	✓	✓
Pre Mean On-Site, Control		3.7	3.8	3.9
Pre Mean On-Site, Focal Group	3.8	3.8	4.0	3.9
# Control Workers		534	270	361
# Focal Workers	1,436	1,431	457	357
R^2	0.44	0.44	0.45	0.45

Notes: This table present difference-in-differences designs that compare the change in calls answered of on-site workers who went remote during the COVID-19 lockdown to that of already remote workers, interacted with female, caregiver status, and tenure of the worker. Each specification estimates Equation 4, with our preferred set of controls for demographics, call-queue fixed effects and worker fixed effects. 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. Caregiver status comes a survey that the retailer 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.8: Heterogeneity in Difference-in-Differences in Call Quality around COVID-19 Office Closures

	Hold Min./Call		% Call Back (2 Day)		Satisfaction Rating		Call Without Call Back/Hour	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Initially On-Site x Post	0.61	0.28	0.04	0.08	−0.01	0.005	−0.19	−0.11
	(0.60)	(0.29)	(0.19)	(0.08)	(0.02)	(0.02)	(0.14)	(0.08)
Initially On-Site x Post x Female	−0.24		0.10		0.01		0.08	
	(0.62)		(0.19)		(0.02)		(0.15)	
Initially On-Site x Post x Mother		0.39*		0.10		0.01		−0.07
		(0.21)		(0.07)		(0.01)		(0.06)
Initially On-Site x Post x Father		−0.33		−0.02		0.01		0.01
		(0.44)		(0.14)		(0.02)		(0.13)
Preferred Controls	✓	✓	✓	✓	✓	✓	✓	✓
Pre Mean On-Site, Control	3.7	3.9	3.7	3.9	3.7	3.9	3.7	3.9
Pre Mean On-Site, Focal Group	3.8	3.9	3.8	3.9	3.8	3.9	3.8	3.9
# Control Workers	534	361	534	361	534	361	534	361
# Focal Workers	1,431	357	1,431	357	1,431	357	1,431	357
R^2	0.13	0.16	0.18	0.24	0.09	0.12	0.42	0.43

Notes: This table present difference-in-differences designs that compare the change in productivity metrics of on-site workers who went remote during the COVID-19 lockdown to that of already remote workers, interacted with female and tenure of the worker. The first two columns compare the minutes a customer was kept waiting on hold. The second two columns consider the rate at which customers call back to the service line within two days, likely with unanswered questions. The next two columns consider the average customer satisfaction scores on a five-point scale. Finally, we consider an alternative measure of productivity that reflects the number of customer calls that do not lead to a call back that the worker answers each hour. Each specification estimates Equation 4, with our preferred set of controls for demographics, call-queue fixed effects and worker fixed effects. 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. 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.9: Difference-in-Difference Around Covid-19 Lockdown for Entry-Level Workers

	Calls per Hour					
	(1)	(2)	(3)	(4)	(5)	(6)
Initially On-Site x Post	−0.18	−0.04	−0.21	−0.14	−0.08	−0.27*
	(0.14)	(0.16)	(0.17)	(0.11)	(0.12)	(0.15)
Initially On-Site	0.53***	0.59***	0.64***			
	(0.07)	(0.07)	(0.09)			
Post	0.90***					
	(0.12)	(0.00)				
County Covid Cases/10K					0.05	0.03
					(0.04)	(0.05)
Mother x Post						−0.16
						(0.14)
Father x Post						0.40
						(0.37)
Pre Dependent Mean On-Site	3.9	3.9	3.9	3.9	3.9	3.9
Initially On-Site x Post in %	-4.6%	-1.1%	-5.3%	-3.6%	-2%	-6.9%
	(3.60)	(4.00)	(4.50)	(2.80)	(3.00)	(3.90)
Age x Gender x Post FE		✓	✓	✓	✓	✓
Call Queue FE			✓	✓	✓	✓
Worker FE				✓	✓	✓
# Workers	1,496	1,496	1,496	1,496	1,496	553
# Initially On-site	1,230	1,230	1,230	1,230	1,230	443
# Already Remote	266	266	266	266	266	110
# Worker Days	75,330	75,330	75,330	75,330	75,330	31,675
R^2	0.06	0.11	0.22	0.47	0.47	0.51

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 lockdown to that of already-remote workers. The sample is limited to entry-level workers who take routine customer calls (e.g., when will my order be delivered?). 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 queue fixed effects specify the date, time-zone, and call-type (see Section II.C). Covid-19 cases and deaths in columns four and five come from NYT (2021). Parenting characteristics in the fifth column come from a caregiving survey that the retailer fielded in June of 2020. Standard errors are clustered by worker. ***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

Table B.10: 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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.11: Productivity Differences When All Workers are Remote Due to Covid-19 with Schedule Controls

	Calls per Hour				
	(1)	(2)	(3)	(4)	(5)
Remote Hire	-0.297^{***}	-0.288^{***}	-0.285^{***}	-0.285^{***}	-0.283^{***}
	(0.080)	(0.084)	(0.084)	(0.084)	(0.083)
Remote Hire 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	0.129	0.147	0.156	0.156	0.167

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., 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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.12: Productivity Differences When All Workers are Remote Due to Covid-19 with Geographic Controls

	Calls per Hour			
	(1)	(2)	(3)	(4)
Remote Hire	-0.297^{***}	-0.308^{***}	-0.260^{***}	-0.184^{*}
	(0.080)	(0.079)	(0.088)	(0.103)
Covid-19 Cases/10K		0.008	0.019	0.023
		(0.020)	(0.019)	(0.019)
Covid-19 Deaths/100K		0.068	0.051	0.096
		(0.085)	(0.086)	(0.086)
% In Customer Service			-0.070	-0.101^{*}
			(0.058)	(0.060)
% Unemployed				-0.026
				(0.017)
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	0.129	0.129	0.129	0.130

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., 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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.13: Customer Service 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 service satisfaction scores 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. 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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.14: 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 in the six months after the offices closed. Each specification estimates Equation 4. 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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.15: 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 in the six months after the offices closed. Each specification estimates Equation 4. 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer 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.16: 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	-0.51^{***}	-0.36^{**}	-0.17	-0.42^{**}	-0.27^{**}	-0.12
	(0.20)	(0.16)	(0.11)	(0.17)	(0.14)	(0.10)
Initially Remote x Female	0.28			0.24		
	(0.20)			(0.17)		
Initially Remote x Caregiver		0.10			0.05	
		(0.17)			(0.15)	
Initially Remote x Low Tenure			-0.22			-0.20^{*}
			(0.14)			(0.12)
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	363	215	498	363	215	498
# Focal Workers	811	324	676	811	324	676
R^2	0.12	0.16	0.13	0.12	0.16	0.13

Notes: This table considers differences in calls handled per work hour and calls answered without a call back within two days per working hour, by different demographic group. Each specification estimates Equation 4. 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, 2020b). Parenting characteristics come from a caregiving survey that the retailer fielded in June of 2020. Low tenure is defined has having worked at the firm for less than 6 months. 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.